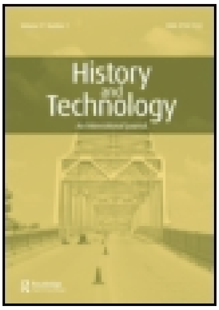


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# Technology-Led Innovation: The Non-Linearity of US Jet Propulsion Development

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# Technology-Led Innovation: The Non-Linearity of US Jet Propulsion Development

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*Despite repeated announcements of its death or uselessness, the 'linear model' of science–technology relations persists, the notion that fundamental scientific research precedes applied studies that in time generate technological advances. This article undertakes first to revisit investigations and critiques of the model, and to remind historians of technology that intriguing alternatives to it have been developed. Second, using the case study of Cold War military jet propulsion, it argues that innovative, complex technologies have been created without reliable understanding of scientific fundamentals. These were messy, non-linear, and failure-filled processes, to be sure, yet they may well prove to have been more the rule than the exception, once scholars pursue richly textured studies of technical practice in experimental development. Ultimately, project needs to overcome engineering obstacles in technological innovation may provide the platforms and the funding to support basic scientific research as well, reversing the linear model's expected flows.*

*Keywords:* *Cold War; Innovation; Non-Linearity; Jet Propulsion*

> We have paid particular attention to the relation of science to innovation. To our minds, our failure to find more than a small handful of direct connections is the more striking for the fact that we set out deliberately to look for them … . Perhaps science is not the father of technology but an anonymous well-wisher who sends it gifts through the post, as it were.[1]

> Entrepreneurs have long accepted that innovation is anything but orderly and simple, and scholars are beginning to understand this too. Governments, however, still tend to view innovation as a pipeline. If public money is stuffed into basic research in universities and national laboratories at one end, they reckon, new technology and commercial applications should pop out of the other.[2]

'The "linear model" of the process of innovation, established in the course of industrialization and dominant in perceptions of the relation between science, technology

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and the job market for almost a century, has entered a state of crisis.'[3] So reads a portion of a recent scholarly conference's call for papers. That same month, a colleague of mine dismissed out-of-hand any notion of basic science's productive linkages to technological innovation,[4] but others may remain agnostic, seeking more illumination of the matter. Meanwhile, the general public, some business folks (if *Business Week* or *The Economist* are any barometers), and science advocates evidently have just begun to absorb two generations of critique by historians of technology.[5]

This essay will attempt three things: (1) to review and assess a series of social scientific research projects that have evaluated the mechanisms delivering technological innovation, including the 'linear model'; (2) to outline the dynamics of jet propulsion's early development in the US; and (3) to analyze the set of key performance challenges gas turbine technology had to overcome and their connections to postwar scientific knowledge. In concluding, I will suggest the jet propulsion case assists us in thinking about technological innovation as creating platforms for framing and pursuing questions that range beyond the boundaries of reliable, basic scientific understanding. In so doing, I will stress the value of regarding innovation as a open, iterative, situated process of problem-stating and solving, often with multiple feedback loops that shift the grounds of inquiry, rather than as a series of discrete events. Prospectively, this work also signals my hope that a renewed emphasis on exploring innovation will also generate initiatives among historians of technology for in-depth studies of design and production, no longer simply internalist, but richly contextualized amid organizations, politics and cultures. Recognizing how provocative and productive the cultural turn in technological studies has proven to be, we may still profit from returning to questions of materiality, power, institutions and innovation.

## Considering Science–Technology Connections

The linear model of innovation, in which basic science spawns technical applications of value to enterprises and societies, was ably promoted by presidential adviser, businessman, professor and physicist/electrical engineer Vannevar Bush. Having headed the wartime Office of Scientific Research and Development, Bush proposed in 1945 a National Research Foundation, managed by civilians, funded by government, and supporting inquiries on the frontiers of knowledge. This prospect brought into 20th century focus a comparable 17th century plan authored by Francis Bacon. In *Novum Organum* (1620) and *Sylva Sylvarum* (1627), Bacon called for 'the State to fund the building of university laboratories,' which would have 'no limits on research' and to sponsor 'traveling fellowships' so English scholars could exchange 'intelligence' with their continental colleagues. Bacon argued that 'pure science underpinned applied science or technology' and that the improvement of technology 'created wealth.'[6]

Bacon was generally ignored, but Bush, despite his arrogance and political blundering, eventually succeeded—the third in a line of American science advocates whose projects intersected with major wars.[7] A 'Yankee Republican' and a devout elitist, Bush 'highlighted the tie between research and prosperity in nearly the same simplistic terms as the senator [with whom he tangled over initiating what became the National Science Foundation]. "More and better scientific research is essential to the achievement of our goal of full employment", he wrote in *Science—The Endless Frontier*.'[8] Beyond signaling employment concerns that represented a legacy of the 1930s, Bush anticipated the technological demands that postwar military superiority would entail. As his biographer explained:

> The nation needed a strong scientific base in order to remain the world's top military power. Research was 'absolutely essential to national security' and was too important to entrust to the military, Bush concluded. 'Modern war requires the use of the most advanced scientific techniques … [so] a professional partnership between the officers in the Services and civilian scientists is needed.'[9]

Such a partnership did develop, but hardly along the lines Bush had anticipated. Congress took five years to authorize the National Science Foundation, and by that time pressures for Cold War military innovations had become intense. By 1950 (and for decades thereafter) the Army, Navy and Air Force independently sponsored and funded weapons and systems development projects that soon dwarfed the spending on Bush's Manhattan Project (US$1.5–2.0 billion). The military certainly relied on civilian expertise, but far more on corporate engineering than on university scientists or private sector industrial research labs. Moreover, the armed forces developed their own scientific expertise, staffs and sites, ranging from the venerable National Advisory Committee on Aeronautics' (NACA) Langley lab and the Army's Redstone missile arsenal at Huntsville, Alabama, led by Werner von Braun, to biochemical warfare researchers at Ft Dietrick, Maryland. Rather than Bacon-like university or industrial centers shipping new scientific principles to the military for transformation into weapons applications for manufacturers to fabricate, a variety of trajectories and networks emerged, grounded in an urgency that assured ample funding, a focus on mission-targeted experimentation, and a sustained emphasis on refining reliable understandings and generating new knowledge.

Even so, the linear model survived, at least in public discourse. In 1951, retired Admiral L. B. Richardson, then director of R&D at Fairchild Engine and Airplane Co. and president of the Institute of Aeronautical Sciences, made it clear. In World War II, he said, 'Fundamental aeronautical research was almost abandoned,' in favor of immediate problem solving by researchers at the NACA experiment sites. This 'technical hand-to-mouth existence' ended in peacetime, because in Richardson's view 'Aeronautical development … can move ahead only [by following] fundamental aeronautical research.' Yet now that the nation was at war in Korea:

> We already see evidences of a resumption of the old practice of using NACA research facilities for specific developments and for trouble-shooting to solve problems arising in the current [air power] expansion. This should not be allowed to grow into a regular procedure! We must not undermine the research foundation on which our future technical progress will be built.[10]

More than a decade later, the US House of Representatives' Appropriations Committee held hearings to explore the effectiveness of research spending by the military, which produced the usual anecdotal documentation of waste, error and failure mixed with modest and spectacular successes.[11] This probe accelerated Project Hindsight, a 'retrospective' study begun in 1964 and intended to analyze 'the recent science and technology which has been utilized by the Department of Defense in weapons systems.' Hindsight's two goals were to 'identify and firmly establish management factors' promoting successful utilization of research and to 'measure the overall increase in cost-effectiveness' attributable to research and technology that current weapons provided over their predecessors.[12] Building on an earlier Arthur D. Little study (1963–1964) that defined an 'R&D event' as the unit of analysis for research into innovation, the DOD team invested an estimated 40 man-years over the next 30 months developing detailed science/technology histories for some 20 weapons systems (six initiated in the Little project), including transport aircraft, naval mines, nuclear warheads and the Apollo space capsule.[13] Though funding for the project ended before its work was completed (perhaps in part because of the controversies initial findings sparked), Hindsight's core conclusions were striking then and remain so:

(1) '[E]ngineering design of military weapons systems primarily consists of skillfully selecting and integrating a large number of innovations so as to produce, by synergistic effects, the high performance demanded.'
(2) Nearly 40% of innovative events occurred after the weapon design was begun and 'were necessary to the ultimate performance of the system.'
(3) The relative efficiency of producing innovative events for defense purposes was ten times higher ('one order of magnitude') when the process was 'funded and managed by the Defense Department or defense industry,' rather than when handled by 'the non-defense sector of government or industry.'
(4) '[T]he contributions from recent (post 1945) research in science were greatest when the effort was oriented,' that is, mission-centered rather than 'undirected' research, the feature of basic science quests, which Hindsight observed 'paid off on the 30 to 60 year or more time scale.'[14]

High-performance weapons resulted from complex dynamics, which involved substantial innovation after projects began, and which worked best when 'funded and managed' by agencies and enterprises long familiar with weapons design and production. Yet the report's characterization of basic science as peripheral to meeting defense needs, except in the very long term, created a minor firestorm.

The Interim Report's Figure 10, underplayed in the text, was hard for Bush–Bacon-type science advocates to swallow (and they fought back). Project directors Sherwin and Isenson indicated that *basic research* where 'the primary result is new knowledge' (the then-standard definition of pure, disinterested research, distinct from that oriented to projects and artifacts) generated a tiny fraction of the over 600 R&D innovation events Hindsight identified. Just two events (0.3%) flowed from 'undirected research' and eight others (1.5%) arose from applied scientific work at Bell Labs' transistor project.[15]

Among the responses this drew was a rapidly assembled National Science Foundation project, TRACES, awarded to the Illinois Institute of Technology, undertaking to show Hindsight's error by commissioning histories of five non-military technologies that arguably showed the linear model at work, technologies for which 'non-mission research' generated 70% of the 'key events' leading to development and commercial diffusion. Yet the peak period for reaching these basic science findings rested 20–30 years prior to the technological innovations at issue, which took more time to reach markets.[16] That accorded rather well with Hindsight's notion of a 30+ year lag between undirected research findings and potential defense systems payoffs.

A more substantial problem with the TRACES study was its immersion in a teleological framework (a 'path-to-the-present' model of history) and the entirely non-random choice of the five technologies to be researched. Nothing in the study report explained how that selection process was formulated, so it remains plausible that science-intensive technologies were chosen to show science-intensivity. More fundamentally, only if the research had proceeded *forward from a point in time toward utilization rather than backward from present outcomes* would there have been prospects for a suitable assessment of scientific knowledge's role in technological innovation. Though difficult to operationalize, determining what elements in scientists' understanding of the chemistry of metals, *ca* 1910–1920, proved foundational for technological innovations in alloys and metalworking over the next two generations would at least have started from a defined universe of information, looking ahead, and could have included dead-ends, failures, plus minor and major influences on innovative technologies. Hindsight was open to much the same criticisms; significantly, the teams for neither project included professional historians.[17]

Of course the current, politicized issue in the 1960s was whether the defense case from Hindsight could be generalized—whether the payoff from non-mission science was so narrow and slow that federal support for such activity represented a poor use of tax revenues. TRACES warred against that conclusion, for it threatened to dissolve the argument from utility (employment or national security) for federally-funded pure science.[18] Other attacks on Hindsight focused more on its methodology, arguing that the unit of analysis was artificial, that it lacked a control group (as did TRACES), that the classification of scientific and technological events was arbitrary, or that flawed selection of programs for evaluation (bad sampling) undervalued basic science's role.[19]

Still, Dudley Coillet's MIT study confirmed several valuable points: (1) basic science researchers had to develop project instrumentation, which became useful to engineering and technological work; (2) 30% of the postwar military innovation events related to new materials; and (3) 'the widely differing resource demand of the development phase [relative] to the research phase is one of the distinguishing features of engineering versus science.' Technological development hogged funds; ratios between scientific research and engineering were 'as high as 1:10.'[20] As David Edgerton explained: 'it may well be the case that the "fundamentalists" [basic science advocates] succeeded, relatively, in increasing the proportion of "fundamental" or "basic" research in total "research and development", but not that fundamental research ever dominated.'[21]

In parallel with these US initiatives, a group of British researchers secured Research Council funding to analyze much the same dynamic, the relationships between scientific research, technological innovation and economic growth (and the implicit corollary that, were these rational relationships, the innovation 'process is plannable by rational means'[22]). The Wealth From Knowledge group (WFK) dealt with the American works' shortcomings when designing its inquiry. They addressed selection bias of both Hindsight and TRACES by researching the histories of UK government prizewinners in technological innovation (the Queen's Awards for 1966 and 1967), thereby drawing on an independent review and assessment project. That there were 84 awardees (*vs* 20 cases for Hindsight and five for TRACES) also addressed the small numbers question. Most important, the WFK team conceptualized its work distinctively, asserting: 'Innovation is a process over time and not a point event ....' Therefore, WFK eschewed counting and weighting innovative moments, dropped the US projects' social scientific baggage, and relied on narrative and simple classification as key tools.[23] Second, WFK discarded unidirectionality: rather than looking for scientific influence or knowledge provision for technical innovations, they explored histories of innovations open to multiple lines of development. This merits a bit more explication.

In TRACES, the difference between non-mission (basic) research and mission-oriented research was that while the former was 'motivated' by curiosity 'without special regard for application,' the latter was 'performed to develop information for a specific application prior to development of a prototype product or engineering design.'[24] Yet, Hindsight had shown that nearly 40% of innovations arose after a design or prototype was commenced; TRACES did not account for this reverse flow, for the possibility that issues arising in creating a novel technology might generate a feedback demand for mission-oriented scientific research. Nor did TRACES conceive that this demand loop might spiral into the most basic of scientific realms—principles concerning the structure of matter, the dynamics of flows, or the characteristics of elements.

WFK recognized that a variety of patterns had occurred,[25] and equally important, in discussing feedback into science, highlighted one of the history of technology's founding claims:

> [T]here are many cases of 'negative time-lags,' industrial advances that come *before* the scientific advances that help to make them understandable. For instance it was not until after the first synthetic rubbers and plastics had been made that polymer science developed. The authors of the 'TRACES' report noted [in its conclusions] 'cases in which mission-oriented research or development efforts elicited later non-mission research which often was found to be crucial to the ultimate innovation.' This emphasizes the point that the relation between science and technology is a matter of *two-way interaction*, not of unilateral dependence.[26]

As Edgerton has put it so forcefully, the 'linear model' was always more a phantasm than a statement about practice. It derived chiefly from academic science advocates and science policy academics, particularly after 1980, for whom overstating Vannevar Bush's influence was convenient. In this view, gauging technological innovation as if it were a consequence of academic research, or of that carried out in major corporations' industrial research facilities, was an error compounded by inadequate historical work.

> Academic historians and other analysts have, by granting so much attention to policy for 'basic' or 'fundamental' research, reproduced the focus of the 'linear model' they criticize, and shared its assumptions that what really mattered was this high level stuff. The rest is

> merely derivative. The problem is that the great bulk of research and development (which is better described as development and research) was not 'basic' or 'fundamental' or thought of in terms of the linear model at all … .[27]

In this context, Stephen Kline's interventions in relation to conceptualizing the dynamics of technological innovation help outline the multiplicity of paths the WFK research identified, so as to grapple with 'the great bulk' of development and research instead of focusing chiefly on the 'high level stuff.'

Kline, a mechanical engineer reflecting on '30 years of consulting in the aircraft, automotive, paper, petroleum, power plant and other industries,' suggested that 'five pathways for innovation exist,' all of which are important. These included *market-finding initiatives* that pushed design and development,[28] along with *feedback effects*—those discovered as an item was prepared for market ('development far more often than not demands alterations in the original invention or analytic design') or 'aris[ing] from deficiencies in service,' at times pointing toward 'later models or new systems.'[29] Each and all could trigger product and process innovations. A third vector commenced with the *technologist facing a problem and undertaking a literature search* for knowledge bearing on the matter. When, as often, the literature proved insufficient, contradictory, or tangential, 'the questions that are thrown up by the processes of invention, analytic design, failure in testing, or difficulties in production in effect define research problems. These problems are by definition applied.'[30]

As well, on occasion there was a *direct link from basic research forward to development*, but this took a time-consuming detour through invention, for '[o]ne cannot develop what has not been designed and built.' Last, Kline posited an occasional *innovation-generating link between present products and perceptions/speculations about long-term needs*, a question familiar in military and electronics circles. Here assessments of existing technological artifacts could trigger funding for basic research that, in the best case, provided venues for invention and fabrication of new 'sociotechnical systems.'[31]

Non-linearity here stems from indeterminacy as to which vector for innovation, if any, might be operative within any technological network, system, or site, as the matrix of use/demand/production/assessment moves across time. Hence Kline argued that, in a complex and dynamic innovation environment, 'the distinction between [technology/science/discovery] pushes and [market/client] pulls loses essentially all meaning.' Neither large-scale planning nor massive investments in industrial laboratories could reliably generate innovations of technical, much less market significance.[32]

For Kline, rather, 'the first line of reference for innovation processes' in his non-linear model 'is not research but the *totality of cumulated human knowledge*.' When putting an innovation process into motion:

> Any modern technical person … will not turn first to research. On the contrary, one turns first to the current state of the art, then to personal knowledge about the governing principles of the field. After that, one goes to the literature, consults colleagues, calls in leading experts. Only when all that does not suffice does one start research, [which depends upon] our stock of tools, instruments, machines, and processes and … increasingly powerful sociotechnical systems.[33]

Promptly, Kline invited readers 'to consider an example—the jet engine. Its design and manufacture would be equally unthinkable without these powerful systems, including special manufacturing processes, advanced materials, and skilled, cooperating workmen.'[34]

The remainder of this essay will do just this—consider the jet engine innovation process—finding Kline's 'powerful systems' lurching into crises while fabricating unreliable, complex technologies in circumstances of unstable demand and escalating expectations. With the Cold War intensifying, the non-linear and non-rational characteristics of America's military jet engine project illuminate the sharp, contingent edges of technological innovation and suggest that 'sociotechnical systems' are not terribly systematic *when dealing with sustained uncertainties*. Even Kline's elaborate multi-directional innovation process inadequately grasps the persistent chaos and failure, confusion and error that jet propulsion development spawned.

## Fabricating Jet Propulsion Technologies

A broad claim to start: the transition from propeller-driven to jet powered aircraft revolutionized military and civilian relationships above ground in much the same way that the internal combustion engine's triumph revolutionized military and civilian relationships with the land, but did so more rapidly.[35] Automobility, roughly from 1900 to 1940, reduced rural isolation, created new patterns for journeys-to-work, enabled novel forms of leisure, sparked myriad new businesses in service and repair, stimulated the search for essential materials and fuels, and initiated new cultural forms—from highway billboards to courting practices. It also transformed war, but slowly; even though tanks, tractors and trucks became crucial to strategic maneuver in World War II, a major segment of Nazi-era agriculture grew and harvested fodder for the hordes of horses providing Werhmacht mobility.[36]

By contrast, the reciprocating aircraft engine powered all but two operational airplane types[37] from the Wright Brothers through 1945, then jet engines displaced it within a decade for most Cold War military uses and steadily thereafter for most civilian and high-value freight transport. Of course, the piston-to-jet transition in the US, UK and USSR differed profoundly from the technological innovation dynamic which yielded the Otto/Benz engine and its successors (*ca* 1900) in at least three ways: (1) it was state sponsored for military use; (2) it cost staggering sums of government funds; and (3) it was secret, its details and practices classified, even as it was 'public' in Cold War propaganda and as its artifacts roared overhead.[38]

Yet the US move to jet propulsion itself had to be propelled by General 'Hap' Arnold's urgent wartime enthusiasm, requesting/demanding from an embattled Great Britain plans, a license and an early Whittle engine.[39] Arnold and a cluster of Army Air Force materiel and engineering planners sensed that fighter aircraft powered by reliable jet engines could achieve tactical air superiority, given their potential for reaching higher speeds and altitudes (and at higher rates of climb) than advanced prop engines.

Persuading others was another matter, however. In June 1944, shortly after D-Day, Colonel R. C. Wilson wrote to Material Command, sketching the case its lead General could make for jets. Yes, he noted, piston engines had several advantages: high wartime rates of production, durability in use, low maintenance costs, low fuel consumption and flexibility in operation (an engine for every use). However, reciprocating engines had reached their technical peak both in range and performance. To go higher or farther, new versions (over 3000 hp, 35+ cylinders) would become so heavy and complex that just lifting them would devour the added power. Maintenance costs and headaches would multiply. Jets, by contrast, had many advantages, or at least so Wilson argued. Their simpler design facilitated ease of operation and maintenance. They would be cheap to manufacture, and a high production rate was feasible. They were quieter, vibrated less, and could be run on less volatile fuel (kerosene instead of gasoline), but the test models still had one drawback: high fuel consumption.[40]

That defect, however, was potentially disastrous, for if more powerful, high altitude props were undermined by excess weight, jets could fail in performance through having to lift great volumes of fuel and through the reduced range that rapid fuel use entailed. Hence the most critical challenge of fabricating jet technologies was reducing fuel consumption. Second, virtually all the jet advantages Wilson cited were prospective, and achieving them proved a tortuous process. As Mundy Peale, president of Republic Aviation, explained:

> all of us were so new in the jet fighter business that we didn't know what we were going to run into. Lots of the things we thought were going to be true just because of our past experience [with pistons and propellers] were so brand new that they weren't true at all—so *we had to learn* to build a plane that was better to maintain and would be as good in service as [prop-driven fighters].[41]

The jet engine's schematic design may have been simple, but its detail designs involved thousands of parts, many placed under extremes of temperature and stress. Jet engine failures, unlike props, had a strong tendency to be catastrophic, as metal fragments whirled through the device at 8000 rpm, scything through other components. When manufactured with precision, jets ran with a hum and howled almost sweetly when accelerating, but minor errors in tolerances, bearings, or fittings could set off efficiency-reducing vibrations that threatened failure. Jets did *not* prove cheap to manufacture; indeed, the waves of component deficiencies and functional failures that coursed through fabrication plants, test beds and flight-trial facilities fed back so many hundreds and thousands of engineering changes, the chance that they would *ever* be cheap to manufacture seemed to recede steadily. Last, something Wilson ignored: prop engines' durability for thousands of hours in flight had long been established,[42] but jet builders found, even before the close of the War, that their novel propulsion units failed routinely after running for 50 or 100 hours. Still, jet builders were not fazed. North American Aviation president J. L. (Lee) Atwood put it this way:

> [Let's say] you make a uniform. There is no sense in raising the quality after you have reached a certain point. It would be uneconomical and foolish. … However, in airplanes we have no limit to the quality objective from a performance point of view. We do not know what the boys are going to have to compete with when they go into battle and we are very apprehensive at all times [that] they will come up against superior enemy aircraft. … Now *improvement in quality means continuous innovations. That means experiment and*

> *experiment means cut and try*. It means rejection of ideas as well as acceptance and incorporation of these new ideas.[43]

Fabricating jets stretched technology's edges; it took place in a risky, constant-learning environment—urgent, hazardous, yet well funded. Even before the war ended, the AAF had commissioned 15 separate jet propulsion projects (including turboprops[44]), taking a profusion approach to developing jet engines for multiple uses. Thus major aircraft and engine corporations volunteered or were drafted into the jet stream: Lockheed, Rocketdyne (later Northrop), Westinghouse, Pratt & Whitney and most dramatically, General Electric, responsible for nearly half the wartime projects.[45] Early builders experienced countless troubles making the new machines function. Moreover, initial models' disappointing operations and combat planes' increasing complexity and rising weight led the Air Force to call repeatedly for improved engine performance (higher thrust, better acceleration), increased durability, better reliability, easier maintenance and other advances. Not only was the jet engine hard to build and hard to keep running, it would not sit still as a technological artifact—thus standardized, cheaper 'quantity production' remained elusive into the 1950s.[46]

To appreciate the technological difficulties attending jet propulsion's development, it may help to encounter a specific engine and learn how it worked. First, we can peer into a bench testing area where General Electric fired up J-47s, one of the workhorses of the Cold War (Figure 1). When being constructed, simple wheeled trucks carried engines-in-progress for the sequential addition of components, not unlike those in the early Highland Park Model-T assembly process, before the standardization of design and integration of subassembly lines that yielded Ford's 'chain system.'[47] Here technicians are preparing an engine for ground running while bolted securely to a steel and concrete platform. In this photo, the engine's air intake is out of sight at the right end of the J-47, and the compressor is underneath the array of electrical equipment, also at the right. The circle of combustion chambers is readily visible at the center. The standing technician is worked just at the point where the turbine wheel is located and behind him (labeled '10') is the afterburner section, into which extra fuel could be channeled for a short-term thrust increase.[48]

The airflow through the engine moves right to left. Incoming air rushes through guide vanes and into the concealed compressor. The compressor holds multiple circles of rotors inside its casing and attached to the engine's central shaft, both bristling with blades. The shaft turns at 6–10,000 rpm, both pulling the air flow through and squashing it rapidly through ever smaller passages, from whence it bursts into the combustion chambers, is blast-mixed with fuel, burns continuously and expands powerfully through the turbine assembly (where the worker stands at the left). Turbine blades capture a portion of this energy to spin the central shaft which continuously compresses the incoming air. The rest of the expanding gases rush out the exhaust, driving the aircraft forward in reaction (as in classical mechanics).

The J-47 was a simple concept—essentially a shell with one big moving part inside, the central shaft on which the turbine and compressor were mounted. Yet it was an immensely complex device, comprising over 8000 parts whose faultless interlacing generated the engine's capacity to function. Internal combustion engines, in land or

**Figure 1** Arnold Engineering Development Center testing the General Electric (GE) J47-GE-23 Engine on the New Engine Test Facility (ETF) Run-up Stand, 27 August 1953. (Source: AEDC press release, 2 September 2003, public domain)

air, could operate despite damage that deadened one or more cylinders. Not jets: they are either on or off, and aloft, off is very, very bad. Discovering those circumstances that could impair their effectiveness or shut them down represented a core problem set for engine builders once first phase development (just getting engines to work at all) had been managed. As the technical editor of *Flight*, a key British aeronautical journal, explained in 1951, while reviewing early UK jet engines (which set the standard for performance):

> Although in the early phases of gas-turbine development the facile simplicity of the new form of power unit was extremely attractive, it soon became apparent that considerable problems were involved and, moreover, were rendered none the more soluble by being, in general, of a kind about which few data existed. Pioneer work was called for in most fields, and today the research effort still continues undiminished.[49]

The J-47 was a significant engine as well. Thoroughly atypical for early American jet propulsion, it was US-designed and produced in the thousands, whereas dozens of postwar engine projects failed entirely, yielded underpowered or unreliable turbines, or generated effective engines made in modest quantities for planes soon obsolete. Eventually installed in thousands of aircraft, the J-47 nonetheless triggered conflicts between builders and users, and not just in its early stages of development.[50] Examining the J-47's course will enable us to review this process of design, fabrication, testing, failure and redesign.

## Creating the J-47s[51]

Immediately after Japan's August 1945 surrender, the Department of War commenced canceling many military hardware procurement contracts and sought renegotiation or downsizing of others. This effort upset the AAF's plans.

> All work on the jet engine program stopped, and the Air Force saw its future as a leader in jet aircraft threatened; the General Electric Company had done so much of the pioneer work in jet engines that without its continued research, the progress of jet engine development in the United States would receive a decided setback.[52]

Scrambling to rescue and fund the jet project, on 15 October Wright Field's Deputy Chief of Procurement, Colonel H. A. Shepard telephoned Major General Edward Powers, AAF Deputy Chief of Staff.

> Shepard: Now, it's boiled down roughly to this, that out of the $127,000,000 we had set up to do the entire job [GE jet work, 1945], the price redetermination procedure has revealed that we have savings of some $14 million on [canceled] production contracts … and we still have some more experimental work to do … unless we can do this [shift the US $14 million to experimental work] our [jet] program will flop … *we know that considerable research work must be done if we are going to get flyable engines out of General Electric*.
>
> Powers: OK, then we'll do that … What is the score on your facilities … on your discussion with … Chevrolet on the continuing production of the TG-180? … I've refused to OK it because of the general overall design—that engine belongs to GE and without their consent we'll have difficulty having anybody else manufacture it.
>
> Shepard: Yes, sir. Well, we've got quite a problem in this [thing.] GE is trying to show that it would cost no more and probably less money to consolidate all of the facilities expansion in one place. …
>
> Powers: I think that would probably be a smart thing to do, because *I don't think we can count on Chevrolet for any development. … They're getting back into the automobile business*.[53]

The salvaged funds, the consolidation of experimental development and research work at GE, and the General's recognition that automobile corporations that built aircraft engines during the war largely planned to drop the business[54] combined to fund initial design work on the TG-190 (J-47). It was to have the same exterior dimensions as the TG-180 (J-35) General Powers mentioned, but would deliver 30% more thrust while achieving lower fuel consumption.[55]

On 4 March 1946, J. C. Buechel of GE's Lynn, MA Aviation Division outlined the firm's jet engine 'development proposal,' the third phase of which was to become the J-47, an engine that would 'give the maximum possible thrust with the lowest possible specific fuel consumption.' Buechel explained:

> Analytical studies are in progress which are necessary before compressor, combustion chamber, and turbine designs can be made. As soon as these studies have progressed far enough to supply the necessary information, design studies will be made of the new model. … Because of the basic research involved in this model, it is impossible to state when the first engine will be completed. It is estimated, however, that the first unit of this design will be *ready for test* in approximately two to three years.[56]

What Beuchel here termed 'basic research' referred to what his Aviation Division colleague R. E. Small described as 'extrapolated performance estimates' (in a mid-May memo to airframe builders regarding the J-47's characteristics). These estimates were based on empirical data from the predecessor TG-180/J-35 engine's operations, but they were not simple calculations. Small warned his clients not to undertake them on their own, giving the first indication that scaling engineering characteristics between engines designed for different thrusts might not be a linear proposition.[57] GE's 'basic' research mattered little, though, for it informed an engine design and production process that could not, would not, did not work as planned.

Materiel Command's discovery of US$25 million in unallocated 1946 Air Force funds set big wheels in motion. Rather than starting with the 'usual procedure,' ordering 'a few engines for developmental, experimental, and test purposes,' Colonel Shepard persuaded his superiors to draft a production contract for 432 J-47s, in part to shake loose funds to assist GE with facilities expansion work (US$3–4 million). After a blizzard of revisions and supplemental agreements, the parties finalized a comprehensive contract on 29 January 1948 (US$102.5 million for 1694 J-47s plus spare parts and 'overhaul tools').[58] That the AAF confirmed this agreement suggests the urgency of jet projects. Even though only eight of the first 20 engines ordered in June 1946 had reached testing by end of 1947, airframe redesign and production was moving ahead steadily. Lockheed, Douglas and the rest may have wanted the higher thrust J-47s as fast as possible, but they would have to wait.[59]

Testing and use of the initial engines revealed four major deficiencies; fixing them generated a fifth sizable problem, more delays. Jet turbines operated by means of tough alloy 'buckets' which caught some of the combustion blast energy and spun the engine's central shaft, driving its compressor.[60] In J-47s, these buckets began cracking and a redesign to strengthen them 'delayed production by at least nine months.' Then they cracked again in different places. Thus in June 1949, 'it was necessary … to ground all B-45 and F-86 aircraft powered by J-47-7, -9, -13, and -15 engines above a certain serial number.' In time, a fix was devised, but defective turbine buckets and compressor blades would remain an issue.[61]

Second, magnesium had initially been specified for a number of engine components, including the compressor wheel, 'to save weight and aluminum,' but this had proven a mistake. Midway through 1948, the Air Force determined to make these parts of aluminum, which added '50 pounds to the basic weight of the engine' and mandated 'complete rebuilding of the first 200 [J-47s].' Third, the engine's fuel regulation controls frequently failed to work properly, especially in 'low temperature conditions' experienced at high altitudes. GE redesign efforts proved fruitless, so a 'temporary fix,' an 'emergency control' was installed in the same first 200, until GE could get it right. None of its substitutes 'proved entirely satisfactory,' however, and the Air Force again grounded planes after a series of failures in fall 1949. 'Inspection revealed that poor workmanship was the cause of the regulator's improper operation,'[62] the military complained.

By far the worst problem, though, was the repeated failure of the air seal at the rear of the compressor, a fixture that guaranteed the high-speed airflow would roar into the combustors without disrupted fluid dynamics or pressure loss. In consequence, seal blowouts 'caus[ed] extremely hazardous flight condition[s].' By October 1949, this component had received over 40 'unsatisfactory reports'; then in one week B-47 pilots reported two 'total failures' aloft. On 21 October 1949, the Air Force grounded 250 B-47s and F-86s, 'practically all aircraft powered by the J-47 engine.' GE quickly generated a new version, designed in a 'new material' and ready for production by August 1950 and retrofitted it into 'approximately 1000' J-47s, each job demanding a full engine 'tear-down.'[63]

If early in 1948 GE had delivered just eight engines for testing, how did the USAF wind up with well over a thousand faulty engines installed in grounded planes by fall 1949? Institutional desperation is the broadest answer, but the protocol in force was more politely termed 'concurrency.' As in World War II, the Air Force contracted for production of planes and engines at rates based on an assumption of design freezes, the arresting of engineering changes so that quantity production could be geared up. Yet for piston and jet engines alike, military use fed back a stream of evidence identifying design deficiencies. Thus the wartime practice of 'fly and fix' settled into postwar 'concurrency,' continuing production of unreliable engines regularly redesigned to overcome failing components or unsatisfactory performance discovered in test and use. This mandated disassembling early models, retrofitting them with improved parts, retesting and reinstalling them, in many cases several times.[64]

Indeed, in 1947 the J-47 twice failed its initial model tests (150 hours bench running to 'qualify' the design'),[65] and manufacturing was so halting that only 28 engines had reached the USAF by June 1948, though 31 more arrived that month. Moreover, 'the engines received had structural deficiencies which restricted them to very short periods of operation between inspection, [indicating] that much overhaul work would be needed.' This was not cheap. In August Major General Charles Stone reported 'six serious problems and several minor ones' with the J-47s (this before most of the other major failures discussed above) and estimated the 'cost *per engine* to make the needed design changes' at just under US$30,000.[66] Even with this work, late in 1949, the 2000+ engines delivered were averaging just 17 hours of operation between overhauls.[67]

Nevertheless, the Air Force believed the J-47 would become a reliable engine and committed it to multiple airframes, including experimental planes like the XB-51 and the XP-91. Initial J-47 dash-number models had averaged about 5200 pounds thrust, which, with afterburners and 'water injection' to boost takeoffs, reached 6000 pounds in some versions. This was not sufficient, however, to lift and speed the USAF combat planes being envisioned or redesigned, so early in 1950, Air Material Command commissioned 'development of the advanced J-47,' a powerplant rated above 9000 pounds thrust.[68] Such escalating expectations were to be anticipated, as were the arrays of technological surprises they yielded.

An organizational complication arose, however. The J-47 program exacerbated tensions between the AMC's Engineering and Procurement Divisions. The former 'decried th[e] practice of accepting new-series engines before the prototype had passed the qualifying model test,' as was repeatedly done with successive J-47 dash numbers. Procurement, by contrast, 'had to choose between costs of curtailing production until a prototype qualified in every part and costs of reworking engines produced before the test was completed.' Often its staff voted for rework, arguing that it provided supply, maintenance, and air crew personnel 'an early acquaintance with a completely new type of equipment,' even if a faulty one.[69] The engineers simmered and bided their time. By the close of 1949, program expenditures had reached US$300 million.[70]

In late July 1950, Supply Division, now on a war footing, complained that though its 'fixed ground rule' for J-47 engine overhaul planning designated 135 operating hours between teardowns, 'the present actual time … is approximately 76 hours.' The proliferation of J-47 types, reaching 17 models by early 1953 added more complexity.[71] Even before the Korean War, General Electric had initiated '"fix-it" shops near the airplane factories to take care of aircraft manufacturers' complaints concerning [the] new engines ….' War brought a rapid acceleration of J-47 demand and increased subcontracting from GE's huge Lockland Plant, at Evendale, OH, near Cincinnati. The USAF recruited Studebaker and Packard to build J-47s and Wright Aeronautical (among others) to fabricate components. Duplication of the necessary tools, fixtures and jigs incurred huge costs; acquisition of the special machine tools needed to carve, forge or mill engine components involved frustrating delays.

Even when parts began streaming in, quality control inspectors reported unacceptable deficiency and rejection rates. Wright was making compressor and turbine components, among other items. From January to July 1950, inspectors rejected some 42% of Wright's 313 compressor rotors, 34% of its 281 stators, and 13% of 306 delivered turbine wheels. GE's Quality Control Coordinator regarded this as 'excessive' and argued that 'Wright's performance is no better now that it has been in the past.'[72] Overall, just for the early F-86 uses, between March 1950 and June 1950, the J47s 'averaged six modifications per engine, a total of 20,487 modifications.' Costs were mind-boggling—by mid-1953, the USAF had devoted US$262 million just to 'plant facilities … for General Electric, its licensees and its subcontractors,' even as J-47 production contracts totaled approximately US$2.5 billion.[73]

The advanced J-47-23, designed for 9000 pound thrust, became a poster child for technological snags and pitfalls. In a six-page, single-spaced memo, Boeing notified AMC in May 1952 about 14 substantial deficiencies in this B-47 aircraft engine.[74] R. E. Brown first relayed Boeing's protest that the first -23 engines received were 'low in thrust and high in fuel specifics as compared with' their predecessor '-11 engines.' Boeing returned these units to GE for rework. Next, both the -23 and -25 versions experienced 'compressor stall' when sharply accelerated, especially at high altitudes. Compressor stall was extremely dangerous: when acceleration 'overloaded' the compressor rotors with excessive airflows, these airflows would break up into eddies and whorls, and amid loud thumping, the engine would suddenly stall, losing power and sometimes flaming out. This frightening problem compelled 'an almost completely redesigned fuel regulator,' among other fixes, Brown reported. In addition, the exhaust gas instrumentation, crucial for sensing engine overheating conditions, had been unreliable '[s]ince the delivery of the first J-47 engine for the B-47 program,' necessitating two rounds of redesign. Boeing also encountered problems with jet nozzles and tail pipes. Apparently, '[d]ue to the inherent manufacturing characteristics of the two facilities, the trend has been that "hot" engines are received from the GE Lynn plant and "cold" engines are received from the GE Lockland plant.'[75]

Despite this litany of problems and deficiencies, malfunctions and irregularities, J-47 engines did fire up and push planes into the air routinely, if not always reliably in the early 1950s, even if they wore out far faster than had been promised. However, repeated engine failures demonstrated that J-47 could self-destruct with little warning. The most common source was breakaway compression rotor blades scything through rear compressor stages, the combustor and turbine, wreaking metallic havoc. In other cases, apparently as a result of lubrication system failure, the central shaft bearings in J-47-23s tore out of their mountings, ripping engines entirely off bombers' wings. In dry prose, Brown recounted one event:

> It has been generally agreed that the recent difficulties with engines breaking away from engine mounts [*note the plural*] is mainly caused by oil starvation of the engine main bearings. On one flight of a B-47B airplane from Boeing–Wichita to the Tulsa Modification Center an outboard engine completely severed from the airplane and caused yaw forces great enough to result in permanent set in the airplane vertical fin and fuselage structure. Therefore in an interim measure to prevent *structural* failure of the airframe in event of recurrence of this situation, the B-47 airplanes were limited to 310 knots indicated airspeed.[76]

At 310 knots the tactical value of B-47s would be modest at best, for World War II prop planes achieved that speed. Therefore GE had to redesign the engine's oil system and oil tank, setting back the readiness schedule: 'the engineering alone for the new tank will require approximately three (3) months.'[77]

The -23, like other dash-numbered J-47 versions, triggered scores, even hundreds, of deficiency reports and underwent continuous component redesign and retrofitting, involving thousands of engineering changes and maintenance handbook updates,[78] even as it was being succeeded by -21, -25 and -29 versions (which segued into a new type classification, the J73).

What this technological innovation story documents, in relation to this essay's opening segment, are the profound non-linearity of jet propulsion development, the modest levels of technological confidence that could be transferred between engine models of increasing power and decreasing fuel consumption, and the Edison-like iteration of cut and try, fly and fix, in the absence of significant bodies of scientific knowledge whose application could guide improving performance reliability. Propulsion engineers learned through the failings and failures of the devices they designed and fabricated. Over and over, they created fixes that also failed and then tried something else.[79] Only through recurrent engineering interations did effective jet engines emerge from this melee; ultimately the J-47s' last versions were among the first gas turbines adopted for commercial use.[80] Yet as the Korean War wound down, the Air Force's newly reorganized Air Research and Development Command was hardly satisfied with General Electric. This military–industrial relationship had become barbed.

ARDC Major General D. L. Putt wrote C. W. LaPierre, Vice-President and General Manager of GE's Aircraft Gas Turbine Division in May 1953, initially to rake GE over the coals for its lax efforts on the J79 engine project.[81] Then Putt unloaded, in classic bureaucratic prose:

> General Electric's past performance on new engine developments has been such as to discourage Air Force optimism toward *any* new GE development, even if it were pursued with an evident enthusiasm and vigor. The first 200 J-47s were subsequently modified at a cost of approximately 2/3rds of the purchase price. The J-73 was scheduled to pass its 50-hour qualification test in November 1950. It has only recently completed this test. This engine [also] came out weighing approx. 30% more than the originally quoted weight. The J-53 development was also overweight by about the same percentage. … The J47-17 engine was originally promised to have completed its qualification test by July 1950 and to be in production by September. It passed an acceptable 150 hour qualification test in December 1952 [on the sixth try] after several hundred engines were shipped which were not qualified. …[82]

The Air Force could not build its own engines, of course, yet when fed billions of dollars, GE could not build them either, reliably, to specification, or on schedule. These partners in what later would be termed the military–industrial complex were tied together, certainly; but rather than imagining them cheerfully looting the public treasury, consider that they may have more resembled two tiring boxers sparring in a locked room, each disappointed with and antagonized by the other, neither having the key to the door.

## Technology-Led Research, Science and Jet Propulsion Innovation

Despite Admiral Richardson's certainty that mission-derived, product-oriented research would derange the progress of aeronautics and that only fundamental research would lead to 'aeronautical development and general technical progress,'[83] basic research rarely informed first-generation jet propulsion work. Research there was, and plenty of it, but it consistently focused on cleaning up failures or extending performance capabilities. Indeed, at the time, the core problem areas for jet researchers occupied terrains then sparsely populated with reliable scientific knowledge.[84] Thus the jet era's persistent technological challenges may have created pressure for basic scientific work in multiple fields, much as Kline suggested in his non-linear model. Here research examples linked to gas turbine work in the immediate postwar and key technological problem areas where existing science was unsatisfactory suggest this dynamic.

The US Navy and MIT jointly explored 'the practicability of casting gas turbine rotors' in the late 1940s. The project brief was as follows:

> The standard method of fabricating gas turbine rotors and disks by forging and machining heat-resistant alloys is time consuming and costly. Most heat-resistant alloys are difficult to forge and machine. Few hammers or presses are of sufficiently high capacity to adequately handle these alloys in the required sizes. Any development which might relieve

> this situation and possibly provide a higher rate of production of satisfactory rotors and disks would be of great importance.[85]

Extensive and systematic research involved forming hundreds of castings under closely controlled conditions, before X-ray analysis and testing to failure (rupture). Investigating the dynamics of metals and alloys was research in service of use, not in search of theory. 'The development of a method to attain close structural control, especially grain size and shape, was a prime consideration of this research program,' the authors explained. They tested variables like cooling rate, mold material, metal and mold temperatures, rate of pour and effect of heat treating. Their findings informed recommendations for practice: the ideal temperature for pouring (1525–50°C), size limits (*ca*. 100 lb), cooling techniques and the like. In closing, the researchers did not appeal for theoretical studies, but rather urged further foundry tests 'for producing superior cast rotors of various alloy compositions, since the work was restricted to only one alloy.'[86]

In parallel, the Air Force funded a seven-partner research effort on the machinability of alloy metals, involving Ford, Curtiss-Wright, Cincinnati Milling Machine, Metcut Research,[87] the University of Cincinnati, MIT and the USAF. Again researchers completed rigorous empirical testing: milling a wide range of irons, steels and high-temperature alloys, developing a cutter–wear scale based on the microstructures of each metallic sample and working out extensive charts indicating optimal speeds for a given composition and structure. They discovered that 'under extremely rapid cooling conditions,' a formation called 'white iron' appeared, so hard that it was 'virtually unmachinable.' This clarified the hazards of overly rapid cooling, but the researchers did not pose meta-level questions about *why* this form emerged from that cooling process, *what* implications their research as a whole held for understanding metals as a category of materials, or *why* different percentages of alloy elements contributed to creating specific metallic structures and characteristics. Sophisticated and precise, this study aimed to help 'cut costs' in auto and aircraft building alike.[88]

NACA certainly framed and executed the mission-specific scientific work that had irked Admiral Richardson, but the practice was ubiquitous.[89] As Lt General Kenneth Wolfe (Deputy Chief of Staff, Materiel, USAF) explained:

> At Wright Field in Dayton, Ohio, we have done considerable research in the field of tool design and planning, as well as the practical evaluation of actual fabrication and assembly methods. To show how important this problem appears to the Air Force, we are currently investing well over a million dollars in direct financing of straightforward investigations of aircraft tooling.[90]

Though in contrast to spending on jet engine production and massive fixes, a US$1 million was not much money, the USAF was systematic and scientific in using it. Researchers studied 'the various methods of tooling used on similar items spread geographically across the country,'[91] examined tooling practice in non-aeronautical industries and explored specific trouble spots: wartime critical material shortages, die-making and handling and jet engine compressor blades among them. Blades presented a maze of special challenges:

> [T]here are on the order of 2000 blades per jet engine. However, a multiplicity of design characteristics immediately reduces this to small units of production. There are distinct rotating and stationary blades in about equal numbers of 1000 each. There are hot end and cold end blades with temperature variation from 1800 to room temperature or less. The rotor and stator may have as many as thirteen different stages, consequently there are approximately 26 lots of 60 to 80 blades each per engine that are identical to each other … tolerances are fractions of thousands of an inch … . We are tooling up to utilize castings, forgings, powdered metal, rolled strip, extrusions, machined blades and ground blades, depending upon the design characteristics and functional requirements.[92]



Somewhere in technology's Pantheon, Edison smiled. No theoretical framework was available to guide the Wright Field researchers' efforts, which were purely empirical and Edisonian—try everything, twice, three times perhaps.

Last, in 1950 General Electric inaugurated its own Aircraft Gas Turbine Laboratory at Lynn, MA, honoring Sanford Moss, who had done foundational work on turbo-superchargers. Rather than undertaking basic research, 'the laboratory is to be used for components testing with facilities for full-scale and model testing of compressors and combustion systems.' In addition, to deal with the fact that 'considerably reduced panel space [inside jet cockpits] has forced manufacturers to develop pint-sized instruments,' GE had also opened a 'new Measurements Laboratory.' One device recently created, 'no larger than a penny matchbox,' reliably told pilots whether landing gear was 'up, down, or in between.' Now, to measure 'temperature, safe operating pressures, and fuel quantity,' designers worked on comparably precise instruments.[93] If laboratory staff reflected on the fundamentals of measurement, physically and epistemologically, this has not been documented—the Measurements Laboratory was not intended as a site for theory building. This is consistent with Edgerton's judgment—basic research was the tail the dog wagged, not the other way round, a minor domain amid a great landscape of project-based research.

So what jet technological problems bordered on scientific domains? As noted above, instrumentation, measurement and combustion were significant among them. So were questions concerning airflows, the characteristics of materials (metals, alloys, composites, plastics, ceramics, lubricants), the behavior of materials under stress, vibration, extremes of heat and cold, and techniques for cutting, forging, forming, casting, drilling, welding and bonding such materials for engine components. In the 1940s and 50s, the relevance of basic science to these problems was far from obvious. Certainly, there were cases where theory delivered the goods. Quantum mechanics provided 'a flood of insights about how matter and energy in forms such as light, heat and electricity can interact in almost magical ways.' Such insights propelled Walter Brattain and John Bardeen's creation of the point-contact transistor at Bell Laboratories in 1947, fathering microelectronics.[94]

Yet fundamental or basic science little assisted metallurgists struggling with creep, fracture and rupture, or seeking alloys with better resistance to high temperatures and cyclic loading. After all, 'jet engines … demand new kinds of metallic alloys that could take the much higher heat and stresses' than internal combustion engines. Relying on '[a]lloys that could remain strong while spinning furiously at temperatures that brought all known metals perilously close to a taffylike state would be the only way to feel confident that the turbine blades of a jet engine would not fail catastrophically.'[95] Had there been guidance from theory, doubtless it would have been used; but there was none. Indeed, in 1989, the Ninth Edition of a classic introduction to metallurgy asserted that 'fundamental understanding of many of the properties of metals still calls for intensive scientific work.'[96] A 1940s text/reference volume explained:

> [Man] knows a great deal about the effects of various chemical and physical reactions entering into metallurgical processes, but he has paid very little attention to the *causes*, the underlying fundamentals which are, in the last analysis, the controlling factors in understanding the nature of metallic materials. He is an expert in the art but a tyro in the science.[97]

Not until 1958 were federal interdisciplinary materials laboratories authorized, the same year the first university department of materials science was approved. Fifteen more years lapsed before a disciplinary organization appeared, when 'the Materials Research Society [convened] at Pennsylvania State University.'[98]

Combustion was another black hole, connected fundamentally to its colleague, compression. Both impinged on the territory governed by fluid dynamics. Combustion processes could overheat and melt or split engine liners, could flame out, yielding zero thrust and could be difficult to re-start at altitude. Compressor stall from excessive airflow has been noted; but it had another source, compressor surge that arose from undercompressed airflows. In some designs 'the front stages would not compress the flow enough to pass through the smaller annuli of the rear stages, causing these stages to stall and the compressor to go into a violent instability called surge.'[99]

Here, the work of auto and aircraft engineers 'for a century of engine development, had yielded empirical knowledge but little detailed information about the process of combustion.' The initial review of combustion research from a scientific viewpoint appeared only in 1965; 16 years later the first US national laboratory for combustion research was created at Sandia (1981), propelled by the dual oil crises and emphases on improved fuel efficiency, ground and air.[100] None of this did early jet engine builders much good.

Thus compressor design remained iterative and empirical, while the fluid dynamics that in some fashion undergirded it was limited by computation shortcomings. Once the severity of compressor problems was accepted, NACA undertook a broad program of research aimed at 'advanc[ing compressor] performance':

> One product of the NACA research program was a three-volume Confidential Research Memorandum, issued in 1956, often referred to as the 'Compressor Bible' in the industry. These volumes presented a complete *semi-empirical* method for designing axial compressors achieving levels of performance far beyond the standard of the mid-1940s. Subsequent advances notwithstanding, including the advent of computer-based analytical techniques in the mid-1950s, this design method remained in use for at least the next quarter century, if not still today.[101]

Moreover, fluid dynamics in the 1950s was not sufficiently sophisticated to analyze mathematically the extraordinary complexity of compressor airflows. George Smith and David Mindell put it this way:

> The aerodynamics in a compressor blade row [are] enormously more complicated than across a wing. … The flow (a cascade) is not flow across an isolated airfoil, but flow in a channel defined by the suction surface of one blade and the pressure surface of the adjacent blade. … The turning of the flow within the blade passage … produce[s] three-dimensional effects, including so called secondary flows causing the flow near the surfaces to migrate within the passages. On top of all this, alternating rotating and stationary blade rows make the flow in a compressor inherently unsteady.
>
> Reasonably realistic calculations of a flow in a compressor blade row became possible *only in the late 1980s*, and even these calculations employ approximate engineering models of turbulence rather than solving the equations of motion for turbulent flows.[102]

Thus, basic scientific knowledge was insufficient to the needs of jet propulsion engineers and technologists, public and private, in multiple, critical problem areas for decades after World War II. Addressing those problem areas generated massive research programs, again public and private, and may have set agendas for basic science as well (investigating this is another task). At a minimum, the jet propulsion case reinforces conclusions the British Wealth From Knowledge group articulated: 'most businessmen already know … that the great bulk of basic science bears only tenuously if at all on the operations of industry.'[103]

## Conclusion

If we return in closing to the considerations expressed in this essay's opening pages, perhaps the awkward record of jet propulsion can be coordinated with several issues raised at the outset. First, in relation to the conference call's reference to the linear model of innovation being in crisis, on the evidence presented here it should surely be discarded. Constant innovation was the heart of jet engine development and the military commissioned task-specific research to address/resolve obstacles in that process. In critical areas basic science remained too underdeveloped to offer guidance. Second, it would be worth exploring where the model does survive, if not in the history of technology, perhaps in the history of science, military and policy history, museum interpretation, in the practice and teaching of engineering and science and elsewhere? Where, after all, does it work, as in the quantum mechanics-transistor case, and how prevalent are such cases? Third, for all its multi-track openness, Kline's non-linear model of innovation does not seem to capture the bizarre interactions evident in the jet propulsion story, even as rendered here in a heavily compressed and partial fashion. Certainly, jet engine innovation was Edisonian and domains of work within it were systematic and rational, alert to scientific protocols.[104] Still, viewed through the eyes of the participants, the entire process was neither rational, nor a random walk. Instead it was a contingent, negotiated struggle with the material world's capabilities and limits, a fierce effort to overcome failure along with the Soviets, a political conflict between and within powerful organizations, and a collective, secret, industry–state project whose interior messiness merits our attention. Further studies will help resolve the question whether innovation and science–technology relations can be modeled successfully, but on the basis of the WTK research and the jet propulsion case, it seems doubtful, unless multiple domains of practice can be defined, within each of which shared approaches and relations emerge.

Historians of technology have long accepted the challenge of moving beyond 'progress talk,' a task they share with colleagues in related fields. Likely, our steps toward cultural studies, consumption, and users' roles in designing and defining technologies have drawn scholars away from the search for system, order and rationality that has often guided our efforts. Surely, from the consumption side, this generation of work has highlighted the non-rational, non-linear elements ever present in technological development. Perhaps now returning to production, recognizing and engaging its own deep irrationalities and passions, its confrontations with the material, and its connections to power, could represent an invigorating effort. In any event, if there are to be future models of technology–science relationships in innovation, they may be expected to emerge from a series of detailed, forward-leaning studies, which start with actors facing situated uncertainty and prospective failure, rather than commencing with outcomes then searching for their origins.

Most generally, beyond system there is disorder, as well as the disorder provoked by insufficiencies in system building, something the jet propulsion case illuminates. Exploring technology's history by composing close-to-the-ground narratives of confusion and error could have lasting value, not least in realizing the potential for understanding that a process-oriented, contextualized 'internalism' could provide. In doing so, historians of technology can revisit artifacts, the mutating stuff of technology, no longer isolated or frozen, but humanized, localized and politicized amid shifting contingencies in design, production, practice, and knowledge. Thereby, we may also grapple with the non-linear, messy contingencies central to innovation, one of this discipline's central concepts and one of its enduring challenges.



## Notes

[1] Jevons, 'Preface,' xii.

[2] 'Networks not Pipelines.' *The Economist*, 18 February 1999.

[3] German Historical Institute, Call for Papers: Science and Technology in the 20th Century, March 2004, 1.

[4] Reader No. 1, Comments and recommendations re: Blaszczyk and Scranton, *Major Problems in American Business History*, 6 May 2004, 23–4.

[5] Indeed, Rosalind Williams noted, in responding to a draft of this essay, that 90%-plus of her MIT colleagues still 'believe in' the linear model, so there is apparently quite a gap between historians of technology and university colleagues in engineering and science. (Williams to Scranton e-mail, 16 August 2004.)

[6] Kealey, *The Economic Laws*, 4–5. Kealey quotes Bacon as writing in the *Cogitata et Visa* (1607), 'the improvement of man's mind and the improvement of his lot are one and the same thing,' and in the *Novum Organum* (1620), 'The benefits inventors confer extend to the whole human race.'

[7] Kealey, *The Economic Laws*, 148. Kealey indicates that his predecessors were Alexander Bache, first head of the National Academy of Sciences in the 1860s and George Ellery Hale, who steered creation of the National Research Council in the First World War years (141–6).

[8] Zachary, *Endless Frontier*, 255. The encapsulated Bush quote may be found in Bush, *Science—The Endless Frontier*, *Endless Frontier*, 13.

[9] Zachary, *Endless Frontier*, 257.

[10] Richardson, 'Air Power in the World Crisis,' quote from 12.

[11] For an earlier effort, see 'Experts Tell Why Planes Cost So Much.' *Aviation Week* 55 (1951): 23–8. For hearings, see *Department of Defense Appropriations for 1965*, Hearings before a subcommittee of the House Appropriations Committee, 17 February–2 March 1964. Washington: GPO, 1964. ii, 771 pp.

[12] Sherwin and Isenson, 'First Interim Report,' 1.

[13] Coillet, 'Management of Mission Oriented Research.' It does not appear that the Apollo capsule assessment project was developed.

[14] Sherwin and Isenson, 'First Interim Report,' 10–13.

[15] Ibid., 7, 16. For a 'naturalistic' account of basic science and especially of its post-1970s transformations, see Ziman, *Real Science*.

[16] Illinois Institute of Technology, 'Technology in Retrospect' (TRACES). The five topics investigated were magnetic ferrites, the video tape recorder, the oral contraceptive pill, the electron microscope, and matrix isolation techniques. It appears these five were selected by something like a focus group of a dozen IIT Research Institute senior staff. The report provided no information on selection criteria or protocols, other than that a larger initial group was identified then pared down to the final five. See Illinois Institute of Technology, 'Technology in Retrospect,' Vol. 1, xi–xiii.

[17] The value of having historians involved is of course debatable, given the triumphalist and 'progress talk' nature of much American history in this era, especially in the history of science and technology. Coillet noted that there was a panel discussion on Hindsight and TRACES at the annual Society for the History of Technology (SHOT) conference in December 1969 'in a joint session with the History of Science Society,' but I have not discovered what took place there (Coillet, 'Management of Mission Oriented Research,' 35).

[18] For a view that claims about basic science's delivery of technological value were concerned more generally with assuring flows of federal funds to university researchers, see Edgerton, 'The Linear Model,' a paper presented at Nobel Symposium 123, November 2003.

[19] Coillet, 'Management of Mission Oriented Research,' *passim*; Kreilkamp, 'Hindsight and the Real World'; Batelle, 'Interactions of Science.'

[20] Coillet, 'Management of Mission Oriented Research,' 21, 33, 78.

[21] Edgerton, 'The Linear Model,' 44.

[22] Langrish *et al.*, *Wealth from Knowledge*, 3.

[23] Ibid., 24. The team did classify entire innovations as major, modest or minor and organized them according to the four-part schema sketched in note 25 *infra*, but the bulk of their study (over 80%) represented individual narratives of the case study innovations, which offer convincing evidence of their finding neither a singular pattern, nor two, three, or four clustered process types.

[24] Illinois Institute of Technology, 'Technology in Retrospect,' Vol. 1, ix.

[25] They explored four putative linear relationships: DS—'science discovers, technology applies'; DT—'technological discovery'; NC—'customer need'; and NX—'management by objective,' the last being the innovations created inside an enterprise without stimulation from client needs. Crucially, NFK found that 'When the Queen's Award innovations are examined, *very few of them fit any one of the above models* in a clear and unambiguous way.' *WFK*, 73–4, emphasis in original.

[26] *WFK*, 35 (emphasis added). Note that in offering this conclusion, the TRACES team violated its own definitions, for non-mission research was to have been motivated *only by curiosity*, not by technological obstacles that needed fuller scientific understanding to be overcome. Were the basic science research stimulated from and funded for technological uses re-coded as another class of mission-oriented research, the scope of curiosity-based science would have narrowed sharply. At a 1966 conference, Derek della Sola Price commented on the porosity of such classifications: 'if a man is paid by a pharmaceutical company to develop a new drug but

finds himself publishing a paper about the new chemical knowledge gained, he has been guilty of doing science on company time; but if a pure mathematician comes up with a new way of designing switchboards, it is technology, however pure be his mind' (della Sola Price, 'The Structures of Publication,' 97).

[27] Edgerton, 'The Linear Model,' 43–4. Site address for the original paper: http://www.imperial.ac.uk/historyofscience/word_docs/Nob_Symp%20May2004.doc It may be worth noting that I stumbled independently on one notion Edgerton offers there, that R&D should be reversed to D&R, in my paper proposal for the GHI conference, drafted in March 2004. As we begin to probe more thoroughly into the dynamics of creating complex technologies, this inversion may make more sense and become more common and useful.

[28] A bit similar to WFK's NC and NX sources (see note 25).

[29] Kline, 'Innovation.'

[30] Ibid., 39.

[31] Ibid., 40–1. Regarding electronics, RCA's television pioneer Vladimir Zworkin commented: 'Whatever the future of electronics might be in the ensuing half century, it is certain to profit from fundamental research as it has in the past and to repay its debt generously in the form of instruments and methods to carry out the tasks of [fundamental research].' This suggests a two-way street, but the lead impetus still moves from basic research to electronics applications. (Zworkin, 'Some Prospects,' quote from 80).

[32] Ibid., 44–5.

[33] Ibid., 41(emphasis in original). Kline, regrettably, did not define these 'sociotechical systems.'

[34] Ibid., 41–2. For an attempt at cross-national comparison of innovation paths, see Kline, 'Styles of Innovation.'

[35] The Technical Editor, 'Designs of a Decade.' The author credited jet propulsion's rapid advance to 'the immense strides made in all branches of mechanical, physical and chemical technology … particularly during the past 15 years, allied to the forcing house conditions imposed by war' (559).

[36] Personal communication from Adam Tooze, Jesus College, Cambridge Univeristy, who is working on a study of German agriculture during World War II, March 2004. Horses as well provided essential wartime service to Soviet, Polish and French military and civilian organizations, at a minimum. See Overy, *Why the Allies Won*, 215–21.

[37] The exceptions being the UK's Gloster Meteor, which attacked V-1 flying bombs, and the German ME262 jet fighter which appeared too late in the War in too small numbers to make a major difference, but which, flying 520 mph, instantly achieved a level of air superiority overcome only at times by swarms of Allied propeller-driven fighters. Before long, Hans von Ohain's axial jet engine design would become the standard form until the era of the turbo-fan, which in essence was a bulked-up axial. For more on Ohain, see Constant, *The Origins*, 194–213.

[38] For one example, the US State Department was hugely upset over British sales of Rolls-Royce Nene and Derwent engines to the Soviet Union in 1946. See 'British Jet Stirs Queries on Soviet,' *New York Times*, 12 September 1950, 35. One technical journalist commented on the classification situation as follows: 'Nothing makes an engineering reporter more unhappy than to have a good story and not be permitted to tell it. Such is the position we find ourselves in each year-end as we sit down to report jet-engine development. Security regulations confine the story to generalities' ('A Story of Jet Engine Progress,' *Westinghouse Engineer* 11 (January 1951): 32).

[39] Frank Whittle was the Royal Air Force engineer who created the initial centrifugal compression turbojet engines and founded the firm PowerJets in the mid-1930s. See Constant, *The Origins*, 179–93.

[40] Col. R. C. Wilson to Commanding General, Army Air Force (AAF) Material Command, 15 June 1944 (declassified), Air Force History Office, Microfilm Reel A2078, Frame 1014.

[41] Muir, 'Republic Aviation,' quote from 61 (emphasis added).

[42] This was less important for fighter planes than for transports and bombers. Some estimates of World War II AAF fighters' 'effective life' periods suggest 100 flight hours of use was a benchmark. Navy fighters did much more patrolling than dog-fighting and bomber escort, demanding far more robust engine durability levels. The Navy was somewhat more cautious about jet power in the postwar than the AAF (soon to become the USAF). See also note 62.

[43] See 'Experts Tell Why Planes Cost So Much,' *Aviation Week* 55 (27 August 1951): 23–8, quote from 27.

[44] Turboprops use the rotation of a turbine shaft to drive a propeller (using a complex gear-set to reduce the jet's rotation speed to a level that would not tear the prop off the front of the engine). Only a small percentage of the thrust the jet develops pushes the plane; rather, most of the power is used to drive the prop, which in effect pulls the plane through the air.

[45] St. Peter, *The History of Aircraft*, plate, inside front cover.

[46] Moreover, the share of engine costs in aircraft total costs rose substantially in the jet transition, as did the share of instrumentation costs (avionics) as a electronic transition also matured. For statistical detail and an overview of the 1940s in this regard, see Robert McLarren, 'Analyzing the New Procurement,' *Aero Digest* 61(August 1950): 15-16, 104–05.

[47] See David Hounshell, *From the American System to Mass Production, 1800–1932*, Baltimore: Johns Hopkins University Press, 1984, 252–53.

[48] For more details, see E. L. Tangerman and Anderson Ashburn, 'How GE Makes Turbojets,' *American Machinist* 95(5 February 1951), 135-54. The rest of this extended article included dozens of photos depicting a range of components-making and engine fabrication steps. As all this work was classified and entry to plants closely controlled, it was to me surprising that this level of detail appeared in a wide circulation industrial journal just as the Cold War was heating up in Korea.

[49] 'Designs of a Decade,' *Flight*, Issue No. 2207 (11 May 1951): 559–70, quote from 564. See also Robert McLarren, 'Turbojet "Simplicity" Is Design Complexity,' *Aviation Week* 50 (7 March 1949): 29–33.

[50] The 2003 Centennial of Flight Commission essay on GE jet engines discusses the J-47 thus: 'GE began developing the J47 from the earlier J35. The J47 would power several of the new front-line military aircraft, including the F-86 Sabre Jet, which set a new world's speed record of just under 671 miles per hour (1,080 kilometers per hour) in September 1948. Demand for the engine soared during the Korean War, and more than 35,000 were delivered by the end of the 1950s. During 1953–54, J47 production reached a rate of 975 engines per month. The J47 was also the first turbojet certified for civil use by the U.S. Civil Aeronautics Administration and the first to use an electrically controlled afterburner to boost its thrust.' http://www.centennialofflight.gov/essay/Aerospace/GE/Aero11.htm This differs considerably from the discussion that follows, omitting all the miseries and failures in design and production along the road, not an uncommon narrative approach in aeronautical and aerospace history.

[51] Virtually all of what follows relies on declassified USAF documents, to my knowledge hitherto not used for research on jet propulsion. They are not cited in St Peter, *The History of Aircraft* (note 45) and were made available to me on microfilm by the extraordinarily helpful staff at the Air Force History Support Office, Maxwell AFB, AL. The AAF and USAF created thousands of project histories, a bibliography for which was compiled in Neufeld, *United States Air Force History*. Many remain classified, but those 'declassed' often include rich narratives drafted by Air Force contract historians along with brief summaries of the documents cited in the narratives and photographic reproductions of hundreds of the original documents these scholars used. To these unheralded historians, whose work was inaccessible for a generation or more, I owe an enormous debt of gratitude.

[52] Vorce, 'Case History of the J-47.'

[53] Transcript, Telephone call between Colonel Shepard and General Powers, 15 October 1945, 2:30 pm, AFSHO Microfilm Reel K2044, frames 888–90 (emphases added). The TG-180 was also known as the J-35 (Air Force designation). Only 124 were produced and shipped by GE; the

company's total wartime jet engine production only reached 700, chiefly because the power plants needed so much testing and rework, leading to major re-engineering of designs. GM–Allison, however, build several thousand GE-designed engines during the war, indicating a division of labor between experimental development and rework (GE) and production for use (GM–A).

[54] The exception was General Motors' Allison Division, which continued to build aircraft engines and does so to this day, though now as a division of Rolls Royce.

[55] Fuel comparisons were made through use of an engineering index, *specific fuel consumption* (sfc), which reflected the number of pounds of fuel needed to produce one pound of thrust for an hour. Aircraft fuel is usually measured in pounds, due to the constant concern with the total weight of the craft. Thus a 5000 lb thrust engine (the J-47) running at 1.1 sfc, demanded 5500 lb of fuel to run at full thrust for 1 hour. (Actual J-47s in the early years generated 5200 lb thrust and had a 1.13 sfc.) Unsurprisingly jet pilots developed various means to save fuel, recognizing for example that less dense air at higher altitudes offered less resistance (and less lift, too), so that sustaining a specific airspeed took less fuel per hour than at lower altitudes, other things being equal. For clear discussions of these and other issues, see Constant, *Gas Turbines*.

[56] J. C. Beuchel, GE-Lynn, to Commanding General, ATSC, Wright Field, 4 March 1946, AFSHO Microfilm Reel K2044, Frames 892–94 (emphasis in original).

[57] R. E. Small, 'Memorandum: General Electric Type TG-190 Jet-Propulsion Aircraft Gas Turbine,' River Works [Lynn, MA], 14 May 1946, AFHSO Microfilm Reel K2044, Frames 906-07. Small explained: 'One question which will probably arise is whether the extrapolated performance given in the TG-180 Performance and Installation Manual can be ratioed in order to obtain advance information on the TG-190 extrapolated performance estimates. At the present time, we do not believe that this would be a sound computation to make because altitude, air speed, and ambient temperature conditions may affect the TG-190 to a different degree that they affect the TG-180.' As Small's memo came two months about Beuchel's 'basic research' letter, it seems unlikely that any fundamental scientific investigations were attempted and unlikely, had any been undertaken, that they would have substantive findings to report in a few months time.

[58] Vorce, 'Case History of the J-47,' 2–4.

[59] Power Plant Branch, AAF Procurement Division to Colonel J. L. Zoekler, Aero Equipment Section, 5 January 1948 (AFHSO Microfilm Reel K2044, Frame 1093).

[60] Also, the shaft drove an electric generator that powered fuel pumps, instrumentation, etc.

[61] Vorce, 'Case History of the J-47,' 6–7. The -7 and similar extensions are known as 'dash numbers' in US aircraft engine terminology. They signal differing versions of the engine type, after being redesigned for use in various aircraft or for remedying major flaws. Odd dash numbers refer to Air Force procurement, even numbers to engines for the Navy's Bureau of Aeronautics.

[62] Ibid., 7–8. 'Poor workmanship' was charged to GE repeatedly, and to Curtiss-Wright, which failed as a jet engine builder. This appears not to have been an issue at Pratt and Whitney, which worked chiefly on Navy jet projects. The Navy was more gradual in its embrace of jet propulsion, which may have been a factor. As late as 1951 only 25% of the floor space at P&W's giant East Hartford, CT, facility was 'devoted to jets. In terms of horsepower this is equivalent to 30 percent of the total engine output.' Stone, 'Mobilization Stirs,' quote from 22. On Curtiss-Wright's failings, see Schultz, 'Case History of the YJ-65,' and Self, 'Case History of the J-65 Series.'

[63] Ibid., 8; Col. R. J. Minty, Chief, Power Plant Lab, 'Project Progress Report on J-47 Engine,' 24 October 1949 (AFHSO Microfilm Reel K2044, Frames 1416–19). Both planes experiencing the total seal blowouts landed safely.

[64] Ethel DeHaven, in the second part of the AMC's J-47 case study explained it this way: 'Under the stress of war the Army Air Forces had adopted the practice of telescoping development with production of the [J-33] engine and both development and production with the training of crews and maintenance men. Development during production continued with the postwar development of the TG190 [J-35] and TG190 [J-47]. This made it

possible to get approvals of transfers of funds from production to development projects, so long as the development was in fact improvement of an earlier series rather than a new design' (DeHaven, 'Case Study,' 9).

[65] A J-47-7 would finally complete a 150 hour test successfully in March 1949, on the fourth attempt. (Power Plant Lab to W. M. Brown, Power Plant Branch, Procurement Division, 22 March 1949, AFHSO Microfilm Reel K2044, Frame 1315.)

[66] Vorce, 'Case History of the J-47,' 9–12; Major Charles Goff, Aero Equipment Section, Procurement Division, to Brig. General H. A. Shepard, Chief, Procurement Division, 23 August 1948 (summarized at Vorce, 'Case History of the J-47,' 53–4). The first 10 experimental J-47s cost US$157,800 each, but production models were negotiated at about US$45,000 each (Vorce, 'Case History of the J-47,' 3, 4). See also Maj. General K. B. Wolfe, Director, Procurement and Industrial Planning to Salmonsen, GE, West Lynn, 26 August 1948, which (in summary) stressed that 'reports of serious latent defects in the J-47 engines when they were undergoing periodic safety inspections at Muroc AF Base had led AMC [Air Materiel Command] to consider taking drastic action. It appeared advisable to refuse to accept any more engines until fixes correcting the various deficiencies were incorporated in production engines. Such a decision would greatly impede the National Defense Program, as well as embarrass the GE Co., but unless immediate and satisfactory action was taken by GE, AMC would have no alternative. *Knowledge gained on the J-35 had not been applied to the J-47*, and the engineering of the latter engine had not advanced as far as the J-35' (Vorce, 'Case History of the J-47,' 55, emphasis added).

[67] DeHaven, 'Case Study,' 11.

[68] Vorce, 'Case History of the J-47,' 16. See also AMC Engineering Division, 'Record of Official Contacts: Performance Growth of J47-GE-17 & J47-GE-23 Engines,' 20 October 1950 (AFHSO Microfilm Reel 23718, Frame 543).

[69] DeHaven, 'Case Study,' 12–13. One sign of how substantially different technically dash-number engine types were can be gleaned from a GE Technical Publications Division communication to AMC Boston in 1950. 'To insure handbook coverage for the new J47-17 turbo-jet engine the contract requires revisions to the existing technical orders. However, because of the wide departure of this engine from current designs, we feel that completely new instructions should be issued. [This is needed because] To revise present handbooks on a "model difference basis" would require changes in an estimated 90 per cent of the Service Information pages, 85 per cent of the Overhaul pages, and all of the Parts Catalog pages' (R. L. Buchanan to Chief, Boston USAF Procurement Office, 30 March 1950 (AFHSO Microfilm Reel 23718, Frame 211)).

[70] For example, in a scathing early 1950 review of GE's attempt to develop an adequate engine power control, the Engineering Division the Power Plant Laboratory Chief concluded: 'It is predicted that the control problem will be extremely serious from both a safety and a monetary standpoint in the probable retrofit program,' Col. M. C. Demlert, Chief, PPL, 'J47-GE-17 Engine Power Control,' undated (from internal evidence, after 16 January and before 20 February 1950), AFHSO Microfilm Reel 23718, Frames 185–6.

[71] DeHaven, 'Case Study,' 22, 45.

[72] DeHaven, 'Case Study,' 48; A. Fedowitz, 'Quality Summary of Major Wright Aeronautical Components,' 21 August 1950 (AFHSO Microfilm Reel 23718, Frames 501-02). On Wright's tattered record in World War II propulsion engineering, see Cameron, 'ATDC' (in *Torque Meter*, the Aircraft Engine Historical Society's journal).

[73] DeHaven, 'Case Study,' 41–2, 51, 71. There would be reductions in the manufacturing contracts as the war wound down, but over 25,000 J-47s in many variants were built for the military.

[74] There were separate analyses and complaints about other dash numbers installed, for example, the J47–17 used in versions of the F-86, but there's no space here for more extended discussion. On these problems see a series of 1951–52 memos from R. H. Rice, Vice President and Chief Engineer, North American Aviation to Commanding General, Air Materiel

Command, 25 June 1951, 18 September 1951, and 21 April 1952 (AFHSO Microfilm Reel K2044, Frames 481–9, 529–39, 548–64). Single-spaced, these three 'Deficiencies' reports cover 37 pages.

[75] R. E. Brown (Boeing) to Colonel Warren, AMC, 'B-47 Type Aircraft—Power Plant and Related Problems,' 21 May 1952, 1–6 (AFHSO Microfilm Reel 23719, frames 30–5, quotes from 30–2). 'Hot' engines ran above the specified exhaust temperature and 'cold' ones below the specification; this affected sfc and thrust both.

[76] Brown to Warren, 3 (Reel 23719, Frame 32).

[77] Ibid.

[78] The plague of engineering changes continued into the NASA era. See 'Change-Order Headaches Will Intensify,' *Engineering News-Record* 167 (16 November 1961): 124–5. Moreover, military–industry projects to increase jet engine capabilities continued to have major technical failures, run over budgets, and encounter substantial delays. See Drewes, *The Air Force*, and Younassi *et al.*, *Military Jet Engine Acquisition: Technology Basics and Cost-Estimating Methodology*, Santa Monica, CA: RAND, 2002. In commercial jet propulsion, the stabilization of key engine types through design freezes appears to have fostered the accumulation of in-depth operational knowledge, by contrast. As well, commercial jets logged hours several orders of magnitude greater than military jets (except for transports), which speeded development of such knowledge.

[79] The language of orderliness infuses the surface categorization of military jet engines, each project having a J-number, the numbers rising ever higher over time, each version of the JXX engine with its dash number, rising neatly as well, some dash number having letter sub-classifications (A, B, C …). This linguistic convention obscures (perhaps assuages) the deeper disorder of technological innovation on the ground and fosters an illusion of stately progress, rather than exposing the reality of continuous scrambling after function and reliability. See also note 50.

[80] This was as the CJ-805, which powered the Convair 880, c. 1957. Pratt and Whitney's JT3C and JT4A jets propelled the Boeing 707 and the DC8 respectively (GE, *Aircraft Propulsion*, 143).

[81] GE was criticized for demanding large production contracts for engines as a guarantee that their development work would not be farmed out to other possible producers and as 'banked funds' that could be transferred as needed to engineering development. This was a complex political problem, and cannot be explored here, but none of the parties—military, Congress, builders—wanted to acknowledge the technological messiness of jet (and perhaps other weapons system) development. The linearity Edgerton rightly critiques was, I would suggest, lodged in the expectations that development would be completed before production was undertaken, that separable funding allocations could logically be targeted to each, etc., rather than that continuous failure and continuous learning would generate continuous redesign of an unstable technology that never would reach the goal of design-frozen, turnkey production. On this and GE's demands, see Vorce, 'Case History of the J-47,' 1, 10; DeHaven, 'Case Study,' 6–8, 54–60.

[82] Major Gen. D. L. Putt, ARDC to C. W. LaPierre, VP&GM, GE Aircraft Gas Turbine Division, 6 May 1953, in Donald McVeigh, 'Turbofan Engine Development, 1953–1960,' ARDC, 1960 [SECRET, declassified 1972], AFSHO Microfilm Reel K2857, no frame number.

[83] Richardson, 'Technical Progress.'

[84] An excellent depiction of this situation in the late 1940s can be found in Silverstein, 'Research on Aircraft.' In the latter 1950s, Silverstein became a core member of NASA's Space Task Group and helped manage the Mercury program, among others.

[85] Grant *et al.*, 'Development and Evaluation,' quote from 86.

[86] Ibid, 80, 237–8. These studies aimed to improve gas turbines for ship propulsion, but the relevance of the research to practices in creating aircraft gas turbines was direct.

[87] For Metcut Research, see http://www.metcut.com/metcut/metcutabout.html. Still in operation, it started in 1948; this machinability study must have been one of the enterprise's first contracts.

[88] 'Air Force–Curtiss-Wright–Ford–Metcut Report Proves Machinability Depends on Microstructure,' *American Machinist* 94 (27 November 1950): 109–24. See also, 'Air Force–Curtiss-Wright Report Machinability Data for High-Temperature Alloys,' *American Machinist* (25 December 1950): 99–106.

[89] For one NACA project see 'Research Pushed on Gas Turbines,' *Steel* 129 (27 August 1951): 72–5, 101. For NACA's increasing role as a testing center for jet components developed in tandem by the military and business contractors, see Allen, 'Research Uncovers Materials.'

[90] Wolfe, 'New Concepts,' quote from 37.

[91] Ibid., 130. For a similar geographical variation in industrial practice on common objects, but far grander ones, World War II aircraft, see the insightful works of Ferguson, 'Controlling Knowledge' and 'One Thousand Planes a Day.'

[92] Ibid., 132.

[93] 'Aircraft Gas Turbines,' *Mechanical Engineering* 73 (January 1951): 22–4. See also 'GE Dedicates Aircraft Gas Turbine Laboratory,' *Automobile Industries* 103 (1 December 1950): 32–3. It should also be noted that the National Advisory Committee on Aeronautics (NACA, later NASA) had established a propulsion lab in 1943 (Lewis, at Cleveland) to which forces from its Langley facility were added. Despite NACA's commitment to basic research, Lewis proved not to be a source of fundamental or theoretical work for jet propulsion, but instead did a great variety of components testing. See Dawson, *Engines and Innovation*, esp. ch. 7.

[94] Amato, *Stuff*, 83–5.

[95] Ibid., 89.

[96] Alexander and Street, *Metals*, 6. The authors pointed out that when they were 'beginning metallurgical research [in the 1940s], it was necessary to spend many weeks reading scientific journals, hoping to discover some information which might be useful' (289).

[97] Sisco, *Modern Metallurgy*, 4–5. Interestingly enough, Sisco was the former Chief Metallurgist at the US Army Air Corps' Wright Field facilities in Dayton, Ohio.

[98] Amato, *Stuff*, 91–5. The first Materials Science department was at Northwestern, which reoriented and renamed its metallurgy unit. Much the same empirically based research animated polymer chemistry in this era as well. 'Many of the polymer players were sophisticated theoretical chemists, but the development of the materials still advanced largely by dogged empirical work. Try this or that reaction. Try this or that temperature. Throw in a little metal and see if catalyzes different reactions … . Not only were researchers clueless as to how polymer molecules clump together to form a material, until the mid 1930s most chemists did not even acknowledge that polymeric materials were made of huge molecules' (76).

[99] Smith and Mindell, 'The Emergence,' 117.

[100] Carlisle *et al.*, *The Combustion*, quote from 6. The overview was Fristrom and Westenberg, *Flame Structure*. Fristrom published an updated edition with Oxford University Press in 1995, the first chapter of which covers 'The Science of Combustion: Its History, Scope and Literature.' See also Liman and Williams, *Fundamental Aspects*, which also discusses the field's history. The key point here is that modeling combustion mathematically only became feasible for simple, 'laminar' flames (think Bunsen burners). In 1993 Liman and Williams reported no adequate modeling of 'turbulent' combustion, the usual flow for 'practical combustion devices': 'Thus far we have studiously avoided mentioning turbulence … because of the complications that it introduces … . Addition of combustion to turbulence increases the complexity and opens new avenues of investigation. Knowledge of different avenues of approach can enable appropriate selections to be made of methods for seeking deeper understanding or for obtaining practical predictions of turbulent reacting flows. … Future study … may be expected to reveal unanticipated aspects of turbulent combustion' (Liman and Williams, *Fundamental Aspects*, 111).

[101] Smith and Mindell, 'The Emergence,' 121, emphasis added. The authors did not define 'semi-empirical' but we may suspect that this references a prescriptive formalization of empirical results that was not founded on adequate or reliable scientific theory.

[102] Ibid., 150–1 (emphasis added).
[103] Langrish *et al.*, *Wealth from Knowledge*, xii.
[104] Here again, Ziman's *Real Science* draws a helpful portrait of scientific practice.

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