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ENGINEERING AND APPLIED PHYSICS IN THE AGE OF FRACTURE

On January 1, 1984, a deal reached between AT&T and the US Department of Justice two years earlier went into effect, ending AT&T's regulated monopoly status while breaking its regional subsidiaries off as independent "Baby Bells." At the time, AT&T was one of the most influential companies in the world: employer of more than a million people at its height; crucible in which Unix, information theory, the transistor, computer music, and many other innovations were forged; operator of nuclear test sites and weapons labs; and of course the nearly unchallenged provider of telecommunications to most of a continent. Its breakup after ten years of antitrust lawsuits set American business, government, and technoscience on a new path of deregulated competition and eventual unfettered remonopolization that has lasted to the present. As such, the AT&T breakup signaled the end of the long 1970s as decisively as any other event.[1]

As in chapter 1, this chapter summarizes the arguments of the book and connects them to present-day ideas of "responsible research and innovation" through a somewhat abbreviated empirical case study. This time, my empirical examples are drawn from the career of a minor figure in AT&T's breakup, Arthur D. Hall III. From 1950 to 1966, Hall was an

AT&T employee, during which time he was particularly associated with the failed Picturephone project—a forerunner of today's Zoom, Skype, and FaceTime.[2] While at AT&T, Hall also wrote an influential book, *A Methodology for Systems Engineering*, and was a founding member of the Institute of Electrical and Electronics Engineers (IEEE) professional group on Systems Science and Cybernetics.

After 1966, Hall initially bounced around senior management positions at other companies while teaching part-time at the University of Pennsylvania. Then in 1970 he founded his own consulting firm, Arthur D. Hall, Inc. (later renamed Advanced Decision Handling, Inc., or ADH). Surprisingly, perhaps, some of AT&T's erstwhile competitors were among ADH's early clients. Through those clients Hall built a reputation as an expert witness who could persuasively undermine the legitimacy of AT&T's monopoly. As a result, the Department of Justice hired ADH to provide evidence for its antitrust suit—specifically, evidence that Bell's rates were unreasonably high and that its justifications for those rates (e.g., ensuring reliability and innovation) were unfounded.

By the mid-1970s, therefore, Hall was in much the same position as Jack Kilby in chapter 5: he had a steady revenue stream from his patents and consulting that enabled him to remain independent of a corporate world that he had come to hate. Like Kilby, Hall could turn his attention toward technological projects that he was passionate about and that held some promise of revolutionizing an entire industry. In Hall's case, those projects related to the farm that he had bought in the early 1960s and to which he applied his expertise in sensors, communications technology, and automation. Out of those efforts grew an idea for an automated agricultural *system* (in the systems engineering sense) called *Autofarm*. Having received a 102-page patent for Autofarm in 1977, Hall spent the rest of his career unsuccessfully seeking government and corporate investment to develop it for use in the United States, Canada, Israel, Saudi Arabia, Poland, and elsewhere.[3]

Like the other people and organizations presented in this book, Hall's case is not representative of American physical and engineering scientists, nor even of that community's square members. He ascended to relatively elite positions within the corporate world and in his professional society (the IEEE), and he had some influence in government, though not as much as he would have liked. His career path was probably messier than average for his time, though in that sense he was a harbinger for the mobile "venture labor" of the 1980s and beyond.[4] Although we've seen many instances of interdisciplinarity and field-hopping in this book, Hall was an extreme example: not only was his model of systems engineering highly interdisciplinary, but also his enactment of that model in his consultancy and especially in his attempt to revolutionize agriculture makes him an outlier.

So Hall is not representative, but he is illustrative. He took the trends examined in my other case studies to new extremes and thereby casts further light on them. In addition, his peripatetic movement through industry, academia, government, professional societies, and an independent consultancy allows us to reexamine all the domains covered in other chapters by tracing his complicated career trajectory. Moreover, the fact that Hall's papers are housed in a single collection (at the Hagley Museum and Library in Wilmington, DE) means that his individual outlook on all those domains and trends can be grasped synoptically.

Thus, examining Hall's career helps me to summarize points I've made in the previous chapters. His example also helps to fill in some of the gaps left by my other case studies. In particular, his case offers some perspective on institutions of the East Coast establishment: Ivy League universities, professional societies, and giant regulated monopolies such as AT&T, IBM, RCA, and General Electric. These institutions have not figured much in this book thus far (though I have written about them extensively in my previous monographs). In the long 1970s they still played an important role in American technoscience—not just through the numbers of scientists and engineers that they employed and the technologies that they were associated with but also through their outsize influence in political and business discourse. That influence waned sharply in the 1980s, but we need to understand what came before then. Hall gives us at least some clues to how people who were associated with these institutions experienced the long 1970s.[5]

Thus, examining Hall helps me expand the book's regional scope somewhat. Some readers will justifiably complain that this book's cases focus heavily on events in California and Texas. Hall, however, lived and/or worked in New Jersey, Pennsylvania, and Maryland, so he—along with the professional societies examined in chapter 1 and Corning Glass Works in chapter 6—brings the Mid-Atlantic states somewhat into the picture. With a little stretching that means the only regions missing from my study are the Southeast and the vast "flyover country" stretching from Ohio to Colorado. I have little confidence in any claims I could make about the former; the politics of race and postwar industrialization in the Southeast are not fully comparable to other regions, even the Texas of chapters 4 and 5. We need more work on the history of technoscience in the South during the groovy era, which this book cannot supply.[6]

I am more certain that one could find close Midwestern analogues to this book's case studies. To a small extent that's because a surprising number of figures in my other chapters hailed from the Midwest and, for the most part, received their initial technical training there: David Phillips, Virgil Elings, and Robert Noyce (Iowa); John Linvill (Missouri); William Magruder (Illinois); Jack Kilby and Glen Culler (Kansas); and Stanton Glantz (Ohio). There were enough continuities between the Midwest and the regions that are spotlighted in this book that someone like Kilby, for instance, was able to move effortlessly among Kansas, Illinois, Wisconsin, and Texas. More importantly, we have reasonably good evidence from other studies that at least some Midwestern research organizations made the same turn toward interdisciplinarity, inclusivity, and socially relevant applied research during the long 1970s as did their East and West Coast peers.[7]

Still, it's fair to critique this book's regional focus. We need to know much more about American science and engineering in the interior of the country—not just for the long 1970s but also more generally. For an exploratory study of square scientists and engineers in the 1970s, however, California, Texas, and the Mid-Atlantic offer a reasonable microcosm. As noted already, the Mid-Atlantic was home to many of the Establishment institutions—such as AT&T and the APS—that saw their authority ebb over the course of the 1970s. California and Texas, meanwhile, benefited disproportionately from the early Cold War expansion of America's R&D capacity and therefore needed to make greater adjustments during the deflating bubble of the 1970s. We should thus expect that the contrast to the early Cold War was sharper in those states, though similar in form to that seen elsewhere.

California and Texas also represent opposite ends of a not wholly inaccurate spectrum in the American imaginary. That is, if I can show that squares were important actors even in supposedly groovy California, *and* that countercultural concerns were on the R&D agenda even in allegedly square Texas—well, then there's good reason to think that my findings will be robust for other regions too. That squares like Hall, moving among the Establishment institutions of the Mid-Atlantic, saw and did the same things as their peers in California and Texas lends further plausibility that my claims might be valid across other regions. Indeed, my main claim with respect to regional variation is that it didn't matter that much. Squares had to adapt to (and create!) local conditions, of course, but most of the situations that I've shown squares contending with were local manifestations of national or even global issues. Arthur Hall, working from his farm in Port Deposit, Maryland, was more tied to a specific place than the other actors in this book; but even when he tried to make very local interventions, he mostly had his eye on how his technical expertise could solve the problems of the nation or the world.

COMMON BEGINNINGS

The case of Arthur Hall helps me, then, to broaden the applicability of at least some aspects of my argument. Hall's main role in this chapter, though, is to embody almost all the trends I have surveyed in the other chapters. Despite his individual eccentricities, he stands in remarkably well for American square scientists and engineers in general. In what follows, therefore, I will gradually lay out different aspects of Hall's adaptation to the circumstances of the long 1970s while relating each of those aspects to examples from the other chapters.

Let us start, then, with the military–industrial "roots of the squares" that I examined in my introductory chapter. If we look across this book's case studies, one of the first things to notice is that nearly all the actors started from a similar place: a career immersed in the military–industrial complex. Some (e.g., James Meindl) were directly employed by the military at some point in their careers. Others (e.g., the NASA engineers in chapter 4) worked for government agencies that shared aims, technology, vendors, and personnel with the military. Others worked for those vendors, supplying hardware (e.g., Jack Kilby and TI) or knowledge (e.g., Philip Wyatt and Defense Research Corporation) to the military. Some (e.g., Calvin Quate) performed academic research supported by grants from military and other national security agencies. Most of the people profiled in this book moved among a variety of different ties to the military over the course of their early careers. Meindl, for instance, worked on nuclear submarine reactor controls for Westinghouse before joining the Army Research Lab at Fort Monmouth, then moved to Stanford, where his research was supported by the DOD.

Like Meindl, Hall moved from one form of entanglement with the national security state to another, both before and during the long 1970s. His most notable achievement—*A Methodology for Systems Engineering*—was written because "during the Korean War, the Bell Laboratories . . . due to its increasing military contracting and civilian business, experienced a severe shortage of systems engineers."[8] This led Hall's boss to ask him to figure out how to train systems engineers quickly, without the need for lengthy apprenticeships—and "thus was born the world's first formal course in systems engineering."[9] After leaving AT&T in 1966, he became vice president of two different companies (Jerrold Electronics and SCM) that made electronic equipment for the military. After forming ADH, he continued to serve military clients such as the Defense Communications

Agency (today known as the Defense Information Systems Agency) and give speeches for military organizations such as the Tactical Air Command.[10] He also envisioned adapting the Autofarm system for military applications such as "detection of chemical or biological agents"—an idea that he asked the DOD Small Business Advanced Technology Program to fund in 1981.[11]

Readers should note that direct and indirect interactions with the military–industrial complex such as Hall's didn't particularly set square physical and engineering scientists apart from their peers who were less square or who were affiliated more with the life or social sciences. Many American scientists and engineers found it hard to *avoid* working within the framework of the national security state in the 1950 and 1960s. Few did try to avoid it, at least not until the beginning of the long 1970s. Even those who were first to push *against* the national security state did so from a position of prior support *by* the national security state. For instance, Linus Pauling (a chemist) and Barry Commoner (a cell biologist) both became widely recognized public figures in the 1950s because of their opposition to nuclear testing, yet both had also conducted defense research during the war.[12] After the war, engineering and physical scientists probably were *somewhat* more beholden to the Pentagon than practitioners in the life and social sciences, but not by as much as one might think.[13] Even Noam Chomsky's linguistics research was supported by the Pentagon![14]

So the national security state hardly discriminated among science and engineering disciplines: it coopted them all. The Cold War may have had more of an asymmetrical effect, however, depending on the *kind* of science or engineering that one did. In particular, the national security state was possibly a more pervasive presence in the careers of scientists and engineers who conducted research that was intended for publication (in some form) than among those who didn't consider publishing. As the author of an influential textbook and founding editor of *IEEE Transactions on Systems Science and Cybernetics*, Hall fell quite easily into the former category, as do almost all the major figures in this book.

Even the industrial scientists and engineers that I've examined—people such as Jack Kilby who were more engaged in development rather than research—occasionally wrote technical articles and frequently published patents.

Admittedly, this is quite an imprecise criterion for demarcating the kinds of people who appear in my study from those who don't. Despite that imprecision, though, it is important to reflect on what we gain and lose by focusing on people who were more oriented to publication—as opposed to, say, scientists and engineers working in quality control laboratories or municipal water districts. In this study I look mainly at scientists and engineers whose work involved (semi)published research because those are the people who figure both in earlier historical studies of socially responsible research in the 1970s and in present-day enactments of responsible research and innovation—that is, by focusing on this class of people I can more directly intervene in both the historical and RRI literatures. Also, like most historians, I use documents to reconstruct the past, so my work is slanted toward historical actors who generated large volumes of relatively accessible documents.

Yet those reasons for preferring actors who publish over other kinds of technoscientists invite fair and serious criticism both of RRI and of my and other historians' studies. Published articles and patents—and the people credited as authors of such documents—represent only the smallest tip of the technoscientific iceberg.[15] The extent to which personnel involved in R&D who publish do or don't resemble their colleagues who don't publish should be an important question for future study. Nevertheless, we can learn a lot about the evolution of the American technoscientific enterprise even if we limit our scope to physical and engineering scientists who were doing research that could be published. Later in this chapter I will try to show what this group can tell us about present efforts in RRI. For now, though, let us stick with what this group of people can tell us about the long 1970s.

From about 1967 on, these people had to make hard choices about their relationship to the Cold War. The national security state's pockets were no longer deep enough to support all of them in the manner to which they were accustomed, and the politics of military support were becoming more complicated. As a result, some physical and engineering scientists moved toward positions that were highly critical of American militarism. Some of those critics, such as Chomsky or Carl Sagan, became well-known public figures about whom historians have written a great deal.[16] I've focused, however, on the much larger group of people who didn't seek or gain public prominence or whose feelings about the national security state were more mixed. As we saw in chapter 5, though, squares and vocal critics mutually regarded and constructed each other. It's difficult to understand the public roles of critics like Sagan if we don't also look at squarer, more reticent scientists' attitudes toward publicity seeking and avowedly activist research. At least some of the backlash against Sagan's support for the nuclear winter hypothesis, for instance, was framed as suspicion that Sagan was chasing a story that fit his very public politics but wasn't justified by the facts.[17]

Even so, public intellectuals like Sagan were not exactly alone. Many less visible but still influential figures also became more skeptical of the national security state in the 1970s. For instance, that era saw the formation of a cadre of physicists—including Hans Bethe, Richard Garwin, and Herbert York—who opposed antiballistic missile (ABM) defense despite their extensive ties to the Pentagon (which generally promoted ABM).[18] At least some of those who cautiously turned away from their earlier military patronage in this period linked their newfound criticisms of defense policy to their growing interest in "socially responsible" science. Bethe, for instance, was one of the relatively few physical scientists to take part in the National Teach-In to debate the Vietnam War in May of 1965; although Bethe was not "a supporter [of the war] but, at that time, he was not an active critic either," by the end of the long 1970s he was more open in his criticisms of defense policy.[19] Similarly, Garwin—one of the inventors of the hydrogen bomb—had, by the late 1970s, made such contributions to arms control that he received the APS's Leo Szilard Award and was made chair of its Panel on Public Affairs—although he was still such a square, behind-the-scenes player that the tag line of his biography is “the most influential scientist you’ve never heard.”[20]

A similar example is Harvey Brooks, a semiconductor physicist at General Electric and then Harvard; a member of the President’s Science Advisory Committee under Eisenhower, Kennedy, and Johnson; and a president of the American Academy of Arts and Sciences. Although publicly politically reticent all through the 1960s, by 1970 Brooks had reached his limit. As he wrote to Norman Hackerman, chair of the NSB,

> I have decided to associate myself publicly with two groups working against present administration policy in regard to the war in Southeast Asia. . . . I thought rather hard before taking this step. As you know, it has been my past policy not to take political stands, and I have always refused to associate myself with such groups as “Scientists and Engineers for Johnson,” etc. I have always felt in the past that it was a mistake for scientists, as scientists, to identify themselves with political causes, especially those associated with particular political parties or candidates. But I feel the present situation is different and that the country is in such deep trouble that for the first time in my mind the costs of silence outweigh the costs of tarnishing the public image of science through political participation.[21]

At exactly the same time, Brooks was becoming one of the leading advocates of the new field of “technology assessment” (TA). Specifically, Brooks promoted a form of TA that would be inclusive of public opinion and reflexive about its own positioning and impact.[22] Reflexivity and inclusivity are two of the principles of modern RRI; as I’ll discuss later, advocates of RRI today often cite TA as a predecessor. Thus, as Danielle Shanley has shown, Brooks should be seen as one of the forebears of RRI; and he came to that status for reasons that are difficult to separate from his rejection of the Vietnam War and of his earlier political quietism.[23]

MIXED MESSAGES

If the Vietnam War pushed some people to the (broadly defined) left, however, it also pushed some to the right. Naomi Oreskes and Erik Conway’s study of a small but influential ring of climate denialist physicists, for instance, identifies that group’s sense that colleagues betrayed them over

Vietnam as a starting point for the denialists' long drift toward political lobbying and lucrative corporate contracts for research undermining the scientific consensus on a range of issues.[24] Interestingly, one of the denialists' main opponents in the debate over whether cigarettes cause cancer, the antismoking activist Stanton Glantz, appears in this book as a Stanford aeronautical engineering student and advocate of socially responsible engineering. Between them, Glantz and his denialist foes illustrate American scientists' and engineers' divergent responses to the war.

We know a *little* about why denialists like Fred Seitz moved right and idealistic students like Glantz moved left—but we don't know nearly enough. Simply invoking "Vietnam" is only a partial explanation, because people reacted to the Vietnam War in many different ways, and because attitudes toward the war were entangled with other technoscientific questions of the day like ABM defense and the supersonic transport.[25] We need studies that parse the motivations of people like Glantz and Seitz as well as even more radical figures such as Charles Schwartz; we also need studies of the interactions *among* those strands of American science and engineering during and after the long 1970s. This book isn't that study, though. What this book does instead is examine just the people who were ambivalent or publicly quiet about their positions on the war and the national security state more generally—that is, I exclude actors on the ideological flanks, such as Seitz and Schwartz, and concentrate on the more reticent or hesitant group running from Glantz on the left to Jack Kilby on the right. This group's attention and goodwill were resources that were fought over by left and right, hawk and dove, square and groovy.

True, at times, some of this book's actors' views were—or appeared to be—discernible to a wider public. For instance, Stanford administrators and prowar faculty members regarded Glantz as a troublesome peacenik because of his campaign to compile statistics on Pentagon support for campus research. A closer look at Glantz's writings, though, shows that he wanted to mediate between Stanford's warring factions, rather than declare one side right and go home. He also opposed calls to ban research support by the military, preferring instead that Stanford's engineers simply diversify their sources of funding to avoid relying disproportionately on the national security state. That is, whatever opinions were imputed to him, Glantz was an ambivalent square, not an absolutist of either left or right.

The same can be said of many of the figures in this study. Virgil Elings, for instance, signed a couple antiwar petitions, whereas James Meindl's crew cut was perceived by coworkers as declaring that he supported the war—yet both men led ideologically diverse research and teaching programs.[26] That is, even though they held broadly discernible views, their practice was accommodating to other square scientists and engineers, even those whose broadly discernible views pointed in the other direction. In this respect, Arthur Hall was once again fairly typical. Even though he left behind a large volume of published and unpublished records, I can't find any clear documentation of his views on the war. Circumstantially it seems likely that he supported it, though I also suspect that he had little trouble assimilating the US military's defeat into his critique of large government and corporate bureaucracies. In any case, Hall seems to have drawn ideas from people who were both pro- and antiwar. In his proposals for Autofarm, for instance, he approvingly cited Barry Commoner—a strident opponent of the war—as an authority on the costs of pollution and the need to replace fossil fuels with photovoltaic energy.[27]

The flip side of squares' ambivalence was their flexibility in accommodating to the times—because their commitments were light, most squares didn't strongly oppose either the things that were changing or the things that stayed the same. Sometimes cheerfully, sometimes grumpily, the people profiled in this book moved beyond military patronage and enlarged their outlook in the 1970s. The course that Hall taught at the University of Pennsylvania on Communications Technology and Public Policy gives some of the flavor of that accommodation: his final lecture was originally entitled "Communications and Military Interaction," but in revising the course in 1973 Hall amended the syllabus by simply crossing out "military" and replacing it with "social."[28] Similarly, he changed the line "Implications and applications for the military in education, identification, etc." to read "Implications and applications for the military, education, law enforcement, trade, etc. Public issues of monopoly, privacy, financing public uses, etc."[29] If military applications were out of favor and "social" ones were in, well, Hall could adapt—no problem.

As the latter syllabus revision shows, ambivalence did not equate to indifference or ignorance. Squares were keenly aware of where the public and their patrons wanted them to focus. Note the expansive list of topics covered elsewhere in Hall's revised syllabus: "techno-economic factors encouraging and limiting the use of communications in the military, in law, education, medicine, and international development, in order and justice, urban decay and rebuilding, pollution control, privacy, nation building, in peace, and in peace-keeping."[30] That list of topics overlaps considerably with Bill Gilchrist's list in chapter 6 or Rudi Kompfner and John Baldeschwieler's in chapter 3 or with the rosters of early 1970s projects undertaken at UCSB in chapter 2 or at NASA in chapter 4. There was near-unanimity among square technoscientists as to the list of societal problems that they were being asked to lend their expertise to in this period.

Yet when historians of American science and engineering in the 1970s have looked at the topics included on that list—mass transit, public housing, disability technologies, environmental monitoring and remediation, education, health care—they have usually treated them in isolation, rather than as part of a broader movement. Perhaps more contentiously, I would also argue that histories of this period have exaggerated the prevalence in the 1970s of topics that were *not* on this list—such as parapsychology research or the construction of high-energy particle accelerators—because those other topics were sexier or more scientifically prestigious than the ones that *were* on the list. Parapsychology and particle accelerators certainly merit historical attention, but not in isolation from attention to the socially relevant research topics of the day.[31] Indeed, we can't really understand parapsychology or high-energy physics of the 1970s if we think that their practitioners separated themselves from socially responsible research. True, some parapsychologists were obsessed with the spooky and exotic to the exclusion of more mundane topics; but at least some—for example, Elizabeth Rauscher or David Phillips—moved freely between parapsychology and topics on the “societal relevance” list. Similarly, some high-energy physicists thought that solving nature’s mysteries was sufficient contribution to society; but others, such as Donald Glaser or Virgil Elings, left the field to pursue projects of more immediate and obvious societal value.[32]

One reason that we should treat the topics on the consensus list collectively rather than separately is that there was an accompanying consensus that these topics should be tackled in similar ways. In particular, almost everyone agreed that all these problems required interdisciplinary approaches. Yet there was far less unanimity as to what interdisciplinarity meant or how it should be organized. Again, Arthur Hall’s Communications Technology and Public Policy course lends some perspective. The course was offered within the Systems Engineering department of the University of Pennsylvania’s Moore School of Engineering; the chair of that department, Kenneth Fegley, hoped that the course could help him forge interdisciplinary links to other parts of campus. Thus, in 1973 Fegley wrote to George Gerbner, dean of Penn’s Annenberg School of Communications, to say that “we are eager to have students from the Annenberg School of Communications who wish to obtain competence in the telecommunications area take graduate courses offered by our department.”[33]

This seems to have been a sincere invitation, since Hall’s course covered topics such as “cable television systems and the ‘global village’,” and “the graphic arts.” Fegley probably believed that Hall would be open to students of diverse backgrounds, because Hall’s own systems methodology combined engineering with approaches “more in the realm of the behavioral sciences, insofar as they deal with psychological and sociological aspects of individuals and of groups.”[34] Hall, however, objected to having nonengineers in his classroom. Thus, Fegley was forced to tell Gerbner that although he had

> anticipated that Arthur Hall would be able to accommodate students with diverse backgrounds. . . . He has advised me, however, that he believes it is necessary for

> students taking this course to hold a B.S. in science or engineering or the equivalent. To admit students with a B.A. in the humanities would, he states, be an experiment at the expense of the student.[35]

Despite that check on his hopes for interdisciplinary education, Fegley promised that "we will continue to work toward bringing students with various backgrounds but common interests together in telecommunications courses. We believe that the value of many courses to students whose background is engineering will be enhanced by interaction with students whose academic home is the Annenberg School."[36]

The tensions between squareness and interdisciplinarity can be glimpsed across the other chapters of this book as well. Take, for instance, one of the highly interdisciplinary clusters we encountered at Stanford in chapter 3: there, we met John Chowning (Stanford's Music Department) and Calvin Quate and Rudi Kompfner (Stanford's Electrical Engineering Department), as well as John Pierce (Bell Labs and later Caltech). Pierce, Quate, and Kompfner all knew each other from Bell Labs; indeed, Pierce and Kompfner were very close. But Pierce was also quite fond of Chowning, who had visited Bell Labs to learn computer music techniques from Pierce and Max Mathews.[37]

Thus, Pierce—who wrote science fiction under the penname J. J. Coupling—enthusiastically supported the groovy and interdisciplinary Chowning in his bid to obtain tenure at Stanford. As Pierce wrote to the Stanford Music Department, "I hotly (rather than warmly) endorse John Chowning's abilities and attainments. Chowning is one of the very few men (and fewer musicians) who has used the computer wisely and originally in producing music. I would put him among [the top] three."[38] In contrast, the much squarer Quate was, as an associate dean, apparently one of the people who blocked Chowning's initial attempt to secure tenure.[39] Kompfner, meanwhile, had been the manager who gave permission for Billy Klüver, a Bell Labs engineer, to collaborate with artists such as Jean Tinguely, Robert Rauschenberg, and John Cage.[40] Yet Kompfner had drawn the line at Cage and Klüver's request to use Bell Labs's horn antenna to pipe in "white noise from space [rather] than just placing a noise generator" on stage. It's not hard to imagine Kompfner's exasperation with Cage and Klüver's retort (paraphrased by Douglas Kahn) that "he did not understand that a 'ssssssss' is not just any 'ssssssss.'"[41]

And yet! Quate himself considered patenting a device that adapted James Meindl and John Linvill's Optacon technology so that it could convert printed music directly into digital sound.[42] Kompfner, meanwhile, was the son of a pop music composer in Vienna; he himself was an amateur artist and former professional architect. Those interests don't seem to have figured much in his work at Stanford or Bell Labs, but in his papers is preserved an achingly earnest semiautobiographical essay about music, technology, and cognition that he sent to Pierce.[43] That is, unlike cultural entrepreneurs such as Pierce, the squarer Quate and Kompfner were grumpy about hip nonengineers like Chowning and Cage elbowing their way into the traditional technological jurisdiction of engineering. Nevertheless, Quate and Kompfner didn't foreclose the possibility of crossover between engineering and the humanities, medicine, and other fields. Indeed, they sometimes facilitated or even participated in collaborations aimed at realizing those crossovers. It's just that for Quate and Kompfner, as for Arthur Hall, interdisciplinarity was best achieved by the scientist and engineer contributing to other fields—and *not* by practitioners of those other fields appropriating the resources or credit that they thought belonged to science and engineering.

COLLIDING WITH REALITY

So squares understood the way in which the world was changing around them, and they responded by gravitating to a widely agreed-upon list of topics of growing public interest, yet they were decidedly ambivalent about those topics and the nontechnoscientists associated with them. Almost the only thing that squares weren't ambivalent about was the relevance of their own expertise in solving the problems of civil society. They were confident that they could help and that they therefore deserved a seat at the table.

That belief stemmed largely from the squares' earlier success in national security contexts, which convinced them that the combination of Cold War technologies and methodologies could solve *any* problem. We can see that confidence, for example, in Hall's listing of all the problems of modern agriculture that Autofarm would solve at one go:

> [Autofarm's] objective is to bring a systems approach to the farm, together with technologies that have not heretofore had major or revolutionary affects [*sic*] on the farm, including electronics, communications, and space technology. The end results are to drastically increase the productivity per acre of farm land, and to . . . reduce soil, water, noise, and air pollution created by current agricultural technologies. Finally it will reduce, or eliminate, farms as consumers of fossil energy and will eliminate the farm as a dangerous place to work.[44]

Moreover, squares like Hall believed that along with Cold War "electronics, communications, and space" technology, they could and should also adapt the *institutional* apparatus of national security R&D that had made those technologies possible in the first place: institutions such as funding and oversight mechanisms, intellectual property protections, and revolving-door career trajectories in which personnel moved easily among industry, government, and academia. Hall, for instance, was a prolific patenter and proposal writer, and he vigorously circulated his patents and proposals through the network of colleagues whom he knew (or at least was connected to) through the IEEE, AT&T, Penn, the DOD, aerospace manufacturers, and other corners of the military–industrial–academic complex.

To be sure, sometimes the squares were justified in the belief that they could adapt Cold War technologies and institutions to solve civilian problems. We saw, in particular, that microelectronics technology successfully crossed over from the military to civilian realms, for example, in the scientific instruments that Virgil Elings and his students invented in chapter 2 or the medical technologies that James Meindl, James Angell, and others developed in chapter 3. Indeed, we saw in chapter 6 that microelectronics companies that *didn't* sufficiently transition from military to civilian markets were outcompeted by ones that did.

Hall's career contained some similar successes in bringing Cold War technology, expertise, and institutions to bear on civilian problems. His consulting work in the AT&T antitrust case, for instance, began with the production of two scenarios anticipating how the Bell System would function with and without being broken up.[45] As we've seen, scenario building was a technique that originated in the nuclear wargaming world of think tanks like RAND. At AT&T, Hall had incorporated RAND's simulation and game theory techniques into his groundbreaking systems engineering methodology. That combination of scenario building and systems engineering was apparently then the selling point of Hall's consulting firm, ADH: scenarios backed by systems expertise were what ADH offered to clients such as the City of New York, the National Basketball Association, and the NSF.[46]

But there were also many cases where square technoscientists' optimism was unfounded.[47] Hall's Autofarm system exemplifies that overreach spectacularly, so it's worth turning our attention to it briefly before coming back to the larger context in which it was embedded. As figure 7.1 shows, Autofarm consisted of a network of sensors monitoring "all important parameters in the homogeneous agricultural production area" (soil moisture content, presence of weeds or pests, weather, readiness for harvesting, etc.); an automated decision tree for responding to sensor data; and a physical apparatus that could be directed by that automated agent to go into the field or orchard and apply fluids (water, pesticides, herbicides, etc.), harvest and sort crops, prune trees, and so forth. The sensors, the automation, and the use of solar panels and fuel cells for powering it all—all these, as Hall acknowledged, drew on technologies and expertise developed for or by the space program and the military. Hall also borrowed technologies—such as Grumman's Sailwing Wind Generator and General Electric's Geniponics system—from military–industrial companies that were similarly adapting aerospace products for civilian markets in the 1970s.[48] Much like the NASA engineers in chapter 4, then, Hall was confident that space-age technology originally developed for a narrow purpose could be applied to complex and open-ended domains on earth.

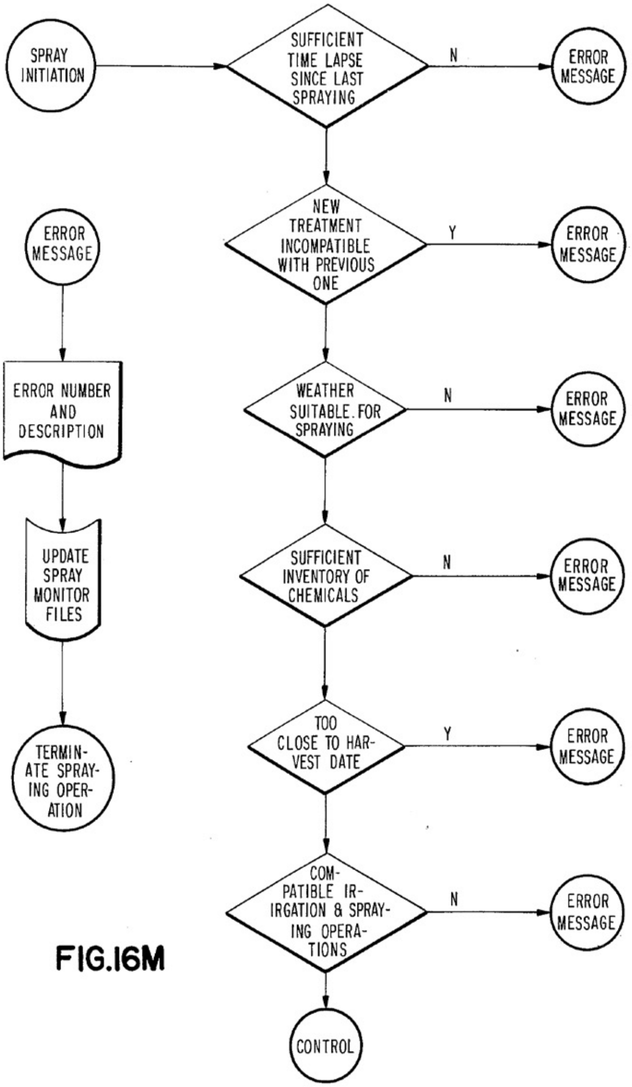

FIG.16M

Figure 7.1

Three of the figures from Arthur D. Hall's Autofarm patent, Highly Automated Agricultural Production System, US Patent #4,015,366, issued April 5, 1977. The figure on the *left* shows a decision tree governing spraying of herbicides, pesticides, and so forth. The figures on the *right* show the Autofarm machinery configured to prune (*top*) and harvest (*bottom*) fruit trees.

FIG.29C

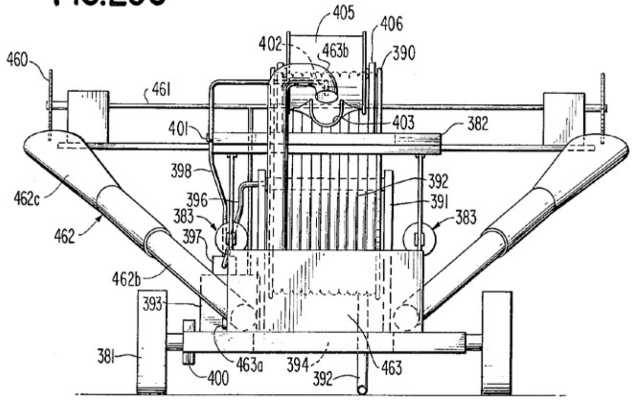

FIG.30A

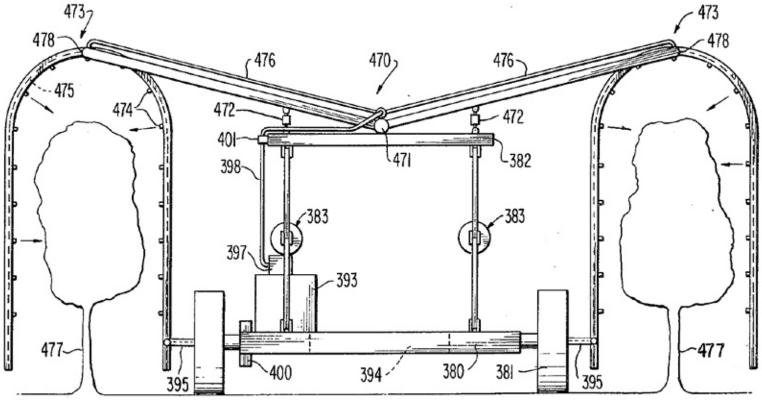

Figure 7.1 (continued)

Remember, though, that the engineers in chapter 4 abandoned their attempts to apply space-age expertise to terrestrial problems after just a few years. Likewise, GE abandoned Geniponics and Grumman the Sailwing Wind Generator; and most other aerospace companies that ventured into mass transit, alternative energy, public housing, and environmental remediation in this period did the same. Hall, however, never did abandon Autofarm—he continued trying to interest investors until the 1990s. The difference between him and NASA's, GE's, or Grumman's engineers, I think, was that Hall was a free agent—he was self-employed and therefore did not have a manager trying to redirect his efforts, and he could brush off external critics who thought that his ideas were unworkable. Hall's vision for Autofarm therefore shows us what the square technoscientific imagination could run to in the absence of constraints.

As it turns out, Hall's imagination could run very far indeed. At a minimum, he foresaw Autofarm replacing humans for a variety of agricultural tasks: monitoring crops and environmental conditions, treating crops in various ways, harvesting them, and preparing them for the market. Even today, only a few of these tasks have been automated to nearly the degree that Hall thought was already possible in the 1970s. But more than that, Hall believed that the components of Autofarm could be generalized to solve a wide variety of problems outside, or at least not normally associated with, agriculture. In a retrospective account he offered a list of those problem areas (here quoted in part):

> AUTOFARM Spin-offs and other uses for subsystems. . . .
>
> B. Automatic remote spectrometry, for water, soil, and air—pollution sensing, battlefield surveillance for poison gases, mineshaft sensing, etc.
> C. Audience response systems in theaters, schools, prisons.
> D. Water filtration and purification by automated biomass production. . . .
> G. Aquaculture: joint fish and biomass production for food and energy
> H. Automated gasifier using wood/coal/corn stover for [propulsion of] trucks, ships, and autos.
> I. Automated silviculture for renewable fuel production.
> J. Total environmental control for space stations, phytotrons, and greenhouses for food production, air and waste recycling.[49]

Hall never achieved *any* of these applications, at least not on the scale that he had imagined. Some of them were possibly within reach at the time, but others were (and are still) quite speculative. His confidence that military–industrial technologies of automation, systems engineering, power, and sensing could provide medium-term solutions to every civilian problem was, thus, profoundly misplaced.

By stepping through some of the items on Hall's list we can start to grasp the immense scope of his vision. Of course, for a few of these items, time has shown that scope to be justified. With item C, for instance, Hall had in mind a large distributed cybernetic feedback network whereby audiences (say, theatergoers at a play or students in a classroom) could shape the performances that they were viewing, or whereby system controllers (say, prison guards or teachers) could monitor the collective murmurings and nonverbal reactions of individual agents. Hall had proposed a theater-based experimental setup along these lines as early as 1968; that version was intended to help psychology and communications researchers (perhaps with applications such as advertising and commercial film and television in mind) to subject an audience to various stimuli and correlate those stimuli with the intensity of responses such as anger and joy.[50] Versions of this idea are today fairly common, for both liberatory and oppressive applications—though Hall seems not to have made such a distinction.

Most of the other applications on this list, however, are unrealistic even today. Items H and I, for instance, envisioned automated cultivation and harvesting of vast forests of monocultured hybrid poplars to be used as a replacement for fossil fuels. Indeed, in at least one proposal Hall painted a picture of autarkic farm families growing vegetable crops in greenhouses and poplars in timber lots, converting the poplars to biogas to power their living quarters and greenhouses, and skimming excess carbon dioxide from the gasification process to stimulate growth of the greenhouse crops.[51] He also foresaw growing potatoes in the greenhouses, and then converting their starch to ethanol to power the farm family's vehicles!

Yet even that highly speculative vision of autarky was far from the outer edge of Hall's imagination. As item J in his list indicates, he also thought that Autofarm could be used in fully closed systems—particularly on board permanently inhabited space stations—to endlessly recycle human waste back into food again. David Munns and Kärin Nickelsen have shown that this was a longtime (but unsuccessful) locus of military-funded square scientists' efforts in the early 1960s—efforts that never led to implementation in any actual space habitation.[52] The most extensive attempt at this kind of high-tech total autarky has been the earthbound Biosphere 2; and the failure of that habitat's closed-loop system resulted in starving, oxygen-deprived "bionauts" descending into paranoid anarchy.[53]

Finally, item D on Hall's list particularly exemplifies the strange politics of the squares' optimism that Cold War technologies could be applied to the problems of civil society. For a more detailed description of item D, let me turn to a letter that Hall wrote in 1981 to the chair of Maryland's Hazardous Waste Facilities Siting Board:

> Now, all these pleasant pursuits [on Hall's farm] have been disturbed by rumblings that this County may become the depository of toxic waste which nobody seems to want and everyone fears. My first inclination to the citizens was sympathy. My more considered response is that this County should actively promote its becoming the central depository for as wide a collecting area as is economically feasible, providing that it can assimilate and employ certain new technologies which I believe can in effect convert an unmitigated disaster into a blessing. In the past three years I . . . became a student of some brilliant research done almost exclusively by NASA, which established that certain subtropical water plants have the properties of being able to concentrate and store absolutely astounding quantities of heavy metals, phenols, and other toxic chemicals, while producing equally astounding quantities of cellulose biomass, its energy in turn being recoverable. . . . These studies led to my conviction that the prospective location of hazardous waste dumps in our County need not be a disaster as nearly all of our citizens are portraying it to be, but in fact can be an enormous industrial boon to economic development.[54]

That is, despite Hall's fairly conservative politics, he didn't follow the denialist path taken by other conservative scientists. Hall acknowledged, here and in many of his other writings, that pollution and global warming were real problems that citizens should be concerned about.[55] It's just that he thought Autofarm and other technologies inspired by the Cold War could solve those problems—indeed, could solve them so completely that citizens should *welcome* toxic waste dumps in their backyards![56]

MUTUAL AMBIVALENCE SOCIETY

To observers looking back from the twenty-first century, Hall's stance seem highly ambivalent if not contradictory. Hall's politics were not the clear-cut politics of left or right, but rather the ambiguous politics of the cybernetic system—with himself, or some other square scientist or engineer, imagined as the rightful system designer and controller. He was deeply opposed both to "authoritarian powers [such] as business corporations" and to a government and political system that (apart from the "folk heroes"—like himself—who broke up AT&T) was unwilling to curb the power of big business.[57] Yet at the same time, Hall foresaw the need for some system-level entity to "manage the environment, restore it, and live within its limits," if necessary by enforced "birth control and planned parenthood as one way to limit consumption of earth's resources. In work on this problem, systems engineers and methodologists can be the most valuable members of the team."[58]

Hall's views complicate the conventional American political spectrum—both now and in his own day. His politics were profoundly ambivalent about government, business, democracy, environment, and so on: he was promarket but antibusiness; progovernment but antibureaucracy; in favor of "freedom" but also in favor of coercive population control. That ambivalent mix of views is part of why Hall merits a chapter of his own in this study. Throughout this book, I've taken political ambivalence and/or reticence as *the* defining characteristics of the squares. Indeed, I hope that studying the squares can lead to a more nuanced understanding of ambivalence and the ambivalent in general. Most histories of science—maybe even most histories full stop—revolve around figures with pronounced views; we therefore don't yet have a good vocabulary for talking about those whose views were indistinct or muddled, even though such people often form a very large majority in any given setting.

To give a small example of what such a vocabulary might include, I would note that Hall's views offer an opportunity to distinguish between two different forms of ambivalence: ambivalence in the moment versus retrospective ambivalence. The former applies to squares who were ambivalent about the politics *of the time*—Harvey Brooks's reluctance to be associated with the antiwar movement or Bill Rambo's waffling about the value of interdisciplinarity are examples. The latter, meanwhile, applies to squares who appear ambivalent only in hindsight, simply because the apparent contradictions between their various views were only established later. For instance, Arthur Hall and Jack Kilby were enthusiastic supporters of alternative energy but almost certainly also backed Ronald Reagan's election as president in 1980—a position that would not necessarily have appeared as ambivalent until a few years later, once Reagan and the movement that he led had more clearly repudiated alternative energy. Both forms of ambivalence are important in understanding American science and engineering in the 1970s. The ubiquity of ambivalence in the moment shows us the lasting legacy of attempts to bind American science and engineering to an ideology of "apolitical" quietism; whereas retrospectively ambivalent figures such as Hall and Kilby show us how confusing the political landscape of the 1970s was to actors at the time (and is to its chroniclers now).

Both these forms of ambivalence characterize square scientists' and engineers' views of the world around them. But if we reverse our perspective and ask how the world saw the squares, then we find an almost equal measure of ambivalence. On the one hand, American technoscientists had many admirers; I've shown in my other case studies, particularly in chapter 4, that many organizations and social movements were eager to enroll square physical and engineering scientists in their projects. On the other hand, we've also seen that that cooptation often failed because the squares wouldn't or couldn't offer what their partners wanted. More generally, as we saw most clearly in chapter 3, Americans saw science and engineering as both the cause of and solution to many of society's problems. I've also occasionally noted instances of Americans' concerns that scientists and engineers were too narrowly specialized or too confident in their own abilities, and therefore that they could cause further problems if their technoscientific hubris were left unchecked.

The reactions of various observers to Hall's Autofarm system display the full range of these ambivalent attitudes. There were, indeed, a few who lauded it, at least on initial inspection. One US Department of Agriculture (USDA) official, for instance, called it an "exciting perspective of the agriculture we will see in the not too distant future."[59] A USDA economist, Leroy Quance, positively reviewed Hall's manuscript of a book about Autofarm for John Wiley & Sons, saying

> there are new unprecedented technologies on the horizon such as photosynthesis enhancement and bioregulators in crop production. Our analysis indicates that these new technologies will not have much of an impact before the year 2000. . . . But the computerization of agricultural production as exemplified by *AUTOFARM is the sleeper*. This technology already exists and only has to be put to use to . . . usher in, at a much earlier date than expected, such . . . technologies as photosynthesis enhancement and bioregulators in crop production.[60]

Hall seems to have been especially grateful for Quance's appraisal of Autofarm as a "sleeper," since he invoked it frequently in letters seeking investors for Autofarm.

Yet the response to those dozens of letters to companies and government agencies was almost uniformly negative. Most of the organizations that Hall approached simply replied that they didn't have the money to pursue his ideas or that they weren't in the business of automated agriculture—hardly surprising because, as we've seen in other chapters, the 1970s were a period of strained budgets and numerous failed crossovers from one industry to another. Some of these organizations, though, explained their rejection of Hall's requests with reference to negative evaluations from in-house experts in the fields that would've been overtaken by Autofarm. Almost none of those experts were impressed. For instance, FMC Corporation, a chemical and agricultural technologies company, cited this assessment in declining to invest in Autofarm:

> The material furnished me regarding Mr. Hall's ideas in regard to his "automatic orchard" was very interesting, but I am somewhat concerned as to whether or not such far-reaching changes can be implemented in our lifetime. . . . I believe that Mr. Hall's greatest contribution will be that he has certainly let himself think boldly and without limitation, and as a result we must admit that there is some merit to some of his suggestions, although I cannot believe that the implementation of any great number of these items would be economically feasible in the foreseeable future. Although I am intrigued with [Hall's] overall proposal, I must return to the cold, hard facts of life and advise you that I can see no practical manner in which we can implement his suggestions into marketable products. Many of his suggestions have already been tried and have not met with market acceptance.[61]

As we've seen over and over in this book, square physical and engineering scientists' optimism crashed on the reality that "problems are much more complicated than meets the eye." Often, other people had studied those problems in depth before the squares got there, had better claims on those problems than the squares did, and therefore rejected the squares' interventions.

Some squares took complication and rejection well, but many became frustrated, leading to anger and/or disaffection. Some were irritated from the start but accommodated nonetheless—we saw that in the contrast between William Rambo's public and private comments on interdisciplinarity in chapter 3. Others gave reform a chance but came to believe that it had been a mismanaged debacle. Again from chapter 3, Calvin Quate's praise for the NSF's cancellation of its RANN program and his retreat from attempts at collaboration with biomedical researchers show where such frustrations could lead. Others didn't become angry so much as they lost interest in socially relevant projects, or they gave up such projects once they no longer thought that there was any pressure or incentive to do them. From chapter 4, the disappearance of NASA projects that were oriented to terrestrial social problems largely falls into this category.

With Hall, frustration was often tinged with anger. For instance, on hearing that his proposal to the RANN program had been denied because of budget cuts, Hall expressed his irritation to NSF thus:

> I am particularly disappointed at this outcome since last year the preliminary proposal . . . and this year the formal proposal [were] solicited. . . . and since the preparation and the printing of this proposal has been very expensive. . . . [NSF] could have told me frankly at the outset that there was no money for the proposal even if it should prove directly responsive to . . . national goals. . . . I must say that I do not think NSF has given proper consideration to this proposal of its merit by people who are professionally qualified in the field of agriculture. So that it is a matter of record that some individuals who are professionally qualified in the field of agriculture have seen the proposal and think it is sound, attached herewith [are letters of support from the Agricultural Research Service].[62]

Elsewhere, Hall showed a certain self-awareness that his irritation might be counterproductive. In his answer to a rejection letter from a subsidiary of the Atlantic Richfield oil company, for instance, he acknowledged that he was responding partly "to unload some of my frustrations" and to "show I'm at least not some overalled farmer nut, but instead am a practicing engineer-scientist with a few practical results to his credit."[63]

Rejection is never nice, of course; but the particularities of Hall's and other squares' responses to rejection tell us something important about how the American science and engineering community changed during, and as a result of, the long 1970s. On the one hand, people like Hall, Quate, and Rambo did have *something* to legitimately complain about. The reforms of the long 1970s were often thrown together hastily, and cut back or cancelled with just as little warning. The Nixon administration's short-lived New Technology Opportunities (NTO) program and the NSF's somewhat more enduring but extremely fickle RANN effort both illustrate the disorienting effect of rolling out new, experimental initiatives and then unexpectedly rolling them back up again.

On the other hand, some squares' complaints reflected an unjustified attitude of entitlement. For this group, American scientists' and engineers' contributions in World War II and the Cold War had made R&D funding a right—a right that the reforms of the 1970s took away. Not all squares adopted this view, of course. Some took the reforms of the 1970s in stride: they recognized that the existential need for scientists and engineers to help America win the Cold War had passed, so resources for R&D would necessarily be thinner in the future. For this latter, more fatalist, group, either you could accept that you had to do more with less or you could reinvent yourself in a new line of work; one hallmark of the era was the sizable number of people who left science and engineering, either temporarily or permanently. But there were plenty who, like Hall, took a less magnanimous position: that the country owed scientists and engineers *more*, not less.

Thus, Hall's response to rejection at home was to inflate his ambitions even further; and when he couldn't find anyone at home to support those inflated ambitions, then he started to look abroad for patrons who would. If the US government and American firms didn't want to fund him because their experts didn't think that he could squeeze a little extra productivity out of wheat fields and apple orchards, well, then the governments of Canada, Egypt, Israel, Kuwait, and Saudi Arabia would beg him to turn tundra and desert into productive farmland![64] And if no one wanted Autofarm for growing wheat or apples, well, then he would find someone who needed it for disposing of toxic waste and replacing coal and oil in the energy supply!

That kind of inflation often succeeded in the early Cold War: if no one wanted your nuclear airplane, well, offer them a nuclear spaceship instead; if no one wanted your analog flight simulator, well, convince them that they really needed a general-purpose real-time digital computer; if no one wanted your ground-based ABM system, spin a vision of space-based missile defense replete with X-ray lasers and "brilliant pebbles."[65] But in the long 1970s that kind of Cold War gigantism hit its limits in North America and Western Europe.[66] In Saudi Arabia, though, one could still talk of towing icebergs to the Middle East and turning the desert into a breadbasket—which is why people like Hall and Kilby sought Saudi help when rejected at home.[67]

Admittedly, Cold War gigantism did make a partial comeback in the United States with the election of Ronald Reagan in 1980. Like many voters, therefore, Hall believed that Reagan would restore his and his peers' supposedly forfeited rights and allow the nation to get beyond the failed experiments of the 1970s. As he wrote in late 1980 to a conservative Congressman close to the Reagan transition team,

> Because of my conviction that our government needs all the competence it can get, and because I know that, at least in the areas of my expertise, it has suffered from incompetence, it has seemed to me that I should offer my talents to the new administration.[68]

In an attachment, Hall noted his political ambivalence and reticence: "I have not been a political person, although I have generally voted and provided financial support for Republicans."[69] But now, "concerned about the future of our country," he asked that Reagan's transition team consider him for eighteen different jobs, including Director of ARPA and Secretary of the Army!

Needless to say, despite his optimism about Reagan's election, Hall was not appointed Secretary of the Army—or to any other position in the Reagan administration. We saw something similar in chapter 5, where Jack Kilby was hopeful that Reagan's people would cleanse the DOE of its hippies and incompetents, leaving the field clear for the government to support TI's solar technology—but in the end the new administration just abandoned solar power entirely. Likewise, Signetics opened a new plant for military products in 1981, in anticipation of Reagan's defense buildup—only to find that military contracts weren't as lucrative as they had been in the 1960s. In other words, for some squares who had prospered in the early Cold War and who had become frustrated with the experiments of the 1970s, Reagan's gauzy nostalgia and promises to boost defense spending must have seemed like a lifeline—yet in the end the new administration disappointed them by instituting a new neoliberal order rather than a return to the early Cold War. For both the squares who voted for him and the ones who didn't, Reagan therefore offered yet another reason for frustration and ambivalence.

RRI'S ANTECEDENTS IN THE LONG 1970S

With Reagan's election we come to the end of the long 1970s. We now have everything we need to survey square scientists' and engineers' trajectories through that decade; and from there, we can start to relate the 1970s to today. In particular, we can use the 1970s to evaluate current efforts to convince square scientists and engineers to adopt a more "responsible" form of research and innovation.

If we step back and look at the careers of the squares who've appeared in this book, we see that "responsible" research and innovation—in at least a broad sense—flickered in *and out* of square technoscientists' lives. Most of the people I've profiled in this book traveled a roughly similar arc with respect to social responsibility: they started the 1960s in (and many maintained long-term loyalty to) the military–industrial complex; in the early 1970s they made forays into interdisciplinary applied research directed to civilian social problems; and by the 1980s most of them had become frustrated and lost interest in—or were channeled away from—those problems. Surprisingly, perhaps, with the end of the Cold War, many squares returned to the kinds of projects that they had pursued in the 1970s. We can see that arc in the rise, fall, and return of interest in biomedical applications among Stanford's electrical engineers. We can see it in Virgil Elings's drift away from AEC funding and toward environmental and biomedical research in the early 1970s; his turn toward entrepreneurship and selling tech to the semiconductor industry in the 1980s; and his final transformation into a philanthropist (focusing in particular on educational innovation) in the 2000s. And we can see it in NASA's meandering path from a tool of Cold War prestige to a pawn of détente, back to an auxiliary of the national security state under Reagan, and once again a facilitator of peace and post–Cold War cooperation in the 1990s.

In other words, square technoscientists' relationship to "societal relevance" and "responsible research and innovation" has been highly nonlinear. Every era in which the talk and practice of "social responsibility" has gained favor has given way to an era in which it has been rejected—and vice versa. This is perhaps not so surprising, but it does run counter to the discourse of the present-day field of responsible research and innovation. Practitioners of RRI seek to make technoscience *more* responsible; some even propose that by exposing scientists to RRI principles early and often enough they could cause a permanent shift toward more responsible behavior.[70] That possibility should of course be explored. But the possibility that scientists and engineers might choose to become *less* responsible is hardly ever mentioned in RRI studies. Yet scientists and engineers do regularly make that choice, and RRI practitioners therefore need to understand what makes scientists and engineers (and their organizations) more *and less* responsible. What are the conditions of possibility of, and the incentives for, adopting, ignoring, *and discarding* the kinds of principles and practices advocated within RRI?

One obstacle to answering this question is RRI's impoverished sense of its own history—RRI practitioners simply aren't aware of the several decades of moves toward *and away from* something like responsible R&D. Admittedly, many programmatic overviews of RRI do contain a brief potted history of the field, often starting with the postwar "social contract for science" and then hopping from the antinuclear movement to the Human Genome Project, mad cow disease to nanotechnology.[71] And those potted histories are correct as far as they go—but they don't fully acknowledge that previous attempts at something like RRI have failed, or at least have given way to periods that were much less encouraging of responsible research and innovation. These potted histories therefore avoid the inconvenient fact that the discourse of responsibility is only intermittently persuasive to scientists and engineers, and therefore that discourses of *irresponsibility* are persuasive the rest of the time.

Folk histories of RRI also often put forward a skewed vision of what the field's predecessors were actually up to and the context in which those forerunners operated. Take, for instance, a recent debate over whether RRI represents an extension, a critique, or a "travesty" of the field of technology assessment.[72] The answer to that question hinges in part on whether technology assessment was or was not exemplified by the US Congress's Office of Technology Assessment (OTA), which supposedly "did not include an emphasis on public or stakeholder participation."[73] Yet that characterization of the OTA is only partially accurate. True, as the OTA institutionalized over time, it did indeed become more technocratic and less public facing.[74] But when the former Congressman Emilio

Daddario founded the OTA in 1972, he imbued it with an ideal of public participation—that is, at its founding the OTA was decidedly *not* a place where "it was generally assumed that experts' knowledge dedicated to the study of intended and unintended impacts of new technologies would be sufficient to provide political representatives with useful tools to govern science and technology."[75]

Moreover, at the time the OTA interacted with a number of other institutional experiments—such as the RANN program and the NTO initiative—that were similarly dedicated to supporting more responsive R&D. RANN's links to the OTA, in particular, were quite close: while he was a Congressman, Daddario had offered the amendment that led to the formation of RANN's predecessor, the IRRPOS program.[76] These connections were well understood at the time; in a speech in early 1972, for instance, Arthur Hall lumped NTO, OTA, and RANN together as common symptoms of "a great concern, that has been building over the past few years, for the need to forecast and to assess the threats and opportunities of new technologies, and to sense and measure emerging societal needs before they reach crisis proportions."[77]

Yet the variety of institutional manifestations of that concern are not widely known today. Experiments like RANN and NTO were more short lived than the OTA and therefore have mostly disappeared from RRI's historical memory. Nor have historians of science and technology done much to keep the memory of RANN and NTO fresh. Thus, RRI practitioners are not very cognizant of their field's *failed* forerunners and are even less aware of the reasons for their failure. Few contributions to the RRI literature, for instance, draw lessons from the fact that scientists and engineers such as Calvin Quate came to loathe RANN and rejoiced at its demise.

The OTA, RANN, NTO, and other programs—and the field of technology assessment more generally—also interacted at the time with a number of other scientific and intellectual movements that have generally been excluded from RRI's genealogy despite their relevance to any discussion of responsible technoscience.[78] As Jessica Smith has shown, for instance, the Corporate Social Responsibility (CSR) movement was forged by mining engineers who absorbed positive lessons from their engagements with the public in the 1970s.[79] Likewise, the appropriate technology movement has been written out of the history of RRI, even though AT directly influenced national laws (in the United States and elsewhere) and supranational policies (of the United Nations as well as nongovernmental organizations) requiring technoscientific projects to account for their social and environmental impacts in a manner not dissimilar to RRI today.[80] Appropriate Technology was also widely adopted as a label for funding streams that supported the same kinds of projects that RANN and NTO fostered at the time and that RRI might encourage today. For instance, the DOE had an Appropriate Technology Small Grants Program that Arthur D. Hall applied to in 1981 to support development of Autofarm.[81]

Early advocates of TA such as Brooks and Daddario frequently appeared on the AT conference circuit and adopted some AT ideas. So if TA can be taken as an antecedent of RRI, then AT surely should as well—because early on there was no clear line between the two movements. And as Danielle Shanley argues, if we look at the shared ideals of *early* TA and AT, then we can see that both movements *did* "include an emphasis on public or stakeholder participation" that some RRI practitioners believe is original to their own program.[82] Appropriate technology in particular had a widespread grassroots appeal—indeed, it attained a degree of grassroots participation that RRI advocates today can only dream of. Appropriate technology wasn't just open to the public—it was *of* the public.

To take a parochial example, my hometown of Lawrence, Kansas, was also home to an Appropriate Technology Collective that among other things pressured the city and county government to adopt recycling and energy conservation measures, and joined other groups in opposing construction of the nearby Wolf Creek nuclear power plant.[83] As their newsletters and meeting notes show, the Douglas County AT collective also made common cause with Indigenous activists, church groups, astrologers, small farmers experimenting with alternative energy technologies—you name it. In putting into practice an ideal that it is "important to involve key groups in community and make it a democratic process," AT set a standard for public participation that RRI has yet to catch up to.[84]

Unfortunately, groups like the Douglas County AT collective had little success in bringing square scientists and engineers into their democratic process. Such groups formed part of the environment in which the squares in this book operated, but none of the archival collections that I have examined contain significant examples of practicing scientists and engineers who collaborated with grassroots AT organizations. No doubt there are examples out there, but they can't have been common. What I have run across, however, are examples of square scientists and engineers who rejected collaboration with such organizations and who were disdainful of open and idealistic forms of innovation. Squares were willing to adapt to the times and converse with other square scientists and engineers of (moderately) opposing views; but they demanded deference to their technical expertise and were often suspicious of potential collaborators who did not share the squares' backgrounds in Cold War modes of R&D.

Part of the tragedy of chapter 5, in particular, is that TI and Jack Kilby declined to seek allies among, say, the readers of the *Small Farm Energy* newsletter (an appropriate technology-oriented monthly to which the Douglas County AT collective subscribed)—preferring, disastrously, to look to the national security state and wealthy homeowners instead. Of course, even if Kilby and TI had wanted to engage with *Small Farm Energy's* readership, it's hard to see how that newsletter's definition of "appropriate" technology would have afforded engagement with a giant, profit-seeking military–industrial company: "Appropriate alternative energy is characterized by the Energy Project as being home built, easy to manage and maintain, built with recycled and locally available materials, low-cost, and cost-effective."[85] The middle, as throughout this book, was largely excluded.

Not entirely excluded, though. Arthur D. Hall, for one, was willing to reach out to people who had similar goals but different approaches. For instance, in 1981 Hall wrote to Robert Rodale, an influential publisher of books and magazines related to health, wellness, and organic farming, to highlight both the differences between their visions and the potential common ground on which they could agree:

> I fully agree about the need for another agricultural revolution comparable to that by Jethro Tull. Because of our different backgrounds, we differ on the choice of specific technologies, government interventions, and other means of bringing it about. For instance, I think we cannot do without modern chemical technologies for pesticides, fertilizers, growth enhancers, etc. However, by specific sensing of plant needs, I think we can use only a fraction of the chemicals we do now. . . . Doing this calls for using information and computer science which heretofore have been used almost not at all in agriculture. . . . [R]eal (peaceful) revolutions are generally not wanted, even if they are impecably [*sic*] sound technically and economically. The . . . USDA rejected my revolution on the mistaken and foolish grounds that it would save labor, and they were enforcing a policy of not encouraging any labor-saving technology.[86]

Yet even if Hall could recognize the common ground between himself and Rodale, he couldn't imagine building anything there. Instead, Hall's natural allies were the same as Kilby's. Like Kilby, Hall was a product of the national security state and of big business, and it was among those institutions that both of them felt comfortable even after they became disillusioned and left big business to become their own bosses. Hall therefore sought support for Autofarm from national security pillars like the DOE, the Army, and the Battelle Memorial Institute (one of the main contract operators of the DOE's National Labs), as well as corporations connected to the national security state such as IBM and Control Data Corporation. He also approached food conglomerates like Campbell Soup Company and Green Giant.[87] And, like Kilby, he tried to interest the oil industry, which was then using its large cash reserves to diversify into seeds, animal feed, and other agricultural markets. Yet Hall didn't, as far as I can tell, seek support from grassroots groups like the Douglas County AT collective.

PRINCIPLED DISAGREEMENT

So RRI owes some kind of historical debt to scientific, intellectual, and social movements of the 1970s such as technology assessment, appropriate technology, and CSR. And practitioners of RRI owe it to themselves to learn more about those predecessor movements' failed rapprochement with square technoscientists like Hall and Kilby—if only to better understand the scale of the challenge today. RRI practitioners could also learn a great deal from the institutional enactments of those predecessor movements, such as the OTA, RANN, and NTO—if only to understand that most such experiments burn out quickly with few enduring achievements. The setbacks of the present might be avoided, and will certainly be easier to endure, if we better acknowledge the setbacks of the past.

Of course, even if RRI owes something to TA, AT, and CSR, well, times have moved on. We now live in a neoliberal world where experiments like OTA and RANN would be unlikely to get off the ground. The technoscientific reformers of the long 1970s had somewhat different aims, and operated in a much different environment, than RRI's promotors do now. As noted in chapter 1, the phrase "responsible innovation" was temporarily in vogue in the 1970s; but it didn't mean exactly what is meant by that phrase today. There is no universally accepted definition of the current version of responsible innovation, but there is *some* agreement that it rests on four principles: inclusivity (more than just scientists and engineers must be involved); anticipation (some thought must be given to future consequences and circumstances); reflexivity (participants should reflect on how their background and positioning influences their views); and responsiveness (RRI activities should lead to actions that respond to the concerns raised in the course of deliberations).[88] Those four principles are a twenty-first century invention; they simply weren't how anyone explicitly defined socially responsible science in the 1970s.

And yet, *versions* of all four principles can be glimpsed in the cases presented in this book. The long 1970s was a festive time of experimentation in how to organize science and engineering; and many of those experiments in some way included practices of inclusivity, responsiveness, anticipation, or reflexivity. That said, the episodes in this book show that none of RRI's four principles straightforwardly gave rise to a more responsible mode of technoscience. Indeed, on some occasions those principles facilitated *less* responsible technoscience—that is, a technoscience that was less humane, less democratic, or less sustainable.

That point is perhaps easiest to see with the principle of responsiveness. American technoscientists of the long 1970s had little choice but to be responsive to any number of voices: student protestors, the Nixon administration, Congress, local communities, activists representing various social movements, and others. Some squares responded more quickly than others, and some were happier to do so than others; but they all responded in some way to shifting budgetary priorities, campus takeovers, long lines at the gasoline pump, and so on. As we've seen in most of the chapters of this book, every time a new national "crisis" (urban, environmental, energy, economic) came along, square scientists and engineers responded.

But responding didn't make them *responsible* in any meaningful way. Their responses were problematic in a number of ways and were for the most part rejected. Critics of the era's technoscientists—such as the STS scholar Langdon Winner—argued that the squares' responses were too top-down, too high modernist, too confident.[89] Perhaps the technoscientists whom I've profiled would object that they were never given the latitude to adequately respond before the priorities that the country set for them were once again upended: by the time they had solutions to the urban crisis, they were being told to respond to the environmental crisis instead; by the time they had solutions to the environmental crisis, they were forced to respond to the energy crisis instead. When scientists and engineers did persevere until they could offer a mature response to an earlier crisis, the country had moved on and didn't want what they offered. At least that's what Jack Kilby thought about the rejection of his solar energy system and Arthur Hall believed about the rejection of Autofarm. Constant responsiveness didn't lead to a technoscience that anyone believed actually addressed society's problems.

Similarly, the ambivalence of RRI's "anticipatory" criterion should be apparent from this book's cases. The tools of anticipation—scenario planning, technological forecasting, and other methods such as those that Hall described as “time series-extrapolation for projecting trends, morphological analysis for discovering discontinuities in the future, the delphi technique for organizing the opinions of experts”—originated in the world of nuclear wargaming before moving into the oil and aerospace industries.[90] As Hall noted in 1972, “There is an emerging methodology called *technological assessment and forecasting*. This methodology has its roots and many of its techniques and attitudes in older disciplines called systems engineering and operations research.”[91] By the time that these techniques were applied to socially responsible research and innovation, they had already absorbed the imprint of the national security state and big business.

Thus, agencies like NASA—and its vendors such as Boeing—had extensive experience in crafting scenarios and were able to deploy them for socially relevant projects such as designing integrated utilities for public housing. But they also deployed exactly the same anticipatory techniques in aid of environment- and economy-destroying boondoggles like the solar power satellite. Indeed, NASA and Boeing used scenarios to argue that the SPS was preferable to alternatives such as energy conservation and terrestrial solar or wind power.[92] Similarly, companies like TI used scenarios as a guide to action—but their scenarios only guided them toward collaboration with the national security state, Saudi princes, and oil companies. Hall’s scenarios led him to predict that the break-up of AT&T would “be yeast for the boom in telecommunications that AT&T has retarded for so long . . . [and] greater freedom” for users, shareholders, managers, and workers.[93] Maybe so. But Hall certainly didn’t foresee that the companies that succeeded AT&T—such as Google and Facebook—would give rise to an anomic panopticon of all networked to all.

If we turn to the RRI principle of reflexivity, we again see the same ambivalence. Now, I have to admit that some of the clearest examples of reflexive practice that I’ve come across were associated with the actors in this study with whom I have the most sympathy: for example, Rudi Kompfner’s heartfelt thoughts on music or David Phillips’s reflections on carving a middle path for parapsychology research between skepticism and credulity. But reflexivity alone wasn't enough: self-awareness didn't necessarily lead to a more responsible technoscience. Phillips, for instance, imbued his teaching in the Master of Scientific Instrumentation program with incredible idealism; but by the 1980s he was working on projects funded by the Army and trying to use parapsychology to anticipate the movements of the stock market. Similarly, Hall sometimes wrote quite sympathetically about his circumstances and the origins of his views. More often, though, he used self-reflection merely to (sometimes literally) litigate grievances: over credit for various inventions, or the rejection of Picturephone and Autofarm, or how AT&T had supposedly treated him.

Some of the choicest examples of reflexive technoscience that I've come across were contained in the era's new semiunderground publications, such as *Grindstone: A Forum for Controversial Issues of Special Interest to the [Stanford] Engineering Community*, a newsletter to which Stanton Glantz was a prominent and very earnest contributor. Glantz also used the Stanford Workshop on Political and Social Issues (SWOPSI), which he ran in 1971, to interview faculty members and put them in a reflective mood. Take, for instance, this interview quote—probably from Glantz's adviser Holt Ashley, a Stanford aeronautical engineer:

> Q. Why do you want to move into the areas that you don't seem to be able to move into?
>
> A. There are several reasons. First, I think it is possible to get stale by devoting too much of one's "creative life" to a relatively narrow specialty as I sometimes feel I have been doing. Second, I think there is unquestionably a fairly direct relationship between a great deal of my students' and my research and military technology applications. Up until 5 or 6 years ago that fact didn't bother me very much and I was satisfied that the problem seemed to me to have an inherent interest. Today I'm certainly not as happy as I was then about many aspects of U.S. Foreign Policy and the domination of the military in the national scene, so it follows that I would like to find some new things for myself and my students to do that don't seem so closely coupled with that one portion of our national activity. I would like to find research activity which seems to me to have a really constructive social potential.[94]

This kind of reflection could indeed undergird a more responsible approach to R&D. But Glantz was an anomalously sincere and energetic actor (as was Ashley), and SWOPSI and *Grindstone* were exceptional institutions—we can't assume that they will have counterparts today. Perhaps one lesson of the 1970s is that RRI practitioners today should put more energy into building up institutions such as SWOPSI and *Grindstone* and treating them as critical infrastructures for sustaining reflexivity. A great deal of effort has gone into constructing infrastructures for the other principles of RRI—particularly inclusivity and anticipation—but less effort has gone into creating infrastructures for reflection.

Indeed, at least with respect to the principle of inclusivity, the infrastructures that RRI practitioners rely on today—science museums, consensus conferences, citizens' juries, public "open days" for academic laboratories—are in fact products of the long 1970s. Take science museums, for instance: such institutions do, of course, have quite a long pedigree, but their earlier incarnations tended to be quite hierarchical and paternalistic. As Rebecca Onion has shown, the long 1970s ushered in a new kind of science museum—exemplified by Frank Oppenheimer's Exploratorium in San Francisco—which was supposed to be open to all, with no distinction drawn between "experts" and "members of the public."[95] No doubt some laboratories had offered open days before the late 1960s, but it was only in the 1970s that institutions such as the CCRMA made the public's access to their sites a selling point in gaining federal funding.[96]

These efforts at inclusion were motivated in part by R&D organizations' growing belief that they could no longer take local communities for granted—or that if they did, the consequence could be picketing and sit-ins or (as in chapter 6) shocking newspaper articles that could inspire new local taxes and regulations. Firms like Signetics therefore began investing more of their money and their employees' time in civic activities, and they recruited a (somewhat) more inclusive local workforce. Organizations such as the UCSB physics department or NASA's Johnson Space Center encouraged their people to conduct research projects jointly with members of local communities. Yet despite the era's incentives for more inclusive R&D, many square scientists and engineers either weren't interested in interacting with local publics or simply didn't know how. Thus, as we saw in chapter 2, inclusive activities were often led by people in marginal positions within the sponsoring organization rather than the organization's more established members.

Established technoscientists were reluctant to adopt more inclusive practices in part because they believed that the costs of those practices outweighed the benefits. We've seen a number of cases in which that belief was clearly mistaken: the inclusive and egalitarian outlook of the UCSB MSI program, for instance, helped it recruit and teach students and led to jobs for the students and start-up companies for the teachers. And we've seen cases—such as in chapter 5—in which the lack of inclusivity had something to do with a project's failure. But we've also seen a number of cases in which frustrations with inclusivity led technoscientists to retreat from reform; Calvin Quate's exasperation with biomedical researchers' disinterest in acoustic microscopy is the clearest example. And we've also seen cases in which inclusivity led to outcomes that benefited third parties more than the R&D organization or the "included" societal partners. NASA's Meal System for the Elderly, for instance, was supposed to aid poor and elderly people with low mobility—but in the end affluent able-bodied camping enthusiasts and the companies that marketed to them probably saw the most return on that research. In other words, inclusivity—just like the other three RRI principles—was a decidedly ambivalent indicator of "responsible" outcomes. Inclusivity helped some technoscientific projects to become more socially responsible but contributed to other projects' failure or their capture for unjust or exclusionary ends over the longer term.[97]

MORE OR LESS RESPONSIBLE

So the technoscience of the 1970s certainly featured numerous experiments with all four principles of today's RRI (inclusivity, anticipation, reflexivity, responsiveness). Yet that experimentation was characterized by ambivalence and ambiguity: sometimes giving rise to more socially responsible outcomes, sometimes decidedly not. The four principles of RRI would therefore seem to be equally amenable to societal relevance and irrelevance, responsibility and irresponsibility.

One could argue that it's not the four principles in isolation that are necessary for more responsible R&D, but rather their synergistic combination—and that that synergistic combination of all four is what sets modern RRI apart from its predecessors *avant la lettre*. Fair enough. It is admittedly difficult to identify an R&D organization in the long 1970s that synergistically enacted all four principles at once. But my sense is that there is too much interpretive flexibility in the enactment of the four principles to use them as a litmus: too often they contribute to outcomes and objectives that don't seem very socially responsible.

So I would argue that the four principles don't help us demarcate socially responsible research and innovation from its opposite. Certainly, the four principles aren't timeless demarcations of responsibility that can be straightforwardly applied to the past. They may not even have much applicability beyond the near future, because RRI is already—as of 2019—being mockingly "buried"; whatever movement succeeds RRI will offer some other set of demarcations. In this book I've therefore tried to avoid timeless criteria for identifying "responsible" R&D; instead, my test for responsibility consists of questions that are keyed to the specific conditions of American physical and engineering science in the 1970s, though they could be adapted for today. For instance, was this person or organization becoming more oriented to taking life or to fostering the good life; to helping the environment or harming it; to serving the vulnerable or the powerful; to chasing profit for its own sake or to pursuing some broader set of social goods (potentially including but not limited to profit)?

By that standard, we can see that—in general—America's square scientists and engineers and their R&D organizations did practice a more responsible technoscience in the long 1970s than they had in the 1960s or they would in the 1980s. My case studies provide reasonably good evidence that even the most grudging squares were at least as successful in enacting a reformist technoscience as their more enthusiastic countercultural peers. Over the course of the 1970s, however, many squares abandoned their experiments with reform or decided that profitability was an adequate measure of social responsibility. No doubt some squares had equated social responsibility with profitability all along. At least at the beginning of the long 1970s, though, profit was more often framed as a *means* for achieving social responsibility rather than as a metric of responsibility or as an end in itself. As Arthur Hall told Emerson Electronics executives in 1972, efforts such as RANN and NTO "reflect some assessment of social needs and an attempt to match them with technologies that might satisfy the needs, and convert them to new markets."[98]

Of course, squares weren't alone in drifting toward an exclusive focus on profitability and disregard for other measures of the public interest. In the 1980s, many countercultural reformers became good Reaganite market worshippers as well. It was hard not to! And those few reformers who persevered with the experiments of the 1970s (such as the New Alchemists or Science for the People) usually ran out of steam by the early 1990s at the latest. Curiously, personal politics or lifestyle didn't seem to matter that much in determining whether someone moved toward or away from socially responsible R&D; whether square or groovy, almost everyone moved toward such projects in the 1970s and away from them in the 1980s. In other words, structural incentives for or against reform were far more determinative than personal politics or lifestyle. So if we want a more socially responsible research system today, we should put at least as much energy into devising new structural incentives as we do into reforming scientists' and engineers' personal views.

But first, we need to know a lot more about *which* structural conditions incentivize more responsible research and which ones encourage less responsible research. My case studies don't settle that question, but I think they do point in some fairly clear directions. Let's start with the optimistic "more responsible" side of the equation. Optimistic, if rather tawdry, that is: for one of the basic findings of this study is that with great funding comes great responsibility. Scientists and engineers of (nearly) all political stripes responded to new funding streams that were dedicated to solving societal problems. In the long 1970s such funding took a variety of different forms: existing organizations such as NASA diverted existing funding in new directions; other existing organizations formed entirely new funding streams (e.g., RANN at the NSF); and a few new organizations (such as the EPA) entered the scene. Funding for socially responsible R&D was still hard to get; but given that defense R&D budgets were shrinking rapidly, funding for R&D oriented to public housing, mass transit, disability technologies, alternative energy, environmental sensing and remediation, and other areas suddenly looked very attractive indeed.

Square scientists and engineers showed significant creativity in looking for money in unexpected places and in revising their portfolios to include the societally relevant research that funders wanted. Sometimes their efforts were quixotic: for example, Hall's and Kilby's soliciting of Saudi money or David Phillips's proposals for parapsychology funding from local peace activists. But sometimes they succeeded: for example, Calvin Quate's grant from the Hartford Foundation for acoustic microscope research, or Virgil Elings's grant from the American Heart Association for development of a new blood flowmeter. And the funders themselves were similarly creative and willing to run risks: witness the joint DARPA–National Bureau of Standards grant that supported Quate and other researchers in aid of the US semiconductor industry; or the joint NSF–National Endowment for the Arts scheme that funded Stanford's CCRMA.

If money was an effective carrot for encouraging more responsible R&D, then there were also several similarly effective sticks: investigative journalism, probing questions from Congress, and laboratory sit-ins. Many squares were particularly bothered by the latter, bemoaning direct-action protests as an end to freedom of inquiry. Yet even those who objected ended up doing more or less what the protestors wanted. As a result, reform was almost certainly in its fullest swing while conscription was in effect and young people were therefore easy to mobilize for protest. The end of the draft in late 1972 (along with the oil shock the next year) concluded the first phase of the long 1970s, with a steady decrease in protests and hence a decline in reformist activities after that point.

Not that I am arguing that a return to conscription would make American technoscience more responsible today! Indeed, one of the clearest signals from my cases is that square technoscientists became ever more responsible while the national security state was shrinking and (mostly) retreated from reforms once the Cold War heated back up. That observation shouldn't be taken as a blanket condemnation of defense research in general. My view—which I think most squares shared—is that *some* kinds of defense research are in *some* circumstances justified and should even count as socially responsible. How much of what kinds of defense research and under what circumstances—those questions are where the disagreements lie. Clearly, the military's enormous R&D budgets in the early Cold War contributed to some socially progressive outcomes; John Linvill and James Meindl's Optacon is as good an example as any. But so long as the national security state was the black hole at the center of American R&D, the considerations of other parts of society were secondary. When the national security state became merely one among many players, the research system became more inclusive and responsive. The lessons of the cases in this study are perhaps most compatible with Stanton "Glantz' goal of reducing DOD funding to 'say 10–20% of the research'" in places like Stanford's School of Engineering.[99]

While the Pentagon was retreating, the civilian state was advancing—and that, too, was closely associated with more responsible technoscience. R&D funding from civilian federal agencies facilitated reform; but in some respects the more durable changes were wrought by regulatory and administrative changes, not just from the federal government but also at the state, county, and municipal level. Environmental regulations made companies like Signetics less lax in disposing of chemicals used in manufacturing and made researchers at places like NASA or Science

Spectrum more interested in designing instruments to detect pollution. Equal opportunity and antidiscrimination laws effected lasting if inadequate changes in the workforces of almost all R&D organizations.

The observation that a shrinking defense establishment and a growing regulatory state foster responsible innovation runs counter to some prominent RRI voices. Particularly in the United States, some leading RRI proponents speak glowingly of the Pentagon's "can-do approach" and ability "to catalyze rapid innovation" while excoriating "fifty years of growing public investments" on the civilian side that supposedly generate nothing but "contradiction, controversy, and confusion."[100] This view is also linked (at least institutionally, but I think also ideologically) to a view that RRI allows "governance from within" which avoids the dysfunctions of "regulations, which often are directives from upstream or downstream, are largely static, can become outdated, and can fail to apply clearly to dynamic and changing R&D processes and contexts."[101]

I find both these views problematic—indeed, problematic *with respect to the aims of RRI*. The view that the Pentagon offers the right model for responsible R&D, in particular, utterly disregards the calamity and irresponsibility of the Cold War. The superpower confrontation was, of course, not cold at all; it stimulated fifty-plus years of armed conflict all over the world. And even in its coldest corner—the stockpiling of nuclear weapons for an Armageddon that never came—the Cold War cost untold lives: from the mining, processing, and disposal of uranium and other incredibly hazardous materials; from the construction, storage, transport, and maintenance of and training for the use of nuclear weapons; and especially from the hundreds of thousands of premature deaths traceable to nuclear testing.[102] Some squares were drawn toward socially relevant research because they grew to be appalled by these horrors. Others adopted socially relevant research but still continued to believe that confronting communism was worth the costs. Both those views can be defended, but to argue in good faith that the Pentagon's "can-do" attitude provides a good model for responsible R&D, one must do a full accounting of the costs of that attitude.

Meanwhile, any form of RRI that dismisses the regulatory state risks discarding the most versatile and effective means of encouraging scientists and engineers to practice a more democratically responsive form of R&D. The Clean Water Act of 1972 and other regulations that were instituted in the 1970s were a direct expression of popular social movements such as environmentalism and appropriate technology. Nationally influential individuals like Rachel Carson and Barry Commoner and locally influential ones such as Susan Yoachum and Lorraine Ross did much to build public support for their cause—but that broad appeal was largely translated into effective outcomes by the actions of a democratically responsive regulatory state. Similarly, participants in the feminist and civil rights movements worked hard to stigmatize discrimination; but they worked just as hard to get laws passed to make discrimination legally impermissible too. Why, then, would RRI's promotors strive to make technoscience more inclusive while simultaneously rejecting one of the most effective tools—the state, and specifically the regulatory state—for reaching that end?

Still, it's probably true that funding, regulation, and civilianization alone aren't enough to sustain reform. Those approaches need to be complemented by other avenues to more responsible R&D. We could, for instance, try to recruit and retain more scientists and engineers who are already more socially aware, especially those who are minoritized in some way. After all, we have some evidence that minoritized scientists and engineers differ from their peers in, as Joanna Weidler-Lewis puts it, "the belief that 'good' engineers use engineering skills to create a better world for all."[103]

We can also try to ensure that the environments in which square scientists and engineers operate are richer in cues that they should reform their practice. Admittedly, in the 1970s such cues often emanated from sit-ins and investigative journalism—communicative genres that often caused squares to harden their attitudes. Yet direct action of that sort at least required squares to account for themselves. And when the senders and receivers of such cues could meet on common ground—as at the postprotest discussions at Stanford's Systems Techniques Laboratory in 1968—then attitudes sometimes softened. Cues that originated from scientists' and engineers' families, friends, students, and close personal networks, however, were probably even more persuasive. That, at least, is one plausible abductive leap from the examples of Virgil Elings and David Phillips in chapter 2.

Durable change probably also depends on finding a mode of reformist technoscience that square scientists and engineers *enjoy*. Evidence for this in my case studies is murky, but I do have the sense that many squares were affectively drawn to and energized by the new societally relevant topics that they adopted in the late 1960s and early 1970s. They were fascinated by the technical challenges and heartened to do something that they thought would directly and obviously benefit others (in contrast to the indirect or inscrutable benefits derived from basic or defense-oriented research). Interactions with people outside their usual circles also sometimes sparked joy, at least early on before the frustrations of miscommunication set in. Hall, for instance, could be quite sour in his correspondence with other "professional scientists, engineers, and managers" in the United States, but when he got to meet similar experts in Poland in 1974, he described it as "one of the high points in my entire professional career" for the opportunity it provided to meet "as we say colloquially, 'the salt of the earth,' which means the kind of people that we like to know as personal friends."[104]

Some of the sense of fun and social worth that squares experienced in the early 1970s seems to have been associated with a spirit of experimentation. Organizations that were under stress experimented with lots of different ways to relieve that stress: new interdisciplinary centers, new degree programs, new collaborations, and so forth. Previous studies have shown how groovy counterculturalists took the era's organizational stressors as an opportunity to experiment with a more fulfilling and enjoyable kind of science.[105] But I've tried to show that at least some squares took the same license and went in not dissimilar directions—for example, Linvill, Meindl, and Bliss's Optacon (and the start-up they founded to sell it) or James Angell's pursuit of electronic music and biomedical electronics. One particular kind of bureaucratic stress—existential success—beset a number of organizations in this study and occasioned a variety of experiments with socially responsible science and engineering. That could indicate that RRI practitioners should think about how to leverage organizational stagnation, stress, and renewal. As Thomas Heinze and Richard Münch argue, organizational renewal *is* research; thus, we should view responsible organizational renewal *as* responsible research.[106]

Throughout this book, I've offered quotes that give some sense of the positive affect that some squares associated with organizational experimentation. Of course, I've also offered quotes that evince the negative emotions that other squares associated with the changes going on around them. And here we can start to talk about the things that made American science and engineering *less* responsible. Clearly, when influential people like Bill Rambo groused—even in private—about reforms being imposed on the physical and engineering sciences, their complaints inhibited reform. But I think a more important factor in reversing the trend toward socially responsible research was simple bureaucratic attrition. Organizations tried out lots of new initiatives in the early 1970s, but by the end of the decade they were ready to cull those experiments and select only the ones that promoted current organizational objectives. And as the 1970s turned into the 1980s, those objectives grew more oriented to economic competitiveness and the resurgent superpower confrontation than to societal relevance.

Many squares offered little resistance to that culling because by that point they thought that socially responsible R&D just wasn't as rewarding as it had once been. Some of that weariness reflected a growing realization that the technical challenges that they had set out to solve weren't *just* technical and possibly weren't soluble at all, and that any solutions they offered would likely be ignored or rejected. Those kinds of puzzles are, well, not very fun. We can hardly be surprised that under those conditions, many scientists and engineers chose to turn away from socially responsible R&D and to seek new projects where they could be more certain of success and approbation.

Of course, square scientists and engineers also shouldn't have been surprised that their efforts didn't meet with instant approval. They *should* have known that the public acceptance of technoscientific expertise must be earned through the hard work of persuasion, and not just through the hard work of calculation and design. Yet when squares tried to understand why their ideas weren't taken up and why the joy had been drained out of their experiments with socially responsible R&D, they almost never assigned any blame to themselves. The hippies at SERI were too technically incompetent to appreciate TI's solar energy system; the bureaucrats at the NSF were too inept to organize funding for Autofarm; the bean counters at HUD were too cheap to pay for space-age public housing; biomedical researchers were too conservative to understand what the acoustic microscope offered them—these were the kinds of explanations that came more readily to the squares.

Such explanations are products of the most intractable factor militating against socially responsible science and engineering: namely, the enormous value placed on (certain kinds of) technical merit, and the disregard for those deemed not to have (those kinds of) merit. Not all, but most of, the scientists and engineers whom I've profiled here seemed to agree with the physicist Alvin Weinberg's view: that is, social problems can be solved with a "technological fix," but only so long as those who don't understand technology listen to those who do.[107] We can see this stance in Arthur Hall's refusal to allow communications students into his classroom, or in his desire to be seen as "a practicing engineer-scientist with a few practical results to his credit" rather than an "overalled farmer nut." More perniciously, we can see it in his claim that systems engineers should oversee a program of "birth control and planned parenthood as one way to limit consumption of earth's resources."

To be sure, technical merit shouldn't be disregarded. The classic retort—that the critic, too, would rather fly in an airplane designed by a competent engineer than by an incompetent one—isn't wrong. But it isn't right, either. As scholars such as Amy Slaton have pointed out, scientists' and engineers' ascriptions of competence and incompetence too easily line up with their own interests in terms of class, race, and gender.[108] So long as they don't question that alignment, technoscientists aren't showing the hard-nosed skepticism that they apply to everyone else.

That is, ascriptions of incompetence should always be two-sided. Maybe, for instance, Jack Kilby was right that "all of" the DOE's solar energy specialists were technically inept and should be fired; but Kilby, too, got some important things wrong. He failed to persuade others of the merits of the TISES system. Nor did he and his colleagues design TISES in a way that would have enduring appeal even after the price of oil dropped; he put his faith in untrustworthy allies and he failed to enroll other potential allies who might have kept TISES going. Similarly, maybe Calvin Quate was right that biomedical researchers were too hidebound and incurious to recognize the advantages of the acoustic microscope. But Quate, too, was counterproductively conservative. He failed to educate himself in how biomedical researchers make judgments, and he failed to immerse himself in their world enough to bring them around. Instead, he turned back toward audiences in semiconductor manufacturing with whom he was more comfortable and who didn't cast any doubt on his expertise.

In other words, we have to see "good" science and engineering as relatable to some kind of shared good, rather than evaluating what counts as good in solely technical terms. Research and development are not machines for generating countable metrics of technical merit: *X* many truths, *Y* many bombs, *Z* many dollars. Rather, science and engineering are social activities that create much that is "good" that cannot be measured by technical standards: meaning, solidarity, opportunity, material advancement, spiritual enlightenment, mystery, fascination, and so on. Science and engineering necessarily draw on the society of which they are part and parcel for the valorization of these more-than-technical goods that are generated out of technical activity. It is these more-than-technical aspects of science and engineering that offer some possibility of greater common ground with the rest of society—some hope that the middle need not always be excluded.

Of course, like all human endeavors, science and engineering have a noxious side too: they can bring discrimination, pollution, violence, anomie, boredom, and so on. The long 1970s saw all these aplenty. Technical competence—at least as usually understood—does not seem to safeguard against these more negative aspects of technoscience. Quite the opposite, in fact. So, on their own, technical measures of good science and engineering do not seem capable of delivering good science and engineering. If we want good technoscience, then the kinds of questions we have to ask are not purely technical—at least not in the usual sense. Rather, we have to ask what and who is technoscience for? Why do we do it this way and not another way? What kind of world do we imagine technically competent science and engineering will lead to?

To make this point more eloquently, I'd like to give the final word to one of this story's nontechnoscientists: Richard Lyman, historian and president of Stanford for much of the long 1970s. Not that historians have all the answers, but Lyman was for the most part a perceptive and sympathetic observer of American science and engineering in this period. He understood that science and engineering are heterogeneous activities, not homogeneous machines—and that therefore technically competent science and engineering cannot, on their own, get us to a society that anyone would want to live in. He didn't shy from pointing out that achieving more responsible science and engineering takes money and patience—you can't get there overnight or on the cheap. Most of all, Lyman saw that creating a technoscience that is responsible to its time can only be accomplished by overcoming the excluded middle and encouraging mutual comprehension and not just mutual ambivalence. Or, as he put it in 1970,

> If we are in difficulties partly because our functions are many, and our focus can therefore never be single, it will do us no good to try to return to some simpler day. . . . Instead we ought to glory in the fact that some people are learning to appreciate Keats in one part of the campus, while others are solving problems of

linear programming in another. Glory in it, and make a towering virtue of necessity by exposing the one group to the other, and each to a thousand further groups, at every available opportunity. It is an arrogant assumption of some humanists that no computer man reads Keats, and no electronics buff can dig Scarlatti. It is an arrogant counter-assumption of some technologists that no humanist has anything important to contribute to life in the technitronic age of the future. The university exists in part to attack such arrogant parochialisms, and with the help of donors of courage, steadfastness, and a capacity to see things in perspective, the attack will in the long run succeed.[109]