Chapter 5

# Strategies for Commercial Technology Development: High-Temperature Superconductivity and Beyond

# CONTENTS



## Boxes



## Table

# Strategies for Commercial Technology Development: High-Temperature Superconductivity and Beyond

## SUMMARY

Together, a collection of government actions constitutes a strategy, just as the actions of a corporation's upper management constitute a strategy. *De facto* strategies, though hardly unheard of in business, are more common in government. indeed, to some, the very notion of strategy implies a measure of loss in one of the primary strengths of U.S. technology policies—the flexibility of Federal agencies, their ability to respond quickly to new circumstances,

Regardless of approach for promoting commercial development of high-temperature superconductivity (HTS)—and regardless of whether the approach is called a strategy—success will require diversity in sources of funding and in the R&D programs that Government money supports, Earlier chapters stressed the uncertain in prospects for HTS. With several agencies involved, good ideas will get a hearing—at the Defense Advanced Research Projects Agency (DARPA), if not the Department of Energy (DOE) or the National Science Foundation (NSF). Duplication, in any case unavoidable, can spur competition. *Continuity*, likewise, will be important. To encourage U.S. industry to take a longer view, the Federal Government must do so itself (a need addressed by several of the policy options in ch. 4).

In keeping with continuity and stability in funding, any strategy for HTS should *avoid high visibility*. If policy makers or the public look to Federal programs for near-term breakthroughs, disillusionment will follow. Technological advance cannot keep up with expectations fed by the media.

The Federal Government will have to let *market forces drive HTS technology* as much as possible. Historically, governments have done a poor job of trying to anticipate what markets will demand, if not the course of technological evolution; picking R&D fields ripe for major technical advance is one thing, picking winning commercial applications quite another. Policies that pull technologies into the marketplace in more subtle ways—e.g., through government procurement—can work, especially in conjunction with R&D funding designed to push the technology along. *Market pull coupled with* technology push—in a policy environment that encourages *collaboration among industry, universities, and Government—will* help speed commercial technology development. Box Q discusses these and other operating principles in more detail.

This chapter considers three approaches through which the Federal Government might foster commercialization of HTS:

- *Flexible response*, Strategy 1—the current, *de facto* U.S. policy–grows naturally out of postwar U.S. technology policy. Characterized by strong support for basic science and for mission-oriented technology development, direct measures for supporting commercial technology development find little place.
- *An aggressive response*, Strategy 2, would differ in three major ways from current policies. First, NSF would have more money for HTS—in essence, an insurance policy to make certain that good ideas for basic research have a shot at funding. Second, the Federal Government would share in the costs of private sector collaborative R&D ventures. The rationale: more work within industry on long-term, high-risk HTS R&D. Third, a working group of experts from industry, universities, and government would be assembled to decide which collaborative R&D proposals were worthy of support, and to otherwise advise on policy measures,

### Box Q.--Strategy: Key Ingredients

Disagreements over strategies for technology development reflect differences of opinion over technical questions (How long will it take to develop flexible conductors made from the new superconductors?) and over market conditions (What applications will be most attractive at liquid nitrogen temperatures?), as well as over matters of political preference (Is it proper for government to support commercial technology development directly?). Some policies—e.g., support for basic research—find nearly universal support. So do some of the other ingredients that would find a place in almost any strategy for supporting HTS:

- Diversity in sources of R&D support. No one knows what new discoveries may emerge in superconductivity, where they will come from, or when. A portfolio of research makes more sense than one *or* **a** few centers of excellence; so do multiple sources for contracts and grants.
- Continuity in support, over a period that could easily be a decade (as emphasized in ch. 4).
- A judicious balance ***of technology push and market pull.*** Technology push via R&D support works best when accompanied by policies such as government purchases and demonstration projects that help pull high-risk, high-cost technologies into the marketplace. Overall, however, government should ***let industry drive technology as much as possible.***
- ***Measures that encourage collaboration among universities, industry, and Government.***

**Diversity**

Many sources of support, and many centers for R&D, may mean duplication of effort, but that is not necessarily bad. Overlap breeds competition and helps ensure that no path goes unexplored. There is another side to diversity. Accountability can suffer: if everyone is responsible for HTS, then no one is fully accountable. Still, lacking an overriding goal—sending a spacecraft to Mars, or building a magnetically-levitated (maglev) rail system along the Eastern seaboard—where is the need for centralized responsibility? Development of HTS is a broad objective, and also a fuzzy one: the technology cuts across the missions of a number of agencies.

NSF funds research proposals rated highly on grounds of their promise in advancing science and technology; the subject of the research carries less weight. Not without controversy, the process nonetheless has found wide acceptance. But when it comes to projects in the mission agencies, decision-makers do not always see eye-to-eye on what should be supported. In the early days of computer technology, visionary projects such as Whirlwind and ILLIAC were as fiercely opposed by one set of agencies as they were favored by those paying the bills; U.S. computer technology would not have advanced so quickly had any one agency been solely responsible.[1]

Continuity

In reality, diversity is seldom a problem in the decentralized U.S. system–it comes naturally. The more common problem is continuity. Stop-and-go decisions have bedeviled U.S. technology policies, as chapter 4 makes clear. Congress passes the Stevenson-Wydler Act, but a new Administration does not fully implement it. The executive branch seeks to double the NSF budget over five years, but Congress does not appropriate the funds.

If there is a secret to Japanese technology policy, it lies in continuity-stability in commitment and financing, without rigidity. Publicly-funded R&D programs in Japan, many of which have enhanced the competitiveness of Japanese industry although budget levels have been modest, often begin with an 8- or 10-year planning horizon. R&D priorities change over the course of the maglev train program (ch. 3) or the fifth-generation computer project as results come in and new directions open. Budgets may change too. But sharp reversals are rare. A decade-long time horizon stands for all to see as a demonstration of commitment by both government and industry. R&D sponsored by the U.S. Government often lives from one budget cycle to the next; the consistency seen in Japanese policies

---

[1]"Government's Role in Computers and Superconductors," prepared for OTA by K. Flamm under contract No, H36470, March 1988, pp. 9-27.

has few precedents, even in defense (just as few American firms have shown the persistence in product development that led to export successes like video-cassette recorders for the Japanese).

In the United States, the spectacular success of a few flagship efforts like the Manhattan Project and Apollo left a trail of unrealistic expectations. NSF's RANN program (box O, ch. 4) sprang from the notion that the technical expertise needed to put men on the moon could be turned, almost as directly, to social problems. Operation Breakthrough, the Department of Housing and Urban Development's effort to revolutionize the technology of residential construction, grew from the same soil. RANN came and went in half a dozen years, Operation Breakthrough even more quickly.

HTS will be equally vulnerable. The public's expectations have been raised by a year and a half of scientific discoveries, a Federal conference featuring the President and three cabinet officers, and a Nobel Prize—all accompanied by ample media coverage. In the absence of steady progress, public support may wane; the painstaking and laborious work needed to turn science into useful technology spawns few headlines.

**Low Visibility**

In the United States, publicly-supported R&D programs have generally been more successful, and more durable, when they avoid high visibility.[2] Apollo was the exception, not the rule. If policy makers or the public look to a Federal HTS program for immediate technological or commercial triumphs—in the extreme, as a flagship in the international competitive struggle—they will be disappointed. A high-visibility, crisis-driven program such as the synthetic fuels initiative of the 1970s may collapse on itself, and in doing so harm the cause of future support for related efforts.

**Technology Push and Market Pull**

Visibility, by itself, did not do in the synthetic fuels program. The failure lay in attempts by government bodies to anticipate the course of technological evolution and the needs of the marketplace. (Japan has not been immune: the Ministry of International Trade and Industry tried to force Honda out of the auto business in the 1960s.) The lesson, repeated many times over: in the absence of convincing reasons for doing otherwise, let market forces drive technology.

Although picking winners is something that Federal agencies have never done well, policies that serve to pull the market in more subtle ways have proved beneficial. They are particularly effective in conjunction with R&D funding designed to push the technology along.

To elaborate, the Federal government has confined its role in (non-defense) technology development largely to funding research, on the assumption that the commercial market could and would create the necessary demand for resulting products. But this is an area where the market does not function perfectly. Because product development efforts in high-risk technologies are extremely expensive—accounting for nearly 80 percent of R&D costs—firms must have some confidence that there are customers at the end of the tunnel; potential customers often do not have enough information or certainty to provide that assurance, however. Thus, there is a role for government in helping assure an early market for such products.

In computers and semiconductors, Federal Government procurement provided that assurance. The defense-space share of the total computer hardware market was 100 percent in 1954, and it exceeded 50 percent until 1962. Similarly, during the early years of integrated circuit production, defense and space procurement accounted for almost 100 percent of sales. Given assurances of stable–indeed growing—demand, companies raised their own R&D spending. Technological advances coupled with learning and scale economies led to dramatic price reductions for computers. Even more important was the "demonstration effect": successful use of computers by military and space agencies proved their value to a skeptical business community.

Defense procurement was effective at pulling computer and semiconductor technologies along for two reasons. The government's mission-based needs meant that agencies evaluated technological

---

[2] "Collaborative Research: An Assessment of Its Potential Role in the Development of High Temperature Superconductivity," prepared for OTA by D.C. Mowery under contract No. H36730, January 1988, pp. 13-14 and 67-88.

alternatives carefully and provided valuable user feedback to suppliers. Moreover, agency needs, and the technologies they spawned, meshed closely with business needs. (As one computer executive observed, “Space and defense computer applications .,. served as a ‘crystal ball’ for predicting the future direction of computer use in industry.”] Federal demonstration projects have been similarly effective under the same conditions, i.e., when mission-based government support of a developing technology steers it in a direction which converges with commercial interests.

**Lacking this,** though, the synergy can quickly vanish. Because of the growing divergence between military and civilian technologies and markets, defense procurement no longer has the positive impact it once did on commercial technology development. That issue aside, procurement and demonstration projects have been less effective when they have been done without the guidance of an agency mission.

Governments can strengthen market forces in other ways: in Japan, government-financed enterprises buy computers and robots and lease them to end users. The result? Guaranteed markets for the manufacturers, and reduced risk for the customers, who can turn back the equipment if they find it unsatisfactory.

Finally, government regulations can also pull technologies into the market, sometimes with good results. Federal fuel economy standards created incentives for American automakers to improve their capabilities in engineering and producing small cam. Technical standards (e.g., for computer languages) can be an important spur to technology diffusion.[3]

**As this discussion suggests, market pull policies create vexing dilemmas for governments. Policies to support the adoption** of **publicly-funded R&D results are an essential** component of a government effort to develop technology. But insofar as these incentives for technology adaption target specific applications, policymakers are placed in the position of trying to forecast the course of technological evolution and anticipate the commercial market. This is a task they have done poorly in the past.

### Interactions Among Industry, Universities, and Government

**A final principle, again emerging from the postwar history of high technology: collaborative interactions among universities and industry speed technological advance, particularly when supplemented by government R&D support and procurement. Coupling between industry and universities played a major role in the development of computing during the** 1940s. The **first practical electronic machine was built at the University of Pennsylvania. After the war, many of the key scientists and engineers left university and government laboratories to staff fledgling computer manufacturers like Univac.**

In the late 1970a, genetic engineering and biotechnology--supported in universities and the laboratories of the National Institutes of Health with Federal dollars--moved rapidly into the private sector, aided by abundant infusions of venture capital. Today, the United States remains well ahead of Japan and Europe in biotechnology. At this stage, coupling among universities, industry, and government makes sense for HTS: much of the research remains well-suited to academic settings; firms in many industries want a window on the technology; Federal agencies are already putting money in.

[3]See *International Competition in Services* (Washington, DC: Office of Technology Assessment, July 19s7). pp. 315-317, Airline regulations, by limiting price competition, forced carriers to seek other insane of differentiating their services. They vied to offer travelers the latest equipment in order to compete on speed and comfort, buying planes from Boeing, Lockheed, and McDonnell-Douglas. These manufacturers, in turn, developed new models.

- *A Federal technology agency*. Strategy 3 considers proposals for altering government responsibilities for science and technology—a subject that goes far beyond the particular needs of HTS. OTA analyzes three variants: a cabinet-level science and technology agency: institutional changes that would substantially raise priorities for engineering research; and direct support for civilian, commercial technologies. The second two hold more promise than the first. Of course, such changes would take time, meaning that they have little to offer in terms of the immediate needs in HTS.

Each of the strategies has advantages and disadvantages. *Strategy* 1 is in place and working; the initial Federal response to HTS illustrates the considerable strength of the traditional approach. Mission agencies moved quickly, funneling millions of dollars into HTS R&Din a matter of months. The scientific community moved even faster, with large numbers of skilled professionals shifting into HTS from related fields. The magnitude of the response reflects the sheer size of the pool of scientific expertise in the United States—itself a major source of advantage.

Several weaknesses are apparent in the U.S. response. First, the universities are having a hard time competing for funds; DOE laboratories are getting roughly as much money for HTS research as NSF has for all the Nation's universities. The second weakness: almost complete reliance on mission agencies to support HTS R&D. As a result, not enough R&D money flows to non-defense industries–which might not be a problem, were American firms pursuing HTS as aggressively as Japanese firms. Neither DOE nor the Department of Defense (DoD) can be expected to provide broad support for industrial R&D in HTS. HTS technologies stemming from DoD R&D may eventually find their way into the marketplace, but time lags that made little difference in the 1950s and 1960s, when the United States dominated the technological frontier, can be fatal in the 1980s.

OTA's analysis suggests that continuing along the lines of Strategy 1—the current approach—will more than likely leave the United States behind in superconductivity. In mid-1988, the U.S. position in HTS looked like a strong one. But this is because HTS remains largely a matter for scientific inquiry. With progress toward applications, the picture will change. At best, a lead in science creates small advantages, often fleeting. The real contest will be over applications engineering and manufacturing—where Japan excels, and where proprietary technology, much of it developed in industry, will make the difference.

*Strategy z,* an aggressive Federal response, would, first of all, assign NSF a greater role in sponsoring R&D. Although basic research in HTS does not seem underfunded, more money for the Foundation—perhaps $20 million over a 5-year period, specifically for HTS—would guard against missed opportunities in relatively fundamental work. With enough funding available for its research center programs, NSF would also be in a position to support one or more proposals for centers dedicated to HTS.

As the second step in a more aggressive strategy, the Federal Government could share in some of the costs of R&D conducted by industry consortia. This is the simplest and quickest way to steer more resources into applied research and generic technology development. Government could direct public funds to R&D industry views with favor, but where the benefits would be difficult for individual firms to capture.

A working group on HTS would be assembled to help carry out this second strategy, and in particular to make decisions on cost-sharing. The aggressive response approach requires agreement on an R&D agenda—a consensus that should not leave out the universities or the national laboratories, even though industry's view of commercial needs would have to come first. The primary task for the working group: deciding which R&D consortia receive Federal dollars—a sticky issue, one involving decisions going beyond the scientific merits of alternative projects.

This strategy skirts the "picking winners" problem raised by Federal subsidies for private R&D by, in effect, allocating government resources to the highest bidders, That is, among proposed R&D consortia—all of which were technically qualified—funds would go to those whose members were willing to make the longest term financial commitment and self-finance the highest fraction of total costs. These criteria would allocate government financing to joint R&D ventures with the greatest expected payoffs over the medium to long term. **The aggressive strategy for commercializing HTS could substantially improve prospects for rapid commercialization of HTS in the United States** at relatively modest cost.

Under Strategy 3, OTA addresses prospects for a Federal agency charged with supporting commercial technologies, A perennial issue in U.S. science and technology policy, many alternatives have been proposed over the years. The possibilities range from a small, independent agency with a budget of less than $100 million, to a cabinet-level Department of Science and Technology pulling together some (though hardly all) of the R&D activities of existing agencies,

The cabinet-level Department of Science and Technology came forward once more in 1985 as the lead recommendation of the Young Commission on competitiveness, but found no more support than in the past. Alternatively, is it possible to envision a smaller Federal agency with industrial technology as its mission? Once again, proposals have been common—e.g., for a national technology foundation, paralleling NSF's role in support of science, or a civilian version of DARPA. The latter has attracted particular attention, given DARPA's enviable reputation—a small group of creative people, with the judgment and experience to seek out and support the best ideas. But DARPA has a mission, and a critical one—support of long-term R&D with potentially big payoffs in military systems.

Lack of a comparable mission is the potential Achilles' heel for a civilian technology agency. All such proposals face a common problem: providing money for industrial R&D, in the name of commercialization and competitiveness, without a well-understood and widely-accepted mission, (Competitiveness is a notoriously slippery concept—more so than national defense or health.) Lacking such a mission to lend discipline to the process of setting priorities and making funding decisions, a Federal technology agency could easily end up subsidizing marginal projects,

The more of its funds such an agency channeled to industry, the deeper the possible pitfalls. Direct funding of industrial R&D raises the specter of subsidies won by lobbying rather than merit. Yet if the technology is to be useful to industry, then much of it should be developed by industry, Dealing with the many and contentious issues posed by a Federal agency for commercial technology development would be difficult, although not necessarily impossible. If such an agency is to support R&Din the public interest, it will need to find ways of identifying what that public interest is, convincing potential critics that it has done so fairly, and that the results justify continuing support.

Plainly, the three strategies analyzed in this chapter are not exclusive. They do represent differing views of the strengths and weaknesses of the U.S. approach to technology development. Those who believe that the fundamental strengths of the U.S. system remain intact feel that industry will be able to commercialize HTS when the time is right. Those advocating a more aggressive policy stress the dangers of a business-as-usual mentality, given the surprising speed with which U.S. industry has lost its earlier advantages in high technology. The underlying worries over loss of competitive advantage lead those who would favor the third strategy, or something like it, to argue that the United States needs to thoroughly overhaul its approach to technology policy.

## STRATEGY 1: FLEXIBLE RESPONSE

### The Current Approach

This strategy presumes that the existing policy framework is appropriate and sufficient for supporting HTS.[1] To those who advocate this approach, a major departure from the current course would be premature—at least during the early stages of HTS. With a good deal more basic research required to overcome the technical obstacles posed by the new materials, the

[1] For general background, see A.H. Teich and J.H. Pace, *Science and Technology in the USA* (Essex, UK: Longman, 1986); also H. Ergas, "Does Technology Policy Matter?" *Technology and Global Industry: Companies in the World Economy,* B.R. Guile and H. Brooks (eds.) (Washington, DC: National Academy Press, 1987], p. 191.

President's initiative (box B, ch. 2), along with other executive branch actions, will provide a sound basis for industry to commercialize HTS —when the time comes, And, so the argument goes, if the pace quickens, or foreign competition intensifies, there will be ample opportunity to agree on a stepped-up response.

This is the de *facto* U.S. strategy. The Federal Government is following traditional channels, relying on existing institutional arrangements, and avoiding direct support for commercial technology development. A continuation of this approach (indeed, almost any approach) will mean:

- Heavy ongoing funding for defense applications of HTS. The Strategic Defense Initiative (SDI) will continue providing a good deal of the money, and DARPA will probably continue to have a prominent place as well. Both industry and universities would get research money—some of the the latter through DoD's University Research Initiative—but within half a dozen years, aerospace firms and military systems houses would probably be conducting the bulk of DoD-sponsored superconductivity R&D. Although DARPA has stated that the results of its processing contracts will remain unclassified, such a policy will be subject to change, depending on outcomes. As R&D moves on to defense-specific applications, classified programs may become common.
- DOE and the National Aeronautics and Space Administration (NASA) would pursue their own mission-oriented projects, with most of the Energy Department's money going to the national laboratories. Much of DOE's support will probably go for R&D and demonstration projects directed at electric power applications. DOE and NASA might pick up a few projects of interest to DoD.
- NSF would continue to fund HTS in the universities, with some of the Foundation's support going to individual investigators and some to research centers.

Other ongoing shifts in U.S. technology policy would proceed along lines suggested in chapter 4:

- The executive branch will continue its efforts to open up the national laboratories, as well as to strengthen university-industry relationships and stimulate technology development and transfer through such initiatives as NSF's Engineering Research Centers (ERCs) and agency Small Business Innovation Research (SBIR) programs.
- The Administration would also continue to press for stronger intellectual property protection, both at home (process patents) and overseas (negotiations with foreign governments aimed at stronger laws and tougher enforcement].
- Some State governments would channel support, direct and indirect, to HTS as part of technology-based economic development programs,
- Venture-financed companies dedicated to HTS would continue to emerge. Private firms, both new and established, would negotiate collaborative R&D arrangements, nationally and perhaps internationally.

Over the next several years, the United States will continue to take the course outlined above. Will such a response, by itself, be adequate? The following analysis indicates that it will not.

## Strengths and Weaknesses

In many respects, the current approach to HTS illustrates the great strength of the U.S. system of technology development. Although superconductivity had become something of a scientific backwater by the mid-1970s, NSF for years supported people like Paul Chu at the University of Houston (an institution much like a hundred others below the top ranks in terms of research funding or prestige), and at least a few large U.S. corporations maintained small superconductivity research programs. Moreover, when HTS broke, American scientists could quickly take advantage of facilities ranging from neutron scattering equipment to the National Magnet Laboratory at MIT—facilities already in place, the result of years of Federal funding. Agencies with their own laboratories –DoD, DOE, NASA–began new R&D internally, while contracting out other work.

U.S. scientists not only responded quickly, but in large numbers, as measured by the flood of proposals to NSF, and papers published in professional journals and delivered at scientific meetings. This response reflects the sheer size of the pool of scientific and technical expertise in the United States—a notable strength. It also reflects the flexibility of the U.S. R&D system.

NSF was perhaps the most agile of the Federal agencies, moving quickly to provide funds –largely redirected–to individual research groups and to the Materials Research Laboratories (MRLs). NSF-funded investigators working in related areas were able to shift their attention immediately to HTS, because of the flexibility of Foundation grants.

Scientists with DoD and DOE contracts or grants were also able to move quickly. In addition, DoD redirected millions of dollars in a few months, as various defense agencies exercised their much-valued fiscal autonomy. Each went its own way, with a resulting diversity of technical approaches that is probably healthy overall. Likewise in DOE, laboratories competed to stake their claim in the newly discovered territory of HTS, resulting in an aggressive, if somewhat fragmented, effort, Interagency coordination, though largely informal, has been relatively effective: program managers and contract monitors working in superconductivity know one another and feel a shared sense of responsibility (box M, ch. 4).

Like government, venture capitalists reacted quickly to the new opportunities, lining up technical experts, many of them university faculty members, and quickly investing nearly $20 million in entrepreneurial startups (box G, ch. 3). These startups are just one illustration of close industry-university links in HTS—another significant asset of the U.S. system. The October stock market plunge led at least one HTS startup to cancel plans for a public offering but, overall, availability of capital has not been a major constraint.

In sum, U.S. R&D in HTS will continue to benefit from the unparalleled breadth, depth, flexibility, and diversity of the Nation's research system. It is easy to see why many people feel the current U.S. response to HTS is sufficient—at least for now. But weaknesses have also begun to surface, and others will probably appear over the next year or two.

Funding for basic research in HTS could be a problem, though probably not a serious one. As the ongoing flood of technical papers indicates, the scientific effort remains broad and intense. (At the March 1988 meeting of the American Physical Society, more than 600 of 3,500 papers presented dealt with superconductivity. Most were written by scientists based in the United States.) There are no obvious gaps in fundamental science: people somewhere are pursuing almost every possibility imaginable. More than likely, the ongoing university efforts will suffice to train enough people for industry's eventual manpower needs.

On the other hand, basic research in HTS may already have reached its peak, More sophisticated laboratory equipment will be needed to keep up in the future (e.g., for work on thin films), and costs will rise, Some of the investigators who used existing grants and contracts to move into HTS will have trouble getting new money to continue; they will have to show real promise, rather than routine results, to qualify for ongoing support. And even if there is enough money in total for basic research in HTS, the money might be better spent (box R). As noted earlier, the national laboratories have an HTS budget in 1988 roughly equal to that of NSF, an allocation that seems out of proportion, given the trouble universities have had getting funds.

Even if mission-oriented R&D funded by DOE and DoD were to transfer to the commercial marketplace, it would not do so immediately, The time lags made little difference in the 1950s and 1960s, when American companies were far ahead in technology. Today, the United States cannot afford to wait while know-how diffuses at its own pace from Federal laboratories to the private sector.

### Box R.—National Laboratories or Universities?

Postwar U.S. science and technology policy has relied heavily on the university system, with the national laboratories concentrating on mission-oriented R&D. Since the budget cutbacks of the early 1970s, many laboratories have sought to broaden their R&D—a trend that, arguably, has already cut into the share of Federal resources flowing to the universities. With DOE efforts to move beyond the big science role inherited from the Atomic Energy Commission (i.e., big physics) into fields such as mapping the human genome, it may not be much of an exaggeration to say that the Energy Department seems to be trying to become a general-purpose science agency.

This expansion raises issues of balance. Compared to the DOE laboratories, the university system is more open and supports a more diverse set of R&D projects. The universities operate with proven systems of self-governance, intellectual autonomy, and quality control through peer review. Bad science cannot hide for too long. Perhaps most important, industry's need for trained people gives the universities a special claim on Federal R&D funds. No other set of research institutions trains scientists and engineers for industry in large numbers. When these people move to the private sector, they take the latest knowledge with them.

While the laboratories' performance in technology transfer has certainly improved over the five years since the Packard Commission report (ch. 2), some of the remaining problems concern the amount of autonomy that should be given to mission-oriented facilities operated under contract to the Government. In addition, the laboratories are poorly positioned to deal with problems related to manufacturing. Laboratory personnel, unfamiliar with industry and the marketplace, often ignore the need to address processing early enough in development. University engineering departments have also fallen into this trap, but seem to be doing more to dig themselves out. Moreover, universities have been willing and able to work with industry; the DOE laboratories, until recently, have shown few signs of the flexibility needed to adapt their ways to industry's needs. At present, some people in industry view the laboratories with suspicion—and also as competitors for Federal R&D dollars.[1]

The laboratories *can* claim advantages over the university system—including a capacity for interdisciplinary research, sophisticated facilities, and experience with large-scale projects. But, as emphasized in chapter 4, even if policies put in place to change the laboratory system prove successful, the process will take time. Neither the universities nor the national laboratories should or could become centerpieces of a strategy for commercializing HTS. Their strengths lie elsewhere.

[1] See, for example, "National Labs Struggle With Technology Transfer," New *Technology Week*, Sept. 28, 1987; Allan S. Gelb (Director, Marlow Industries, Inc.), testimony to the House Subcommittee on Energy Research and Development, Committee on Science, Space, and Technology, Oct. 20, 1987.

Granted, the mission agencies are trying to address national concerns over competitiveness in their pursuit of HTS—e.g., through DARPA's processing R&D initiative. However, the near-term focus of the DARPA processing program in HTS may not take advantage of the agency's own strengths—funding of visionary research. Finally, DARPA must ultimately serve DoD missions, which means that when civilian and military needs diverge, commercialization will recede as an objective. Only if the agency can link its HTS R&D with military objectives will it find continued support within the Pentagon. Other problems aside, the program is relatively small—only about $18 million in 1988—and it was not fully underway as this report went to press,

In short, the lack of more direct mechanisms for Federal support of commercially oriented R&D has become a weakness in the U.S. approach to technology development. The issue is not overall funding levels for HTS. The issue is the allocation of those funds: mission-oriented R&D does not provide enough support for commercialization to ensure that American firms will be able to keep up in HTS.

The problem is particularly acute because of the wait-and-see attitude in much of American industry. As described in chapter 3, the Japanese are putting more effort into exploring applications. Furthermore, Japanese companies, with their strengths in engineering and manufacturing, would probably be able to catch up even if U.S. firms were first to reach the market with innovative products.

In sum, the current U.S. response to HTS displays the strengths and weaknesses that have characterized the performance of American companies in high technology. The U.S. effort looks formidable in the middle of 1988, but that is to be expected: the challenges so far have been largely matters for the research laboratory. If there is a surprise, it is that the Japanese—not known for innovation in science—have already posted such a strong showing.

As HTS moves toward applications, science will recede in importance. Basic research results, by their nature, will diffuse rapidly, providing little in the way of national advantage in commercialization. (Patent coverage sufficiently broad and strong to lock up a critical class of HTS materials seems unlikely.) Rather, the critical technological advantages are likely to reside in proprietary know-how associated with processing and fabrication techniques, and design-manufacturing relationships—precisely where Japanese companies have demonstrated an advantage over many of their American counterparts.

A continued response along the lines of Strategy 1 is quite likely to fall short: **the widely expressed fear that Americans will win in science, while the Japanese take the commercial markets could come true.** Support for science and for military technology—the essence of this strategy—served the United States well from 1950 to the middle 1970s. But the lesson of the past 15 years is clear: in a world of increasingly effective national competitors, these two levers no longer suffice.

## STRATEGY 2: AN AGGRESSIVE RESPONSE TO HTS

Three primary features distinguish this second strategy from the current approach:

1. A larger role for NSF, both in funding individual research at universities, and through the establishment of one or more university centers in superconductivity.
2. Federal cost-sharing of long-term, high-risk R&D planned and conducted by industry consortia.
3. A working group on commercialization of HTS charged with helping shape consensus on an R&D agenda, and making decisions on Federal cost-sharing.

This strategy preserves the strengths of the traditional U.S. approach to technology development, while compensating for the weaknesses brought out by stronger international competition —i.e., lack of breadth in industrial R&D, and heavy reliance on mission agencies for Federal support.

### Step One: A Larger Role for NSF

The initial step toward a more aggressive response to HTS should be straightforward: give the National Science Foundation more money for university research. For reasons outlined earlier in this report, a dollar spent by NSF should contribute more to commercial development, on the average, than a dollar spent by DoD or DOE. In view of this, NSF's existing 15 percent share of the total Federal R&D budget for HTS is too small.

There are two complementary ways for Congress to expand NSF's role, An otherwise unrestricted appropriation, earmarked for HTS—money that the Foundation could spend on superconductivity as it sees fit—would permit NSF to fund some of the highly rated HTS proposals that it is currently forced to turn away.

As noted above, OTA has found no evidence of serious underfunding in basic research on superconductivity. Nonetheless, with an NSF research budget that has been flat in real terms for several years, funds for condensed matter physics were cut back during fiscal 1988.[2] (Ch. 4, which discussed this and other symptoms of the pressure on the NSF budget at some length, included a number of policy options addressing the general problem.) As part of Strategy 2, Congress **might consider appropriating, say, $20 million (in additional new money) for** NSF for the 5-year period beginning **in fiscal 1989,** specifying that the funds go for HTS **re**search. Such a step would help ensure that the basic science underlying superconductivity gets adequate support, without cutting into budgets for NSF-sponsored R&D in other fields.

In addition (or as an alternative), Congress could **authorize and appropriate funds to NSF specifically for one or more university centers dedicated to HTS research,** To give NSF maximum flexibility, the centers could be established under one of several existing programs, or through a new program altogether.

While none of the Foundation's existing or proposed center programs (discussed inch. 4—see especially box N) seems ideally suited to the needs in HTS, an ERC comes closest. Although a number of the MRLs have good experimental facilities, and active research in superconductivity, industry involvement has rarely been a major goal. Nor are the proposed S&T centers, although emphasizing multidisciplinary research, likely to focus as strongly on industry interactions as the ERCs. While the program is relatively new, and as yet few of the ERCs have themselves demonstrated close working relationships with the private sector, their focus means the ERC program fits the needs in HTS more closely than other candidates. (None of the existing ERCs has a research agenda embracing HTS.)

Several bills introduced in the 100th Congress —e.g., H.R. 3048 and H.R. 3217—would instruct NSF to establish a program for interdisciplinary National Superconductivity Research Centers. Would a new center program for HTS —one with the explicit mission of building a strong technology base for commercialization, and one with teeth in the requirements for multidisciplinary work and industry involvement—do more for HTS than funding for one or more ERCs or S&T centers? The answer has to be yes, if Congress appropriates the money and if NSF moves relatively quickly. (The Foundation would have to solicit new proposals, while it already has proposals in hand for S&T centers on superconductivity,)

In sum, congressional funding for several (say, one to three) new multidisciplinary centers in HTS could represent a modest but important step. It would not be realistic to expect such a measure to expedite commercialization dramatically. University centers, even at major schools, would no doubt remain relatively small in scale and scope. Sums of $10 million to $20 million annually (perhaps $5 million, at most, per center) are about the maximum that make sense for an NSF center program—the Foundation does not do business on a scale much above this, Such centers would serve primarily as a source of new ideas and trained people—an important contribution to commercialization, not to be undervalued,

### Step Two: Industrial Consortia with Federal Cost-Sharing

As another element in a more aggressive strategy, **Congress could direct the Administration to partially offset the costs of joint industrial investment in long-term, high-risk R&D.** Such a policy—designed to address the gaps in HTS R&D in U.S. industry—would be based on two premises discussed at length in this report, First, just as the Federal Government supports basic research, it must bear part of the burden

[2] Overcom mit ments by the Division of Materials Research during 1987, in the expectation of a substantial budget increase, forced the cuts. Not only NSF, but DoD and DOE maintain that a considerable number of highly rated research proposals are going unfunded. The problem is not a new one, particularly for NSF. But the problem has gotten worse, and program managers understandably feel uncomfortable trying to draw lines between proposals that are almost indistinguishable in quality.

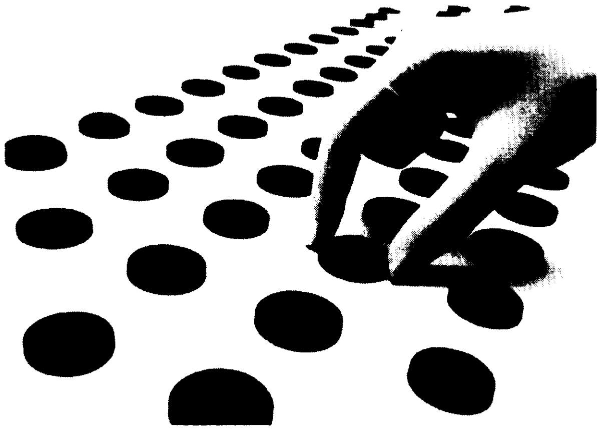

Photo credit: Superconductive Components, Inc.

**Pellets of HTS ceramic material**

of exploring risky, radical industrial technologies, which often provide large public benefits but only small private returns. Second, although DoD has borne much of this burden in the past, the growing specialization of defense technologies, and continuing pressure to meet the immediate needs of the services, mean that the United States now lacks a consistent champion for major new technologies with potential impacts on the civilian side of the economy.

Partial Federal support for one or more industry consortia—establishethe, as discussed in box S, to share R&D costs–is a simple and workable policy to address this problem. Financing some fraction of joint industry efforts with Federal funds (or tax expenditures) would pull more resources into applied research and generic technology development, and raise the overall level of R&D investment. The approach would direct public funds into areas that industry itself thinks will have the highest payoffs, but where the benefits would be difficult for individual firms to capture.

In some respects, the approach envisioned for HTS resembles that of Sematech, the microelectronics industry's new R&D consortium (to which more than a dozen firms have pledged 1 percent of their revenues). While providing substantial funding, the Federal Government would be a largely silent partner, with an R&D agenda put together and managed by member firms. And as with Sematech, there would be explicit linkages with universities and DOE national laboratories, so as to tie publicly-funded basic research to the joint R&D.

The differences between Sematech and the HTS R&D consortia envisioned for Strategy 2 are perhaps more important. First, the semiconductor industry itself proposed and fought for a Federally-supported venture. The companies likely to be involved in HTS consortia have made no such effort, and are not likely to; thus, the job of initiating and organizing such programs would fall in part on Government.

Second, DoD, through DARPA, serves as the financial channel to Sematech. A report by a Defense Science Board Task Force argued that the industry's troubles imperiled national security—one reason for DoD's oversight role. Moreover, DoD was perhaps alone among Federal agencies in having the technical expertise to monitor microelectronics R&D. Even if the DoD arrangement proves satisfactory in the case of Sematech, it holds small promise as a model for HTS. DoD—as well as DOE and NSF —could be involved with HTS consortia, but none of these agencies should oversee them, lest their purposes be subordinated to ongoing agency missions. This point is discussed further in the following section, which deals with institutional mechanisms.

Finally, Sematech has a focused R&D agenda, stemming from a consensus within the industry that manufacturing technology has been a major source of competitive difficulty. Rather than a single, well-defined focus, HTS lends itself to multiple agendas, different sets of participants, Three among many possible candidates:

- Electric *Utility* Applications.—Utilities normally make highly conservative investment decisions. They are unlikely to adopt a new technology until sure it will work reliably for many years. Thus, there may be a useful role for Government in accelerating the development of the engineering database and field service experience in HTS through support for cooperative projects (which could involve DOE laboratories),

## Box S.—Collaborative R&D[1]

The breakthroughs in superconductivity brought forth many proposals echoing the theme of strength through collaboration. The President's initiative urged Federal agencies to cooperate with universities and the private sector. Legislation has been introduced in Congress with similar objectives. In part, these calls for collaboration represent a response to rising R&D costs and the loss of U.S. technological advantage. To some extent, they stem from a misapprehension of the sources of Japan's success (the myth of cooperation discussed in ch. 3).

Although joint research is nothing new, the past decade has seen a steady growth in U.S. R&D consortia, and a marked change in research focus. Firms that once cooperated only on matters such as technical standards have increasingly banded together to undertake pre-competitive R&D—projects on new technologies with direct commercial relevance. The best-known—the joint venture Microelectronics and Computer Technology Corp. (MCC)–has begun exploratory work on HTS.

The economics of joint R&D hold considerable appeal. By pooling resources and avoiding duplication, cooperative research increases the potential leverage of each firm's R&D budget, while limiting the costs to any one firm of a failed project. Joint efforts also enable participants to monitor developments in a technical field without developing a full capability in-house.

While the benefits of cooperative research are sizable, so are the limitations. Most important, joint endeavors cannot substitute for in-house R&D efforts, only complement them. The participating firms must absorb the results, and transform them into commercially relevant products or processes—something that requires a sophisticated and independent internal effort. For such reasons, government efforts to encourage cooperative research programs in industries where firms pursue little or no R&D on their own have seldom succeeded.[2]

A second limitation may prove more serious. Ideally, cooperation in research should help lengthen project time horizons; but many of the participants in cooperative ventures shift R&D strategies in two ways: 1) by focusing their internal research on still shorter-term work; and 2) by seeking to move the cooperative's agenda away from basic research and toward more applied projects. The trend has been evident in MCC, which recently restructured its largest program—in advanced computer architectures—to emphasize more immediate paybacks.

Collaborative efforts involving universities (or government laboratories) have an easier time focusing on long-term research, often with little interference from participating firms, because the firms are not seeking specific R&D results so much as access to skilled graduates and faculty expertise. Of course, the financial commitments are generally much smaller than those for participation in a joint venture such as MCC (and may be viewed in part by the firm as good corporate citizenship). While cooperative R&D programs housed in universities help maintain a strong technological infrastructure, they should not be viewed as engines of commercialization.

Finally, the sheer difficulty of organizing and managing a collaborative research venture creates its own set of limits. Fundamental issues—reaching agreement on an R&D agenda, finding ways to share technologies and business information, controlling costs and determining intellectual property rights—can pose enormous obstacles, particularly when the collaborators are also competitors. These are among the reasons that cooperative research accounted for only $1.6 billion of the more than $50 billion spent by American industry on R&D in 1985. Of this total, 85 percent ($1.4 billion) went to support R&D cooperatives in the communications, gas, and electric utility industries, whose members do not compete directly. It is no surprise to find that industry leaders rarely put cooperative R&D very high on the list of steps needed for rebuilding U.S. competitiveness.[3]

Joint R&D has a role to play–in HTS and in solving the more general problems visible in the U.S. technology base–but that role will inevitably be circumscribed by tensions between competition and cooperation among the participating companies. Firms seek proprietary technologies in order to compete with one another. Cooperation between firms in the same business can only go so far; cooperation between firms having supplier-customer relationships, or those in different industries with common R&D goals, holds more promise.

[1]See "Collaborative Research: An Assessment of Its Potential Role in the Development of High Temperature Superconductivity, ' prepared for OTA by D.C. Mowery under contract No. H36730, January 1988.

[2]NSF's Industry/University Cooperative Research Center program (IUCR, box O, ch. 4) was initially designed to substitute for in-house R&D in technologically moribund industries. An NSF evaluation found that "Companies with little research background, such as the utilities and furniture companies, are traditionally conservative with respect to new technology and depend on their suppliers for whatever changes they adopt. " ***An Analysis of the National Science Foundation's University-Zndustry Cooperative Research Centers Experiment*** (Washington, DC: National Science Foundation, 1979). IUCR programs have gone on to more success in other industries.

[3]For the results of a recent survey of corporate executives, see "The Role of Science and Technology in Economic Competitiveness, " final report prepared by the National Governors' Association and The Conference Board for the National Science Foundation, September 1987, pp. 20-30. The R&D spending figures for cooperatives come from P.F. Smidt, "US Industrial Cooperation in R& D," remarks at the Annual ESPRIT Conference, Brussels, Belgium, Sept. 25, 1985.

- *HTS Magnets.*–Strong magnetic fields, by dampening thermal fluctuations, can give purer, more uniform crystals of silicon and other semiconductor materials, making this a natural candidate for superconducting magnets. R&D directed at HTS-based magnets could, at the same time, help move HTS out of the research laboratory and lessen U.S. dependence on foreign sources of semiconductor wafers. X-ray lithography using compact synchrotrons—a promising candidate for making next-generation integrated circuits—likewise could serve as a spur for HTS R&D while filling a chink in U.S. microelectronics capabilities.
- *Superconducting Computer Components.*—HTS interconnects may help improve the performance of computers. Hybrid semiconducting/superconducting electronics may also prove viable. Collaborative projects on prototype circuitry, with subsequent Federal purchases of very high-performance machines if the technology panned out, might help speed commercial development of HTS, while at the same time preserving the U.S. lead in high-end machines. Such a program would have a substantial basic research component, but involve manufacturability and applications issues as the technology began to mature.

Whatever the substantive agenda, the private sector should take the lead, insofar as practical, with Government participation limited to that necessary to achieve the Government objectives–leveraging critical R&D, filling gaps in the technology base, lengthening time horizons. Federal cost-sharing justifies such procedural rules as these:

- *Government participation should be conditional on significant investment by industry—say 50* percent or more. (The Sematech formula—40 percent from both industry and DoD, and 20 percent from State and local Government—provides an alternative.) The less of their own money companies contribute, the greater the risk of R&D that strays from marketplace needs. So long as Government funding decisions can be limited to choosing among alternative approaches, all of which industry is prepared to back, some of the problems of picking winners can be skirted.
- By also requiring, as a minimum, *a 3-year financial commitment on the part of consortium members,* the Government will have added assurance that public funds will help support medium- to long-term R&D. (MCC requires only a l-year commitment, down from 3 years initially; this change is both cause and consequence of pressures for tangible early R&D results.)
- *Companies should be either in or out.* MCC, which permits members to join projects selectively, has found itself trying to wall off some of its work to prevent R&D results from leaking to companies that have not joined a particular project.
- *Entrance requirements should be transparent,* lest the consortium become a smokescreen for anti-competitive behavior; any eligible U.S. firm should be able to join (those without majority U.S. ownership might reasonably be barred).
- people transfer technologies most effectively. To see that knowledge flows out of the consortium, *employees of the member companies should be heavily represented among the R&D staff.* It seems reasonable to insist that half or more of a consortium's staff come from member companies (rather than new hiring). Assignments might be temporary, but should be long enough—perhaps 6 months as a minimum—for meaningful contributions to R&D, and for learning purposes.

  Furthermore, member firms should be encouraged to send their best people. MCC dealt with this by retaining—and exercising liberally—the right to reject employees sent by shareholders (initially, it turned down 95 percent).[3] University involvement can also help attract the best industry scientists.
- No less important if the consortium is to affect commercialization, *member firms must conduct ongoing complementary R&D*

[3]B.R. Inman, "Collaborative Research and Development," ***Commercializing SDI Technologies,*** **S.** Nozette and R.L. Kuhn (eds.) (New York: Praeger, 1987), p. 65.

*of their own.* MCC encourages "shadow research" by members, paralleling the joint venture's work. This ups the ante for members; Digital Equipment Corp. spends half again its investment in MCC seeking ways to use the consortium's results—a level of commitment that has, however, been rare. Parallel efforts will be particularly important for firms with little or no experience in superconductivity. They will have more learning to do than companies with backgrounds in, say, LTS.

- If one purpose of Federal funding is to stimulate visionary R&D, *it may make sense to discourage too much publicity,* By reducing the pressures—political and other—for short-run success stories, the chance of success stories over the longer term should go up.

  A long-term orientation also suggests that a consortium's work stop well short of full-scale commercial development efforts—i.e., at the prototype stage, leaving further development to the members' own efforts.

### Step 3: A Working Group on Commercialization

The final step in the aggressive strategy would be to establish a working group of experts—drawn from industry, universities, and government—with a limited mandate to promote the new technology. Such a group—with a lifetime fixed at, say, 10 years—could serve a number of important functions.

The first is *fact-finding and analysis.* Currently, no public or private body has a continuing responsibility to provide authoritative policy guidance concerning such questions as: What problems do we need to start on now to assure rapid commercialization? How much money will it take? Are some HTS R&D areas getting too much money? Which areas are not getting enough? While such questions never have definitive answers, the first step toward good decisions—given that the Federal Government will have to make decisions in any case—is to understand what is going on in both government and industry, here and in other countries. The problem is not inadequate coordination. Rather, the problem is that no one has the task of drawing even a crude map of the road to commercialization, and setting the necessary priorities along the way. To do so will require solid and timely analysis, on a continuing basis. (The President's Wise Men's advisory committee on HTS will evidently prepare a one-time report, rather than provide ongoing policy guidance,)

While the working group's responsibilities would involve decisions on Federal cost-sharing in response to proposals from private sector consortia, it might first have to engage in *consortium-building and facilitation.* In contrast to Sematech, HTS consortia are not likely to organize themselves spontaneously, at least until guidelines for Federal cost-sharing have been set down. The working group might be able to play a match-making role, helping bring together companies, universities, and government laboratories, and aiding them in reaching consensus on research needs—a function that could continue even after the joint R&D effort was underway; as the experience of Sematech and many other joint ventures demonstrates, conflict will be inherent in any consortium of independent firms,

The working group's ability to get things done will flow in part from its power to *allocate Federal resources.* Decisions on who is to get Government money will hinge not only on technical questions, but on economic judgments. Technical evaluations of competing projects can rely on the tried and tested approach of external review by recognized experts without a stake in the outcome,

Evaluating the economic merits—i.e., likely impacts on commercialization—would be more difficult, but the problem can be sidestepped to considerable extent by using procedural rules to allocate public funds. First, a consortium's proposed R&D agenda would have to meet minimum criteria, which the working group would set, based not only on technical merit, but, as discussed above, on rules for participation, and provisions for member funding and R&D time horizons, Then, among the qualifying proposals, funds would go to the "highest bidders" as measured by length of time commitment participants in the consortium were willing to make, and the fraction of total costs they were willing to self-finance. (A third criterion—the investments of members in complementary internal R&D—is also relevant, but it would be difficult to determine what was "complementary" and what was not.) These measures should help to allocate government resources to joint R&D projects with the longest term payoffs, and the greatest value to industry.

If the working group makes decisions on funding, the issue of administering such a program remains. As an ad hoc body outside the ordinary apparatus of Government, the working group would need to look to an existing agency for help with staffing and managing its responsibilities. This poses a dilemma. As discussed above, the working group is not simply an advisory body, but a center for decision-making on commercialization policies. Yet only a minority of its members would be Federal employees, it would go out of existence after some period of years, and the intent is not only to complement the activities of existing agencies but, to considerable extent, to substitute for the agencies—to undertake tasks that they do not (and perhaps cannot). Attaching the working group, even for administrative convenience, to an existing agency could undermine its impact.

Each of the three agencies heavily involved in funding HTS R&D—DoD, DOE, and NSF—has noteworthy strengths: experience in funding LTS research, technical competence, and the administrative tools needed for monitoring the expenditure of Government funds. But each has flaws as well: DoD's military mission will always come first; later if not sooner, commercial technology development will probably devolve into a secondary objective of consortia with Pentagon involvement. DOE has less experience with the private sector than DoD—e.g., in managing extramural R&D—and a narrower base of technical expertise. For NSF, the assignment would be a substantial departure from the norm; the Foundation has limited ties to the private sector, and few employees with industrial experience. Its past attempts to foster applied research have not met with great success.

Are there other possibilities? The Commerce Department is seldom seen as a technology agency. The Office of Science and Technology Policy (OSTP), as pointed out in the preceding chapter, has a small staff and is not set up to handle the kind of tasks the working group would need to pass along, (A Federal technology agency, as described in the final section of this chapter, might be well-suited; but even if Congress were to pass legislation creating such an agency, it would not be ready in time to serve the working group.)

In the end, the best solution is probably to set up the working group as an ad *hoc* independent body, with a small staff of its own, and attach it to OSTP. As such, it should have the necessary qualities for promoting commercialization of HTS: the flexibility and substantive depth to learn by doing, tailoring its procedures to the special needs of HTS as these became apparent,

The mission agencies will take care of their own needs in superconductivity. What is lacking is an organization to look after the broader national interest in commercializing this new technology. Federal support of joint private-sector R&D investments addresses the need. This is not the only alternative—box T summarizes others—but it seems a promising one.

## STRATEGY 3: A FEDERAL TECHNOLOGY AGENCY–THREE ALTERNATIVES

The Federal structure for science and technology policy has changed little since the late 1950s. Within DoD, the Office of Naval Research set the post-1945 pattern for support of R&D. In 1950, after prolonged debate, Congress passed the authorizing act for the National Science Foundation. The same year saw major new legislation setting the National Institutes

## Box T.—Aggressive Support for Commercialization: Other Possibilities

### Flagship Projects

The symbolic and potential economic importance of HTS have led to calls for a flagship approach. With HTS already a symbol, among other things, of U.S.-Japan economic competition, advocates of the flagship alternative see virtue in Government initiatives that likewise have symbolic value. A flagship should rally industry, universities, and the public sector, building on the enthusiasm created in the media and galvanizing the Nation's creative and entrepreneurial vigor in a race, not to the moon, but over the hurdles of some earthbound Olympics—an Olympics of science, technology, and competitiveness. This country's strength is in meeting crises–the Manhattan project, Sputnik—not in incrementalism, say advocates of this approach. Political consensus, and a bold national effort, could pull superconductivity out of the laboratory and into the global marketplace. Visibility can be a strength as well as a potential source of weakness.

As appealing as such images might seem, HTS is not the kind of technology that lends itself to a massive, concentrated effort. Although bills have been introduced dealing with magnetically levitated trains, public and political attitudes toward rail transportation would probably have to change a good deal before maglev would have broad appeal. Talk of an energy crisis evokes little response today. Superconducting computers will just be black boxes; no one much cares what is inside. Defense systems do not fill the bill either.

There is another dilemma. A flagship has visibility. It must succeed, if only for such reasons. This forces technological conservatism on decisionmakers. Apollo's achievements were in systems engineering and large-scale project management, not in revolutionary technologies; space is no place for trying out unproven technologies. Pressures for success—or pressures to avoid the appearance of failure—mean safe choices by the managers of such projects. If the goal is Government support for long-term, risky technology development, the flagship approach has little to offer.

### DoD Processing R&D

Processing will be vital in commercializing HTS, and several legislative proposals have made it a central element. For example, H.R. 3024 would authorize $400 million for a 5-year, DARPA-centered effort also involving DOE, NSF, and NBS.

DARPA has left a deep imprint on U.S. high technology, most of all in computers. Chapter 4 discussed DARPA's current HTS processing initiative—an effort that could fill an important gap in superconductivity R&D. Giving DARPA the lead in a more ambitious effort, as in H.R. 3024, might seem appropriate, On the other hand, programs aimed explicitly at (non-defense) commercialization fall well outside DARPA's historical mission and experience; in part because it seems to critics in the Pentagon too far removed from military needs, DARPA's HTS program has not found widespread support inside DoD. Congressional enthusiasm—reflected in a direct appropriation—made the program possible, but if Congress loses interest, the program could fade away. Only if DARPA could link the R&D with military objectives would it get internal support in the face of budget pressures and competing DoD demands.

In the past, such pressures have periodically led DARPA to abandon longer-term R&D and embrace more immediate military objectives. DARPA's transfer of the MRLs to NSF marked the first of these periods (box O, ch. 4). After the end of the Vietnam War, fatter R&D budgets enabled DARPA to move back toward long-term research. But in the early 1980s, the pendulum swung once again, with the Strategic Computing Program a prime case in point; DARPA has channeled much of the program's funds to military contractors, rather than the university laboratories responsible for most of its earlier successes in computing technology, and set program objectives that will appeal to the services. Ironically, over the last few years, DARPA has behaved much like American corporations—stressing projects with quick payoffs. Finally, there seems little question that major breakthroughs in HTS resulting from DoD-sponsored R&D would be classified, should the Pentagon feel that, as a practical matter, they could thereby be kept from the Soviet Union.

### The DOE Laboratories

Although no one has formally proposed the designation of a lead laboratory for HTS, the suggestion has been in the air, There are at least 10 DOE laboratories with work of one sort or another underway in HTS.

The chief argument for greater concentration and centralization is one of efficiency: with all the laboratories on the HTS bandwagon, duplication of effort will be hard to avoid. By giving a clear mandate to one, DOE should be better able to manage the division of labor. On the other hand, with HTS remaining primarily a matter of research—research with fuzzy objectives—centralization for its own sake has little to offer. The conventional management wisdom that basic research is cheap, the benefits of competition among scientists great, makes central control unnecessary and undesirable. Nor is there an obvious candidate for a lead laboratory in HTS.

of Health on a course it has continued to follow.' At the end of the 1950s, the Soviet Sputniks spurred another set of changes: the establishment of NASA and DARPA. NASA grew out of the National Advisory Committee for Aeronautics (NACA). Founded in 1915 to conduct research and testing, NACA remained small until World War II, when its staff grew to nearly 7,000 people. NASA's staff eventually reached five times that level, while its budget grew even faster (NASA contracted out much more of its work). DARPA, setup in 1958—and originally given the mission of developing a U.S. space program, later passed to NASA—quickly established itself as the home of long-range R&D within the Defense Department.

Since this period, the Federal R&D budget has grown steadily, but organizational changes have been minor. In 1950, the Federal Government spent about $1 billion on R&D. Today, half a dozen Federal agencies each spend over $1 billion annually, and more than a dozen others spend lesser amounts. Given this growth in R&D spending, and the increasing concern over the Nation's ability to utilize its technology effectively, many proposals to reorganize Federal science and technology functions have come before Congress during the 1980s. (This is nothing new: in 1913, during the debate preceding the formation of NACA, some of the opponents of a new organization for aeronautics research saw it as a stalking horse for a cabinet-level science department.)

## An Umbrella Agency for Science and Technology

The more ambitious sounding proposals often call for a science and technology (S&T) agency to consolidate Federal R&D functions. Advocates of consolidation argue that an umbrella organization would lead to clearer priorities, less duplication, and greater efficiency—in a word, to better management. They point, for instance, to the more than 700 national laboratories, managed, often quite loosely, by many different agencies, and note the frequent criticism that the laboratory system has come under.

In fact, calls for a Department of Science and Technology tend to be a bit misleading. Because the mission agencies control most of the Federal R&D budget, the resulting changes would necessarily be modest. When the Young Commission called for a cabinet-level S&T agency in 1985, it offered no guidance on how such a proposal might be implemented.[5] The problem is clear enough. Some 70 percent of Federal R&D goes for defense and space. Much of the rest pays for health-related research.

It is hard to envision moving more than a few bits and pieces of DoD's current R&D–say a billion dollars or so—into another part of Government. Moreover, since creating the Atomic Energy Commission in 1946, Congress has kept nuclear weapons research isolated; currently, nuclear weapons account for about half of DOE's R&D budget.

The second largest R&D agency, Health and Human Services (DOE is third), operates a research arm—NIH—with a hundred-year tradition of excellence. Why risk disrupting organizations like NIH in the name of management efficiency?

Even the strongest advocates of an S&T department acknowledge that consolidation could not go too far without harming the ability of agencies to manage R&D in support of their own missions. But without pulling much of the R&D that is currently the responsibility of these agencies under the new S&T umbrella, there

[4]See, for example, J.A. Shannon, "The National Institutes of Health: Some Critical Years, 1955-1957," *Science,* Aug. 21, 1987, p. 865.

On NACA and NASA, below, see F.W. Anderson, Jr., ***Orders of Magnitude:A History of NACA and NASA, 1915-1980,*** NASA SP-4403 (Washington, DC: National Aeronautics and Space Administration, 1981); and A. Roland, ***Model Research: The National Advisory Committee for Aeronautics, 1915-1958,*** vol. 1, NASA SP-4103 (Washington, DC: National Aeronautics and Space Administration, 1985); also "Collaborative Research: An Assessment of Its Potential Role in the Development of High Temperature Superconductivity," prepared for OTA by D.C. Mowery under contract No. H36730, January 1988, pp. 29-34.

[5]*Global Competition: The New Reality,* vol. I (Washington, DC: U.S. Government Printing Office, January 1985), p. 51. The Commission simply said that the department should include "major civilian research and development agencies. "

For extensive discussion of proposals for an S&T agency, see the special issue of *Technology In Society,* vol. 8, Nos. 1/2, 1986, entitled "A Department of Science and Technology: In the National Interest?"

would be little left, It would be hard to take seriously a cabinet-level S&T agency that would oversee perhaps 10 percent of the Federal R&D budget.

Given this dominance of R&D by the mission agencies, most of the legislative proposals for reorganization have had quite modest objectives. H.R. 2164, for example, is fairly typical. This bill—introduced in the 100th Congress to create a Department of Science and Technology—would pull together NSF and the National Bureau of Standards (NBS), together with several smaller Commerce Department programs, while also also creating a National Bureau of Technology Transfer and an Advanced Research Projects Foundation. The latter—charged with supporting generic, industrial R&D—represents one variant of a recurring proposal—a proposal that, according to the analysis below, has more in its favor than an umbrella agency for science and technology.

## Higher Priorities for Technology and Engineering

Proposals for a technology agency that would stand alongside NSF—perhaps called a National Technology Foundation (NTF)—start with the premise that technology does not always depend on science. Indeed, development of new technology—a goal-directed, problem-solving activity—differs fundamentally from scientific research. Even where the interrelationships are close, as they often are in high technologies, the two activities depend on different kinds of people, with different skills and expertise. Science seeks understanding. Technology seeks satisfactory solutions to practical problems. Science looks to technology for tools—computers to unravel the structure of DNA, or to guide powerful telescopes as they scan the heavens. By the same token, technology looks to science for tools: knowledge of DNA leads to new pharmaceutical products; theoretical insights into computer software now guide the design of hardware.

U.S. problems in commercialization lie in technology, not science. And while scientists make their contributions to innovation and

*Photo credit Biomagnetic Technologies Inc*

**Brain scan using equipment incorporating superconducting quantum interference devices (SQUIDS)**

competitiveness, the engineering profession carries much of the burden (ch. 2). **Raising priorities within Government for engineering research—work directed at technology rather than science—would be a straightforward and positive step toward renewed competitiveness.**

NSF has made considerable progress at this in recent years, most notably through its ERC program, while amendments to the NSF charter have also given engineering more prominence. Moreover, the Foundation's current director, Erich Bloch, who came to NSF from industry, has provided strong leadership for initiatives such as the ERCs. But Bloch will not be there forever. And NSF's fundamental job is the support of science—science for its own sake. The Foundation cannot tilt too far toward engineering without provoking a strong reaction from its primary, and well-organized constituency—university scientists. Indeed, the ERCs have already provoked such a reaction. NSF is unlikely to shift its priorities much further unless pushed from the outside.

Currently, about 10 percent of NSF's budget goes for engineering. It is hard to envision the Foundation, as presently constituted, increasing the proportion for engineering to more than 15 or 20 percent. The Engineering Directorate's budget has been spread among several thousand departments in the Nation's nearly 300 engineering schools. With relatively low levels of support from NSF, faculty have turned to DoD for money, skewing research toward specialized military problems. (This trend affects curricula and course contents as well, although less directly.)

An NTF, independent of NSF and DoD, could be a powerful lever for moving university research back toward the civilian side of the economy, and for steering engineering education back towards practical industrial problems. Nonetheless, OTA's past analyses have found restructuring NSF—making it, say, into a National Science and Technology Foundation—to be a more attractive option than creating a separate National Technology Foundation.[6] Science and engineering *do* depend on one another. Thus, **an integrated agency, charged with supporting both engineering and science, makes more sense than two parallel agencies—provided sufficient resources can be guaranteed for engineering.**

In this variant of Strategy 3, with the focus on engineering research in the universities, impacts on commercialization would be long-term and indirect—both a strength and a weakness. Government money would not go directly for commercial technology development in industry, avoiding the problems such a step would raise. But it would take time before the resources flowing to new research in engineering could make a difference for competitiveness and commercialization. In particular, creating an NTF would not do much for HTS.

[6] "Development and Diffusion of Commercial Technologies: Should the Federal Government Redefine Its Role?" staff memorandum, Office of Technology Assessment, Washington, DC, March 1984.

Other suggested reorganizations would target industrial technologies more directly. Box U, for example, discusses one recent proposal, in this case a reorientation of NBS.

## An Agency for Commercial Technology Development

For years, DARPA has enjoyed an enviable reputation: an elite band of non-bureaucrats, able to pick technological winners and drive them forward. Why not, many have asked, do the same on the civilian side of the economy? The response follows just as quickly. DARPA can pick winning technologies because it has a reasonably clearcut mission, whereas a civilian DARPA would have a much fuzzier charge. Nonetheless, at a time when U.S. industry has lost ground competitively, the notion of a civilian DARPA holds considerable appeal—an agency devoted to championing high-risk, long-term projects, technologies that could make a real difference in international competition, The huge U.S. trade deficit, and especially the imbalance with Japan, is today's Sputnik. Somebody has to do something.

DARPA has, in fact, been able to anticipate technologies important to civilian industry. Although commercialization *per se* has not been DARPA's goal, for most of the agency's history the defense mission has not tightly constrained its decisions. Rather, DARPA has invested in what it regarded as high-payoff technologies, on the rationale that DoD would ultimately benefit as a purchaser.

If DARPA can make make technically sound decisions, a civilian agency should also be able to do so. But the DARPA analogy can be taken only so far. A civilian DARPA, by its nature, would be much more difficult to run efficiently: nurturing new technologies intended to succeed in the marketplace is a more complex and exacting undertaking than supporting a technology for which the Government is the end user. In addition, a civilian DARPA would have high visibility politically. Technology development is now seen as the *sine qua non* of economic prosperity. This means that a civilian DARPA would be under strong pressure from

### Box U.—An Advanced Technology Program

Among reorganization proposals, the proposed Technology Competitiveness Act has come closest to implementation. The bill—incorporated in the omnibus trade package passed by Congress in the spring of 1988 (and vetoed by the President)—would:

- rename the Commerce Department's National Bureau of Standards (NBS) the National Institute of Standards and Technology;
- authorize regional centers for the transfer of manufacturing technology;
- provide for technical assistance to State technology programs;
- establish a clearinghouse on State and local initiatives on productivity, technology, and innovation;
- create an Advanced Technology Program as part of the revamped NBS.

The technology transfer and State assistance provisions in the bill could be useful. But it is the Advanced Technology Program (ATP) that would most directly address U.S. needs for industrial technology.

The ATP would assist businesses in applying generic technologies, and in research needed for refining manufacturing technologies and for rapid commercialization of **new** scientific discoveries. This would be accomplished through, among other things, aid to joint R&D ventures (including ATP participation in such ventures under some circumstances). The bill also authorizes cooperative agreements and contracts with small business, and involvement of the Federal laboratories in the program. Although the trade bill itself does not appear to specifically authorize appropriations for the ATP, a predecessor bill in the Senate (S. 907) would have authorized $15 million for the ATP in its first year.

special interests, States, and Congress itself to steer resources to particular projects, In other words, a civilian DARPA could easily become a pork barrel. Political interests could override economic sense.

Could a Civilian Technology Agency (CTA) avoid these pitfalls? What might it look like, and what it would do? In many versions it would be small and lean (DARPA's staff numbers about 125), emphasizing flexibility and making use of experienced professionals on temporary assignment from the established mission agencies, In more expansive alternatives, a CTA might pull in relevant functions from elsewhere in Government, such as support for university-based engineering research (from NSF), a technology extension effort, and perhaps aerodynamics programs (from NASA). Box V outlines one recently proposed agency. Beyond questions of size and scope, a CTA's effectiveness would depend heavily on four questions: 1) its mission; 2) project selection and monitoring; 3) the quality of its staff; and 4) intramural research.

#### Mission

The CTA's central mission would be to extend the time horizons of U.S. industrial R&D, and help fill some of the gaps in the Nation's technology base. More specifically, it would be responsible for supporting two rather different kinds of work. The first is long-term, *high-risk 17&D* at pre-commercial stages, with the goal being relatively dramatic advances in technology. For example, candidate projects in the manufacturing area might include direct reduction steelmaking, or expert systems for shop-floor production scheduling. HTS examples could begin with three-terminal electronic devices, or integration of semiconductor and superconducting electronics,

The second area is *generic technology development*, which would typically be incremental.

**Box V.—An Advanced Civilian Technology Agency, as Proposed in S. 1233**

**S. 1233—the Economic Competitiveness, International Trade, and Technology Development Act of 1987—reported by the Senate Governmental Affairs Committee in 1987. It was then incorporated in that** house's omnibus trade bill, **later to be dropped. S. 1233 is of interest here because of its provisions for an Advanced Civilian Technology Agency (ACTA), which would have been part of a new Department of Industry and Technology-the latter created through a major reorganization of Federal Government responsibilities.**

**The ACTA provisions in Title I, Part III of S. 1233 represent the closest. that Congress has yet come to implementing some form of civilian DARPA.[1] Intended to support technology development and commercialization through contracts and grants, cost-shared with industry, the agency would give particular attention to risky, long-term projects. Technology-related functions transferred from the Commerce Department, including NBS, would stand alongside the ACTA (rather than becoming part of it). The bill authorized an ACTA budget big enough to make a difference—$80 million in the first year, rising to $240 million in the third year. Financial support to industry would be permitted through the stage of prototype development.**

**S.** 1233 would provide for a high-level outside advisory board, but **in most other respects leave agency operations up to the Secretary of the new department and** his **or** her deputies. Report language **calls** for **a small** professional staff (35, initially), coming largely from industry, with considerable **use of scientists and engineers on loan from the private sector.**

[1]As explicitly stated in *Economic Competitiveness, International Trade, and Technology Development Act of 19S7: Report of the Committee on Governmental Affairs, United States Senate, To Accompany S. 1233*, Report No. 100-82 (Washington, DC: U.S. Government Printing Office, June 23,19871, P. 10. In Section 122 of the bill, the agency is directed to coordinate its activities with those Of both DARPA and NSF, the directors of which are to be members of its advisory board.

Many projects here would aim to reduce research to practice. The work would help many companies, but would rarely lead directly to proprietary advantage. Examples (again from manufacturing): nondestructive evaluation techniques, especially those suited to real-time operation as part of feedback control systems; small hand tools for mass production that are ergonomically designed for ease and speed of use (something that gets little attention in the United States compared to Japan and Europe). HTS examples include processing of the new ceramic materials, and magnet design and development for applications such as separation of steel scrap, or refining of ores.

Why is mission so important? Because it is a precondition for accountability. DARPA's mission creates discipline over the decisions of its staff and managers: only so long as DARPA can show that the work it funds will support future military requirements can the agency expect support from the Office of the Secretary of Defense and the relevant committees in Congress.

For a civilian agency, vague statements concerning commercialization or competitiveness will not do. Lack of agreement on mission is one of the reasons why none of the many bills introduced over the years to create a new technology agency has become law. Consensus on mission is critical for any agency with substantial budget authority—and given the size of the U.S. economy, and the needs for industrial technology, a CTA would have to have an annual R&D budget of $100 million or more for meaningful impact (DARPA's current budget is about $800 million).

Whatever form a CTA might take, it would never have a mission as clearly defined as that of DARPA or NASA. The overall goal—supporting commercial technologies in order to support the international competitiveness of U.S. industry—does not lend itself to neat and clean decisionmaking. Competitiveness is difficult to measure, harder to predict, and depends only partially on technology. Many people and many groups may view competitiveness as a legitimate goal. But as a practical matter, it would not be possible to judge the merits of a CTA's work—or evaluate the outcomes of completed R&D projects—by linking that work to competitiveness. The linkages are too loose, the causal connections often spanning many years. Even so, it should be possible to define the technological objectives of a CTA tangibly enough to provide a handle on mission.

### Project Selection and Monitoring

With the charge of supporting commercial technology development, much of a CTA's budget would have to go for contracts with the private sector (including consortia), on a cost-shared basis. Some money might also flow to non-profit laboratories, and to universities through grants and contracts, But the point, after all, is to channel direct support to industrial technology, supplementing the many indirect measures the Federal Government already calls on.

A CTA would not be able to rely exclusively on review panels or outside experts to develop an overall strategy—a broad view of where resources should go—or for help in setting priorities. Outside experts, by definition, have a narrow view. The further science and technology advance, the greater the specialization among experts. The CTA would have to depend on the collective judgment of its own staff for strategy and priorities.

How about project-specific decisions? Money for private firms raises questions. The agency would have to choose projects on grounds that would be accepted as fair. Again, the answer begins with a competent staff, combined with merit review processes (box W).

### The CTA Staff

Federal support for commercial technologies will always run the risk of devolving into little more than a program of subsidies for industry, with much of the money going to marginal projects, The primary guarantee against that danger is to staff the CTA with professionals who have the independence of judgment and the technical knowledge to make good decisions and stick to them,

To gain the respect of their industrial counterparts, CTA employees—technical specialists, program managers, administrators—would need a good grasp of market realities, as well as of industry's technical requirements. They would need to function as part of a peer group that includes industrial scientists, engineers, and R&D managers.

If the agency's managers were to provide exciting work, give employees substantial responsibilities, and maintain a selective and competitive personnel policy—more like DARPA or the Office of the U.S. Trade Representative than the Commerce Department—a CTA should have little trouble in assembling a capable staff, Finding people with strong technical credentials is relatively easy (American universities excel at deep but narrow training of engineers and scientists). Breadth and experience—industrial experience, in particular—are harder to find, Bringing in people from industry might require exceptions to normal civil service requirements,

Given that the Federal Government already employs many highly competent engineers and scientists, the CTA could begin by assembling a core staff borrowed from other agencies. Continued use of detailees would ensure a steady flow of fresh perspectives, while also helping with inter-agency coordination and technology transfer. Industry sabbaticals that sent CTA employees to the private sector for periods of 6 months to 2 years could help serve the same purpose (as suggested inch. 4 for national laboratory employees),

### Intramural R&D

Although most of its projects would be contracted to industry, it would also seem desirable for a CTA to carry out in-house R&D in its own facilities. This need not be a large-scale undertaking (say, 5 percent of the agency's budget). But it would allow staff members to keep their hands in. Some technical employees might rotate through the CTA's laboratories. Others could spend part of their time engaged in R&D more or less continuously.

## Box W.-Project Review

**When it comes to selecting projects for extramural funding, Federal mission agencies rely primarily on expert technical reviews.** DoD **conducts many of its technical reviews internally, but also kinks to external bodies,** permanent and ad hoc, on occasion.[1] **NSF** and **NIH use** outside review panels extensively, aiming at merit-based rankings reflecting the collective judgment of a group of **recognized experts-the peer review model.**

**Not perfect, these processes can be criticized if the reviewers do not have appropriate qualifications (a problem particularly for internal agency reviews), or represent the conventional wisdom when the need may be to break the mold of ongoing research. Sometimes reviewers may favor their friends (true anonymity may be impossible in a specialized field). But the general approach has been widely accepted. It works, and-if applied appropriately-should work for sponsorship of industrial R&D by a CTA.**

**Once a broad agenda of R&D priorities had been set, the CTA could look to review panels for merit-based judgments, mixing outside engineers and scientists with the agency's own staff. The outside people would have to come from organizations without a direct stake in outcomes. There is no reason why a materials scientist working for an electronics firm could not give a fair review to the materials-related portions of HTS proposals on electrical machinery. Alone, such an individual would not have the expertise. In a group, he or she would contribute a useful perspective.**

**Contract monitoring, necessarily, would be the responsibility of the CTA staff. Agency employees would need to keep a critical distance from sponsored work, and be willing to cut off funding to companies that failed to perform (something DoD has difficulty doing on occasion).**

[1]The Advisory Group on Electron Devices, a longstanding committee of specialists from the three military services, industry, and universities, exerted considerable influence in shaping the Very High-Speed Integrated Circuit program, See "Federal Support for Industrial Technology: Lessons From VHSIC and VLSI," prepared for OTA by G.R. Fong under contract No, H3651o, December 1987.

NSF uses criteria for rating proposals that include: technical competence of the proposed research, based on past achievements, as well as the details of the proposal; "intrinsic merit of the research," meaning the likely impacts on scientific advance; and relevance. See *Guide to Programs: Fiscal Year 1988,* NSF 87-57 (Washington, DC: National Science Foundation 1987), p. ix. For a recent examination of scientific peer review, see *University Funding: Information on the Role of Peer Review at NSF and NIH*, GAO/RCED-87-87FS (Washington, DC: U.S. General Accounting Office, March 1987).

This activity would also force staff members to demonstrate that they can produce what they want others to produce—R&D that is relevant. The test is simple. If the intramural R&D is picked up and used by industry, the CTA staff has passed. Table 15 summarizes this and other features of a CTA.

### Pitfalls

The primary difficulties for any agency charged with supporting commercial technologies are likely to be political rather than technical. The problems of defining an R&D agenda, and recruiting a staff with the right mix of skills and experience would be straightforward compared with the problems of establishing credibility within the broader system of U.S. policymaking. Like any Government institution that seeks to endure, a CTA would have to respect notions of democratic virtue. That requires—in addition to accountability—prudence in spending public funds, fairness in dealings with the private sector, and some degree of balance with respect to regional interests.[7]

For a CTA to become a reality, any proposal would have to satisfy constituencies having very different interests—some conflicting. The Frost Belt, seeking to rebuild its technological base and infrastructure, would no doubt want

[7]E. Bardach, "Implementing Industrial Policy, " *The Industrial Policy Debate, C.* Johnson (cd.) (San Francisco: Institute for Contemporary Studies Press, 1984), p. 103. The discussion following draws heavily on Bardach. Also see H. Heclo, "Industrial Policy and the Executive Capacities of Government, " *The Politics of Industrial Policy*, C.E. Barfield and W.A. Schembra (eds.) (Washington, DC: American Enterprise Institute, 1986), p. 292; and *International Competitiveness in Electronics* (Washington, DC: Office of Technology Assessment, November 1983), ch. 12, especially pp. 475-482,

**Table 15.—Desirable Features in a Federal Agency for the Support of Commercial Technology Development**

**Budget.—$100 million to $500 million annually** in early years, exclusive of industry cost sharing, with 90 percent or more going for the support of **R&D projects.** These projects might be split roughly as follows:

- Industry, both single companies and consortia-80 percent
- Universities and non-profit research institutes—15 percent
- Internal agency projects—5 percent

Cost-sharing on industry projects at 40 to 60 percent seems appropriate to ensure that companies view the work as important.

Staff.—At the $500 million level, the agency would probably need about 250 professional employees. At any one time, about half the professional staff time would be devoted to intramural R&D—with technical employees expected to spend some fraction of their time, over a period of years, actively engaged in R&D.

Substantial use of detailees from other Federal mission agencies, as well as people on leave from universities and industry would be desirable. The agency's permanent staff members could also be expected to spend periodic tours in industry.

**Intramural R&D.—At the $500** million level for the agency, 5 percent for intramural R&D means $25 million annually (and perhaps 100 full-time equivalent professionals), It would seem preferable to maintain a number of relatively small efforts, spread quite widely across the spectrum of industrial technologies; given that the primary function of intramural R&D is to maintain staff expertise, breadth would be essential. Even with half a dozen R&D areas, many staff members with other specialities would have to spend time in industrial laboratories to maintain hands-on R&D skills.

**SOURCE Office** of **Technology Assessment, 1988**

parity with the Sun Belt; small business interests would probably begin lobbying for set-asides. Long before it opened its doors, an agency with the mandate to "restore U.S. technological competitiveness" would face pressures from companies in financial straits, seeking, for instance, relaxation in the CTA's requirements for cost-sharing.

Other threats to neutral allocation of CTA resources would be almost as quick to materialize. Perceived inequities between competing firms and industries would be all but impossible to avoid even with long-term R&D. CTA-funded R&D on magnetic separation of steel scrap would help minimills at the expense of integrated steel producers. Advances in magnetic levitation rail technology would threaten aircraft manufacturers and airline companies.

Such pressures could easily jeopardize the CTA's intended focus. Not only would distressed industries be pushing for a quick fix, a national emergency—an energy crisis, say—would bring calls for technological solutions. With high turnover likely in its political leadership—on average, assistant secretaries remain in Government for only 18 months—a CTA would be under constant pressure to show results. Pressures for immediate results would coexist with pressures to maintain funding for major projects, even if they proved flawed. Managers would be reluctant to admit mistakes, and–compared to their private sector counterparts—have less incentive to do so. Even flawed projects develop constituencies, moreover, ready to argue for continued funding.

In sum, a CTA—like any public institution—would have to win favor from enough well-situated constituents to continue its work. That is as it should be. The danger—and a very real one—is that, as an institution charged with spending money, a CTA would become just another forum for the distributive clashes that already consume Congress and much of the executive branch. Said one close observer of science policy, "We have the most highly-developed system of interest groups in the world, and they've discovered R& D."

## CONCLUDING REMARKS

The Federal Government's responsibility for promoting technology is plainest in two cases: support for basic research, and investment in risky and speculative technologies, In both, substantial public benefits may coincide with meager private returns. The problem for U.S. technology policy is not only to find and support such R&D, but to stimulate industry to use the results in timely fashion.

Basic research continues to flourish under the present system of U.S. support for science and technology: when the breakthroughs in HTS occurred, the Nation had the resources and flexibility to mount a considerable effort in short order. To support advanced technologies, however, the United States has traditionally relied on the mission agencies—DoD, in particular. The approach reflects a philosophical distaste for government involvement in the economy, and also the belief that government cannot anticipate the needs of the marketplace; spinoffs, rather than direct financing, have supported many of the new technologies that American industry commercialized.

The approach worked well for several decades. But the world has changed. Military technologies have grown steadily more specialized, the defense sector more isolated from the rest of the economy, If DoD R&D funding was ever a cornucopia for U.S. industry, it is no longer. Second, other countries have caught up in technology. Today, both Japanese and West German firms spend higher proportions of their revenues on R&D than American firms. That spending has been one of the critical elements in their competitive success,

The emerging pattern in HTS seems much like that in microelectronics. Japanese firms are investing their own funds heavily. Government policies support their efforts. In the United States, only a small fraction of the Federal money for HTS finds its way into industry, and most of this will pass through DoD.

These and other indicators lead to the conclusion that a continuation of current policies for supporting commercialization of HTS will leave U.S. industry behind its strongest international competitors, The United States may continue to dominate the science of superconductivity, and might pioneer in commercial innovations. But the contest will eventually come down to engineering and manufacturing, where American industry has fallen down in recent years, and where the Japanese continue to improve.

OTA has analyzed two alternatives to the business-as-usual approach. One of the choices —creation of a Federal technology agency, with HTS as a piece of its territory—holds promise for the future. Such an agency might support industrial technology directly; many proposals have envisioned a kind of civilian DARPA, established to focus on R&D relevant on the civilian side of the economy. The pitfalls are not so much technical—maintaining a sound portfolio of projects–as political. A CTA would have to deal with the demands of distressed industries, depressed regions, and companies simply attracted by a pot of R&D money,

Whatever their merits, the alternatives under Strategy 3 cannot offer near-term support for HTS. The *ad hoc* measures outlined under Strategy 2 could. This approach —Federal cost-sharing of joint R&D—would be explicitly designed to promote an industry-centered agenda of long-term, high-risk R&D in superconductivity. Government's role—carried out through a working group on commercialization—would be as facilitator, as well as financier, helping to establish consensus on a research agenda, and securing cooperation from universities, Federal laboratories, and mission agencies, The three elements in Strategy 2 meet the needs summarized at the beginning of the chapter: diversity and continuity of Federal support; market-driven decisions; technology push complemented by market pull; low visibility; collaboration among industry, universities, and Government. In conjunction with ongoing activities in the mission agencies, they would substantially improve the odds on U.S. industry in the race to commercialize HTS.