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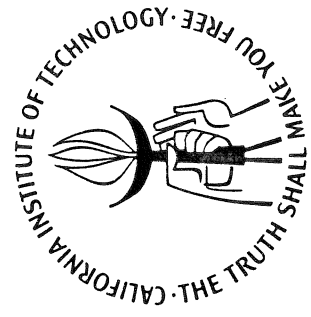
THE POLITICAL ECONOMY OF GOVERNMENT PROGRAMS
TO PROMOTE NEW TECHNOLOGY

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By Linda Cohen and Roger Noll

ABSTRACT

An important component of the federal budget is programs to develop and to commercialize new technology. This paper applies recent developments in the rational actor theory of political behavior to examine the politics of these programs. The principal theoretical conclusions are that government commercialization projects are relatively unattractive as particularistic distributive programs, but that once a project is begun, the political incentives are to rush the research phase and get on with demonstration, to continue unlucky efforts after their expected payoffs are known not to be worth their costs, and to redefine the goals of the program so as to deemphasize the initial objective of commercial adoption. These conclusions are then tested by examining two recent projects: the Clinch River Breeder Reactor and the space shuttle. Both programs appear to illustrate the problems predicted by the theory.

An important and largely unstudied domain of government policy is programs to commercialize new technologies. This arena of public policy is hardly new. Samuel Morse's first telegraph line, between Baltimore and Washington, was financed by a \$30,000 appropriation from Congress (Brock [1981], p. 56), and high-yield, hybrid seeds were developed and promoted by the Department of Agriculture (Grilliches [1957] and Rosenberg [1971]). Nevertheless, this federal activity has shown marked growth since the introduction of the Atoms for Peace program in the 1950s -- the program that gave us commercial nuclear power plants.

The purpose of this paper is to examine the political foundations of policies to promote new technology: how they come about, and how political forces affect their performance. Our theoretical approach is rational choice theory, and our focus is Congressional decisions. The programs of interest are attempts by the federal government to cause the private sector to adopt an identifiable new technology. Such programs involve more than support for research, or passive methods of subsidizing innovators and entrepreneurs. The special characteristics of these programs are that they place the

government in the role of deciding which technology ought to be adopted by a specific industry, and that they involve developing a commercial demonstration project.

Recent examples of the programs we have in mind are the following:

- (1) The Colony Project in western Colorado which, until canceled, was supposed to demonstrate a commercially-viable method for producing fuel from oil shale;
- (2) The space shuttle, which was intended to demonstrate the feasibility of inexpensive, reusable launch systems for numerous commercial uses of space;
- (3) The Clinch River Breeder Reactor, which was supposed to prove the commercial feasibility of fuel-efficient, sodium-cooled nuclear reactors; and
- (4) The Supersonic Transport, or SST, which was intended to result in an economically sound high-speed jet passenger aircraft.

The key features of these programs are as follows. First, they require overcoming significant technical barriers if they are to be successful. Second, the government commits itself not just to assisting in solving the key technical problems, but to seeing the program through its first commercial applications. Third, the stated objective of the program is economic: to reduce costs or enhance

performance by a sufficient amount to provide an efficiency justification for the project. Fourth, despite the third criterion, the private sector is unable or unwilling to undertake the project on its own.

The paper presumes that the set of projects that could satisfy all four criteria is not empty. There are theoretic grounds for believing that in some circumstances the private sector lacks sufficient incentive to develop warranted new technology, and examples in which government commercialization projects were successful. What we seek to examine is the correspondence between projects that are economically justifiable and projects that are politically feasible -- that is, projects that serve the interests of legislators seeking reelection or political advancement. We also seek to determine how political incentives affect the probability that a project will succeed.

The principal conclusions from our analysis are as follows. First, in comparison to other large construction expenditures, demonstration programs have no special political attractions yet do have some political liabilities; hence we would expect them to be undertaken only under special political conditions which we elaborate. Second, given that a demonstration program initially is economically warranted and politically feasible, a project that turns out to be a poor one will be continued longer than it should -- e.g., government systematically is slow to cut losses from promising ideas that turn sour. Third, given a warranted and feasible project, the government

systematically moves too quickly from research to development, and from development to commercial demonstration, which means that some good projects will actually turn out to be failures even though, with better management, they would have succeeded.

The structure of the paper is as follows. Section I sets forth the theoretical arguments in support of these conclusions. Section II discusses two specific cases, the Clinch River Breeder Reactor and the Space Shuttle, that support them. Section III contains a concluding discussion.

I. THEORY

In this section we develop a theoretical analysis of the political incentives facing legislators with respect to decisions about whether to undertake, to continue or to cancel a developmental research project. In this analysis, we seek to make two kinds of comparisons. First, at each stage how does an economically warranted developmental research project compare politically with alternative expenditure programs that are available to Congress? Second, to what extent are there differences between politically attractive and economically warranted decisions? We adopt a broad definition of economic justification. We include the increased profits of the private companies that adopt the technology, the increase in benefits to consumers in lower prices and/or higher quality (e.g. increased consumer surplus), and spillover effects in the development of other technologies or in other areas of public policy (national defense and foreign relations being two examples). It is the latter two elements

that constitute the basis for justifications on economic efficiency grounds for government to play an active role in developing and promoting new technology. Our purpose is not to examine the validity of these justifications, but to examine whether political incentives cause them to be pursued efficiently. For an analysis of justification for government participation in commercialization, and an extensive bibliography, see Schmalensee (1980).

The Economics of Commercialization Projects

A necessary preamble to the political analysis is to establish a conceptual baseline regarding the economics of commercialization projects. Borrowing from the work of Lee [1983], we will outline the characteristics of an economically optimal commercialization project in which the optimum plan takes correct account of the public objectives that might motivate government action. Once a general understanding of optimal R&D is in hand, we can filter the decision points in the optimal plan through a political analysis to determine whether and how electoral incentives might affect them.

A program to commercialize a new technology, whether public or private, has four distinct phases: research, prototype, demonstration and adoption. Initially, there is uncertainty about precisely how to proceed. Several alternative approaches to solving key technical problems are promising, but none are certain to work and little basis exists for choosing among them. The number of such alternatives, and the degree of uncertainty associated with each, depend on how radical a change in technology is contemplated. More radical approaches

generally possess more alternative paths of development and greater uncertainty regarding the ultimate outcome of the project.

The initial phase consists of a relatively large number of small research projects. Most of the expenditures are on research personnel, who are given relatively great latitude in what to investigate and how to proceed. At this stage specific components of the project are difficult to defend on other than highly sophisticated technical grounds or even the hunches of sophisticated researchers.

In this phase the application of more resources is most likely to encounter severely decreasing returns. Attempts to speed up research by increasing appropriations are, beyond a point, likely to lead primarily to duplication of effort rather than greater rates of progress. In large, research-oriented companies, this phase is more or less continuous -- a research group is continually exploring new opportunities, and cancellation of one project causes the group to move on to exploring another.

The second phase of a project involves the production of prototypes and pilots. It takes place when research has narrowed the uncertainties regarding the alternatives, permitting managers to make distinctions among them according to their promise. Prototypes and pilots require constructing working models of at least the major new subsystems, and sometimes of the entire system. Consequently, they involve people other than research scientists and engineers, and substantially greater expenditures in procurement and construction, than are needed in the research phase. The rate of expenditure

required to sustain an alternative through this phase is substantially greater than in the initial phase, expenditures are spread over a more heterogeneous group of people, and the outcome is less uncertain. Because of the differences in economic requirements, only a few alternative lines of development can be pursued into this stage; most are terminated during the initial stage. Finally, whether in the public or private sectors, these projects are intermittent. If one is canceled or completed, another does not necessarily take its place.

Because some of the expenditures in the intermediate stage are for constructing and testing a physical model, work is more amenable to variations in the rate at which it is accomplished than in the research phase. But there are limits arising from the fact that prototypes and pilots still have uncertain performance, and the process of building and testing them feeds back on research projects. Indeed, that is the point of having this phase. Consequently, the prototype phase also has a narrow range of variation in rates of progress without making substantial sacrifices of efficiency, and is uncertain regarding its ultimate success.

The third phase is the demonstration project: the Clinch River reactor or the Columbia orbiter. On rare occasions two technical alternatives reach the demonstration stage, but the common number is one. Demonstration projects are intended to solve a few remaining technical problems that can only be tackled in a full-scale model, and to show that it is possible to build a commercially interesting facility with respect to scale, cost and performance. They are

normally much larger than pilots and prototypes, and sometimes are the first attempt to combine all of the subsystems into a working technology. Consequently, a demonstration project is normally much more expensive than a prototype. Moreover, because it is intended to prove a technology more than to be a means for further development, it normally involves relatively little research. Most of the expenditures are for procurement and construction. At this phase, the project is more amenable to speed-ups and slow-downs without a major sacrifice of efficiency.

The fourth phase is commercial adoption. It occurs when the potential users of the technology, after observing the demonstration, decide that the economic and technical characteristics of the new technology warrant its regular use in commercial activities.

Developing the economically optimal R&D plan requires solving a complicated dynamic programming problem. The solution is a sequence of decisions about which alternatives to pursue in each stage, when to cancel further investigation of an alternative, and when to switch from one stage to another, with each decision contingent upon the information that thusfar has been obtained from the ongoing projects. The optimal contingency plan will maximize the expected net benefits of the project; for a risk-neutral government manager, this would be the discounted present value of the net social benefit, using the correct social rate of discount and including the range of consequences discussed at the outset.

The net benefit of the project hinges on its total cost and the

ultimate performance of the new technology. Other than some possible technical spillovers as the project progresses, the gross economic benefits of the project virtually all accrue after the demonstration project is finished and the new technology is adopted.

At the start, the ultimate value of the program is subject to three sources of uncertainty. First, associated with each technical alternative is a range of possible outcomes in cost and performance. Second, associated with each alternative is a range of estimates of how long it will take to develop the technology -- and hence a range of estimates of its discounted present value given that it succeeds. Third, the options pursued in each phase depend on the results of the preceding phase or phases. Consequently, the sequential nature of the optimal R&D plan introduces uncertainty over which technical options will be pursued to the next phase.

Research and prototype projects are undertaken because they provide valuable information about the new technology without requiring that a full commitment be made to it -- but also without proving its commercial viability. Because research is relatively inexpensive, the promise of a technical alternative can be rather problematical and still be sufficient to make it worth examining. As research proceeds, uncertainties are resolved and the promise of alternatives becomes easier to distinguish. The advantage of further research is that it enhances the likelihood that the most promising alternatives will be identified and pursued to the prototype phase, thereby reducing the chance of wasting money in the phases that are more expensive. The

disadvantage of deciding to undertake further research is that it postpones the date on which the new technology will be adopted, and the benefits obtained, assuming that it succeeds. The switch from the initial to the prototype phase occurs when managers believe the gains of further research no longer justify the sum of its direct costs and its delaying effects.

Similar calculations underpin the decision about moving from prototypes to a demonstration. Prototypes provide still more information and enhance the likelihood that the technology will be successful; however testing delays adoption. A demonstration will be undertaken when the additional knowledge from further testing is not worth delaying the try at commercializing the technology.

The sequential nature of decision-making implies that the optimal level of effort in the latter phases of the R&D program will not be known with certainty ex ante. This introduces a further complication into planning and evaluating an ongoing R&D program. The features of the optimal plan that we wish to emphasize are the following. First, all the salient characteristics of a program are measured in terms of dollars. Funds spent on one kind of activity are directly compared with funds spent on another. Second, costs and revenues in different time periods are made comparable by discounting. Third, the response to uncertainty is a contingency plan: programs in which actions at a later date are made contingent on information derived from preceding work. Fourth, decisions whether to proceed to the next step in pursuing an alternative are based on the incremental

cost of continuing another round and the expected benefit to be derived therefrom in terms of the expected payoff of the program.

The process is analogous to decisions based on a series of updated decision trees. As work progresses through each stage, the distribution of discounted present benefit from each option is updated, and its variance presumably narrowed. This allows a revision of the research plan, to a new optimal choice of projects and appropriations based on the information then available. While the revisions depend on previous work, decisions are based on estimates of subsequent costs and benefits. Thus, as with optimal investment decisions under certainty, sunk costs are irrelevant to the actual decisions made at each stage.

The Politics of Commercialization Programs

The foundation for our analysis of the politics of government commercialization policies is rational actor theory, which connects the policy-making actions of legislators to the voting decisions of their constituents under the assumption that citizens vote on the basis of self-interest and candidates seek to maximize their probability of staying in office. We will briefly summarize the important features of this theory that apply to the case at hand. The general thrust of our argument is that particularistic and distributional concerns are especially prominent in the politics of the later phases of developmental R&D programs, and therefore cause certain systematic inefficiencies in the way programs are implemented.

The first element in the analysis is how citizens decide to participate in political activities: voting, lobbying, contributing to

campaigns, or joining organizations that engage in these activities. Casting a vote is a very incidental act to a citizen because the vote conveys very little information. It is a simple, dichotomous signal in response to a very complicated vector of policy positions and other attributes of the list of candidates. Moreover, a single vote has very little affect on the election. Consequently, a rational voter will devote very little time and resources to gaining expertise on the wide spectrum of issues that the winner will have to address in order to identify the candidate that best serves his or her self interest. Instead, voters will be passive, responding to information provided by others and assessing candidates on the basis of highly general attributes and issues, plus their personal experiences with them. To the extent that a voter does focus attention on positions taken by candidates, the issues selected will be the ones in which the voter has the greatest personal stake.

Other forms of political participation differ from voting in two ways. First, they provide more opportunity for citizens to express the intensities of their preferences. Lobbying, contributing and participating in political groups can be undertaken with varying degrees of commitment. Second, because these forms of participation are generally more costly than voting, a citizen must have a greater stake in an issue to be motivated to engage in them.

In calculating stakes in an issue, a citizen considers personal rather than total societal benefits from the project. A citizen makes no distinction between benefits accruing from improvements in the

general state of the nation and from public goods, on the one hand, and highly targeted programs that enhance the voter's income or personal purchasing power. Consequently, candidates will be evaluated on the basis of both aspects of their platforms and records, as analyzed in Fiorina and Noll [1978]. While the general societal benefits of a program may be its most important effect, these normally are very diffuse. Among citizens whose only stake in the issue is in its tax costs and its general public benefits (or project outcome, in this case), the personal stakes normally will be too low to merit significant weight in voting decisions. Similarly, among citizens who are directly involved in implementing a policy, the effect of the program on their income is likely to overshadow their evaluation of the policy outcome.

This observation has three consequences of importance to this discussion. First, political support for a program grows as its distributional role increases and becomes more focused. Second, the ultimate outcome or success of the program may play a secondary political role to the form of its implementation and may, indeed, be of relatively little consequence in the political calculus. Finally, voters with low stakes in a program are likely to base their voting decision on minimal information about it, if it enters their calculus at all.

In the case of government commercialization projects, all consumers of the product of industries using the technology will benefit if its commercial adoption is economically warranted, but this effect will be diffused over a large number of people with small per

capita stakes. People earning income in the industry in which the technology will be used may also have a stake in that warranted technical change may expand sales, profits and employment; however, this, too, is diffused over a large number of people, and may be dissipated by competitive entry.

Having higher per capita stakes will be the people who are employed in implementing the R&D program. When facing a decision whether to undertake a phase of a program, including the research phase, the identities of the people to be employed will be to some extent unknown. A group that is larger than the number who eventually will win contracts and be employed faces the probability that the program will benefit them, which makes the income-generating side of the program more diffuse. Once a new phase is entered, the lucky winners become identified. This reduces the size of the group whose income depends on the program, but raises their per capita stakes in the programs' continuation by a compensating amount.

For reasons summarized above, as the per capita stakes of a citizen in an issue rise, he or she becomes more likely to give the issue substantial weight in making decisions about political participation. Hence, two phenomena emerge about the connection between political participation and commercialization projects. First, at each phase of the program, the particularistic elements are more important after the phase is entered and the work has begun than when the project is first proposed. The implication of this observation is that, ceteris paribus (including the likely success of the project),

projects should have less political support when the issue is whether to begin a phase than later when it is to continue it. Second, because the rate of expenditure of the project increases from phase to phase, the electoral importance of the particularistic aspects of the program should increase as the program moves to later phases. The implication here is that, ceteris paribus, the political support for a project should be highest in the demonstration phase and lowest in the research phase.

Presumably legislators are aware of these effects, and decide to enter developmental R&D programs bearing them in mind. To legislators, the relevant question is whether a program is worth the political opportunity costs in terms of the electoral consequences of taxes, deficits and other programs forgone. In the spirit of the political benefit-cost analysis discussed in Weingast, Shepsle and Johnson [1981], we will compare the electoral consequences of various types of programs. We will first examine the pure particularistic benefits, comparing developmental R&D programs with other ways to spend federal dollars on procurement and construction. Two such comparisons are especially relevant: federal construction programs that involve no research (the traditional pork barrel), and military weapons systems development.

To a legislator, one political advantage of these other programs is that they are more difficult to evaluate after they are completed than is a commercialization project. Dams, highways, weapons systems, federal buildings and sewage treatment facilities do not have

to pass a market test by becoming adopted voluntarily by an industry. While the success of any such project can be disputed, the issue is certain to be more conjectural than the rather clean test at the end of commercialization project: namely, does anyone want to use it? Even weapons systems, which face a test relating to their contribution to national security, are evaluated more in terms of pure technical performance -- do they meet technical specifications -- rather than whether a dollar measure of benefits constitutes a reasonable return on the dollars expended. Consequently, because of easier evaluation, developmental R&D programs represent more of a political risk to legislators than do other means of providing the same amount of particularistic expenditures.

A second undesirable characteristic of demonstration programs to politicians is their intermittency. Other expenditure programs go on more or less continuously. One weapons system or federal construction project can be expected to be followed by another; indeed, the unanimity norm in pork barrel appropriations essentially assures this result, as discussed in Weingast [1978]. In contrast to this, demonstration projects end. For them to be replaced immediately by another of the same type would cast doubt on the value of the first (radical technical change is not supposed to be a continuous process), and in any event would require a return to the less expensive research phase. Thus, happy constituents benefitting from these programs one day lose their source of income without another government program necessarily taking its place. Legislators would prefer a less

intermittent way to provide particularistic expenditures.

The third politically unattractive characteristic of commercialization programs is that, to the extent they provide particularistic electoral payoffs, they do so only after the relatively inexpensive and unpredictable research phase is over. Legislators will not know whether their constituency base will be the beneficiaries at the financially more important prototype and demonstration phases.

And, in any case, these electoral benefits are in the future -- normally several elections away. Among alternative income-creating expenditures, only military weapons systems share this disadvantage, and even here it is partially ameliorated by the fact that, for any given type of weapons system, there are very few competitors for the important developmental phases, so that the ultimate political beneficiaries of a commitment to develop a system are easy to guess in advance. In addition, weapons development leads to an even bigger expenditure program on weapons acquisition, with concomitantly large particularistic electoral benefits. By contrast, the adoption of a new technology by industry will involve only private expenditures on investments in the new technology for which a politician will not be able to claim credit to constituents who are employed in this phase.

To sum up, as a purely particularistic, pork barrel activity, technology demonstration projects are not very attractive to legislators. In the absence of other factors influencing the political calculus, we would not expect to see this kind of government activity. Nevertheless, other factors do sometimes emerge that tip the balance

the other way. As stated earlier, particularism is only one factor in electoral politics. Another is general public issues that achieve electoral salience. When performance in some sector of the society is perceived to be sufficiently poor, dissatisfaction is translated into an electoral demand for a policy to address the problem. For a demonstration program to achieve this status, the technical performance of a sector of the economy must become salient. An example is the energy sector during the 1970s in the face of rapidly rising world energy prices and the sense of increased vulnerability to foreign sources of crude oil. The issue of pushing the development of new energy technology became salient; a passive subsidization program of the industry that was perceived to be performing poorly, while perhaps more promising from the aspect of particularistic electoral incentives, was not a viable response to the general public issue because it would have appeared to reward what was regarded as bad performance.

A second, related political circumstance could also make technical demonstration projects more attractive. It is the rise of political sentiment against more traditional forms of particularism. If dams, highways and new weapons systems come to be regarded as public "bads," which probably happened in the late 1960s with the rise of environmental and antiwar movements in the United States, the disadvantages of demonstration programs discussed above could be offset by their more benign features in the eyes of opponents of more traditional means of delivering particularistic favors.

Finally, an advantage of demonstration programs is derived from

their positive economic value to the industry that is ultimately expected to adopt the new technology. This makes it possible that a substantial part of the cost will be picked up by the private sector. If the program is clearly labeled a public program, legislators can still claim credit when contracts are let in their districts; however private sector cost sharing lowers the price tag of achieving these electoral benefits. This situation is less common in alternative ways to deliver public construction and procurement expenditures, for the principal output of these programs is usually government goods, not something of value to private businesses.

Implications for the Characteristics of Demonstration Programs

The preceding analysis leads to several important conclusions regarding the way demonstration programs will be viewed by legislators during the lifetime of the program. Normally, demonstrating new technology should not appear particularly attractive. In order for it to become so, one of three events has to occur:

1. A public issue arises concerning the technical performance of a sector of the economy;
2. Political limitations emerge to constrain the extent to which more prosaic pork barrel programs can be adopted; or
3. The private sector becomes willing to bear a major share of the cost, while giving up enough control of the program to allow politicians to claim credit for its distributive aspects.

Once a commitment has been made to undertake a program for one of these three reasons, the political support for the program is likely to grow as the program matures into a full-scale demonstration. This creates an incentive for legislators to want to move to the latter phases of the program as quickly as possible. Because the political benefits are concentrated in later stages, they will want to rush the early phases more than would a decision-maker who seeks to maximize economic efficiency. Because these phases are not very amenable to being rushed, the effect is a lower chance of ultimate success of the program.

A second source of inefficiency follows from the nature of the political support in the latter phases of the program. As is discussed above, the growth in support is due to the increase in particularistic benefits: the large-scale, concentrated expenditures on procurement and construction of a demonstration program. To the extent that the program fits the mold of distributive politics, the standard "pork barrel" inefficiencies can be expected to occur. As is discussed by Shepsle and Weingast [1980] and Weingast, Shepsle and Johnson [1981], legislators effectively discount the costs of such programs because they generate political support. Consequently, political evaluations diverge from economically efficient evaluations of project costs and benefits, causing expenditures that are not justified on economic grounds to be politically attractive. This phenomenon is intensified by the uncertainty in a research and development program that pertains to the demonstration phase. The actual costs and performance of the

demonstration are unlikely to be known until the project is well underway. At that point a status quo has been established: a group has been created whose political support is based on continued expenditures for the demonstration, rather than its (economic) benefits. Finally, the tendency to rush early stages of the program exacerbates the problem. One inefficiency due to early speedup is that the program can be expected to enter the demonstration phase with greater uncertainty over costs and benefits than would exist in the optimal economic plan. Thus, the bias for early demonstration increases the likelihood of establishing a political support group for a project that is excessively prone to cost overruns and performance shortfalls. The joint effect of these observations is that expenditures on a demonstration program that does not turn out well will be continued past the point that is justified on the basis of cost and performance.

The diminished chance of success and the growing particularistic aspects provide an incentive to decouple the program from its ultimate commercialization goal. As discussed in Fiorina and Noll [1979], incumbent politicians seek to couple particularism and noncontroversial activities so that they can avoid running for reelection on truly divisive, controversial issues. A failed project can become the latter -- a public scandal that threatens its advocates in the legislature.

All R&D programs are uncertain, and some warranted projects will prove to be unsuccessful enough to deserve cancellation. To avoid

this, a politician will want the ultimate evaluation of the project to be on some other basis than the narrow criterion of its efficiency as a new technology. Hence, the politician will seek opportunities to change the justification of the program to something more vague, like the justification for other pork barrel and military programs. This gives the program a wider array of paths to ultimate success, and even if it is a failure, makes the job of so identifying it more difficult.

Once the program has entered the phases that contain strong particularistic aspects, the original commercialization goal may contribute little to its political popularity. At that point, goal transformation to more abstract concepts has little political liability, or in any event, less than that associated with cancelling a project, alienating the beneficiaries of its distributional consequences, and admitting a mistake (alienating everyone else). Examples of the kinds of vague goals a project might inherit are national security, national prestige, and technology as an end in itself, rather than a means to other objectives.

For these reasons, three general characteristics of government-sponsored commercialization projects can be expected to emerge. First, to the extent the government has a role in this area, it may have insufficient reason to undertake warranted projects because the particularistic aspects of them are not very attractive. But once government undertakes a program, it has incentives to mismanage it by foreshortening the research phase, being insufficiently willing to retain options and change paths of development as more information is

obtained, and continuing programs after it becomes clear that the likely benefits do not warrant further investment. Third, the public rationale for the program is expected to shift over time, from the narrow issue of developing efficient new technologies to more vague goals that are less amenable to clear tests of success.

II. CLINCH RIVER AND THE U. S. BREEDER REACTOR PROGRAM

The U.S. breeder reactor effort is a continuation of government-sponsored civilian nuclear power research, the origins of which date back to World War II. Breeder research got underway about the time that the light-water (LWR) became commercial, and its development program was termed a success, in the mid-1960s.¹ At this time, the prospects for nuclear power looked very bright. As a result, advanced reactor designs that were more fuel-efficient than LWRs² appeared to be highly desirable. An AEC study, complete in 1967, concluded that the liquid-metal fast breeder reactor (LMFBR) technology

1. The breeder program received considerable support directly after World War II culminating in the first U.S. LMFBR called EBR-I. In 1951, EBR-I, a 150 KWe reactor, actually generated the first electricity to come from fission in this country. EBR-I suffered a meltdown accident in 1955, was repaired, and was finally decommissioned in 1964. A second experimental breeder, EBR-II, rated at 16MW, came on line in 1963, and remains operational today. The third early breeder, FERMI-I, a 60 MWe LMFBR, was built under the cooperative power reactor development program by Commonwealth Edison and the AEC. After difficult licensing and court battles, the plant went critical in 1963. Tests to bring the plant up to full power began in 1966, and the plant suffered a partial core meltdown during one test. It was repaired and again became critical in 1972, but because of licensing and economic issues, the plant did not resume commercial operation and has been decommissioned.

2. In theory, a liquid-metal, fast-breeder reactor can use up to 90 percent of the energy in uranium, a factor of 70 better than LWRs.

showed greatest promise, and should be emphasized for development.³ The program to build an important test facility (the Fast Flux Test Facility, or FTF) began in 1968, and simultaneously, discussion started about building several demonstration plants. The original plan called for government and industry to share the costs of three LMFBRs, spaced two years apart during the 1970s. This was to be followed by commercial facilities without federal participation. In the optimism of the late 1960s about the prospects for LWRs and nuclear power in general, coupled with estimated growth rates in electricity consumption of six to seven percent per year, a commercially viable breeder industry was projected for the early 1980s.

In 1969, negotiations began for the first demonstration: the Clinch River Breeder Reactor (CRBR). The original plan was that the facility would cost \$200 million, with the government paying 40 percent of the costs and electric utilities the remaining 60 percent. It was to be completed by 1978, with expenditures beginning in fiscal 1970. Risks of cost overruns were to be assumed by the reactor manufacturer, in the spirit of the "turnkey" era of light-water reactors a few years before.⁴ By the fiscal 1982 budget, the cost estimate had escalated to

3. At the same time, Admiral Rickover and the Navy developed a light water breeder using a thorium-uranium cycle. The LWBR at Shippingport went critical in 1977 and is now being decommissioned and evaluated. While more fuel-efficient than the LWR, the LWBR is nowhere near as fuel-efficient as the LMFBR. Its main advantage appears to be its ability to utilize thorium.

4. Burness, Montgomery and Quirk [1980]. That the turnkey episode nearly bankrupted manufacturers of light-water reactors seems to have been overlooked by officials who planned to duplicate the program for breeders.

\$3 billion with the government assuming virtually the entire cost increase. No construction had yet been started even though several hundred million dollars had already been spent, and the expected completion date was 1990 -- that is, in twelve years the time horizon for completing the project had not advanced at all. The subsequent demonstration projects have been abandoned, and (to date) alternative research plans have not received any significant appropriation.

The focus of this section is on CRBR, the demonstration phase of the R&D program. Unfortunately, it illustrates many of the negative attributes of a government-sponsored R&D program that were discussed in the previous section. It was supported by Congress for years after its initial economic rationale disappeared. The process of goal transformation is particularly marked: indeed, as even the more abstract goals failed, they continued to be replaced by other, vaguer goals, while the original physical concept of the reactor remained unchanged.

The arrangements regarding CRBK changed dramatically over the 1970s. The initial estimates of \$200 million, a 1978 completion date, and private assumption of risk evaporated before any contracts were signed with utilities. The agreement reached in 1972 was based on the following terms: the project cost was estimated at \$700 million, or which utilities would provide \$250 million. The government assumed responsibility for cost overruns and provided other financial indemnification for its industrial collaborators. Although legislation was needed to implement the agreement, the AEC and the lead utilities

-- TVA and Commonwealth Edison -- commenced licensing activities before it passed. They immediately ran into trouble as project opponents (the first was the Natural Resource Defense Council) seized on the commercial look-alike aspects of the project to challenge it. The commercial features gave rise to a need for various licenses and environmental reviews, providing opportunities for intervention and due process to intervenors. Licensing difficulties continued to plague the project as the D.C. Circuit Court, the EPA, and the NRC in succession have been unable to obtain necessary environmental and safety guarantees to license the plant as required by its semi-commercial status. Because of the lack of sufficient research and prototypes, this information has simply not been available.

A thorough study of the LMFBR program in 1974 came up with startling results. ERDA published a projected cost for CRBR of \$1.7 billion, and estimated that construction could not commence until 1980. New authorizing legislation was clearly needed. In 1975, Congress reauthorized the program, but specified a lead management role by ERDA, as government anticipated assuming an additional billion dollars in project costs. The utilities held firm at \$250 million.

At about this time the original rationale for the project came under serious criticism. By 1975, various factors, including cost overruns, technology problems, public opposition, and declining energy use, had caused a sharp downward revision in the projected growth rate of LWR capacity. Consequently, the projected benefits from a fuel-saving nuclear technology fell, and the desired date for a breeder

industry was extended to about the year 2000, although considerable controversy remained about this date. Second, substantial concern was voiced about the desirability to build CRBR on technical grounds: the design was ten years old and, according to some (although hotly disputed), technically obsolete. Finally, the enhanced federal role and subordinated utility commitment strained the claim that the technology was near commercialization.

The cost estimates and technical problems continued to grow. By 1978 -- when the Carter Administration attempted to kill the project -- the date at which the breeder industry was predicted to emerge had been extended to 2020. The obsolescence claim was receiving more support: indeed, this was given by the administration among its reasons for its attempt to cancel Clinch River. By 1981 the cost estimate stood at \$2.8 billion. Despite support of the Reagan Administration, construction at Clinch River had not begun, and the NRC had not yet issued the necessary construction permits.

An optimal economic response to cost and licensing difficulties and declining estimated benefits would be a reemphasis of research, but the opposite actually took place. CRBR was initially intended to be only a small component of the program: in 1969 its total cost was estimated at 2.4 percent of the total anticipated expenditures on developing nuclear technology. In 1972, that figure stood at 6.7 percent; in 1975, it was 19.3 percent; and by 1981 it reached 30 percent. Moreover, the growth in support for CRBR came at the expense of research activities. The total estimated cost of the breeder

program between 1978 and 1982 dropped from \$12 billion to \$10 billion, although the CRBK component estimate increased 36 percent. Thus CRBK had moved from being one small part of the program to its central component.

This all took place even though, had the original expectations about its cost, performance and technical value panned out, CRBR still would not have become a commercially viable technology for another half-century. Support for CRBR came at the expense of research activities. Indeed, the total estimated cost of the breeder program between 1978 and 1982 dropped from \$12 billion to \$10 billion, although the CRBR component estimate increased 36 percent. Meanwhile continued Congressional support of CRBR from 1970 to 1982 was accompanied by modification of its goals. The 1970 authorizing legislation states that:

"Design guidelines for the demonstration plant stress maximum use of existing technology to reduce technical risks and assure safe reliable operation.

Major objectives will be to (1) demonstrate the technical performance, reliability, maintainability, safety, environmental acceptability and economic feasibility in a utility environment of an LMBER central electric power station and (2) confirm the value of this concept for conserving important non-renewable natural resources." (U.S. Congress [1976], p. 71)

By 1975, it was not clear if CRBR would accomplish these goals.

While supporters continued to espouse them, other rationales were added. Chief among them was national prestige ("the French are threatening to take the ball away from us..."), the difficulty in restarting a program, and (despite controversy) greater emphasis on the technical knowledge that would emerge from the program.

By 1979, retreat from the commercialization goal was making the commercial licensing process -- a major roadblock -- seem rather silly. However, avoiding it was only possible if, as Carter proposed, the project was dumped and an entirely new project was started that emphasized research and prototype development and was located on a federal reservation. Congress responded to this proposal by making licensing itself a primary goal. A GAO report states this succinctly:

"[the utilities have] serious reservations about the possibility that DOE will build the large plant on a Federal reservation and not subject it to the scrutiny of the Nuclear Regulatory Commission's full licensing process. In the utilities' view, licensing the construction and operation of a breeder reactor is the most formidable hurdle that must be crossed in demonstrating breeder technology. Another concern is the negative impact this retreat from public scrutiny might have on public confidence in and acceptance of breeder reactors. In the present climate of public concern and debate on nuclear safety, the DOE plan is not appropriate.

The recent accident at Three Mile Island nuclear power plant has intensified this concern and debate over nuclear reactor safety. In this case, a water-cooled reactor, designed and constructed after a long series of gradual reactor scaleups experienced unanticipated events that resulted in releases of radioactive materials and serious reactor core damage. This event underscores the utilities' point that today and for the foreseeable future, gaining early public confidence and acceptance may be difficult but are as important as establishing technical and economic viability." (GAO, [1979], p. 16)

Finally, by 1981 the project goals stated by DOE Secretary Edwards avoid economic efficiency altogether. He justified federal expenditures on this project (as opposed to other energy demonstrate projects) because Clinch River

- represents a "long-term high-risk R&D venture";
- enhances national security;
- realizes the potential of nuclear power;
- contributes to non-proliferation goals; and
- "industry cannot be expected to invest significantly in CRBR when government decisions have so abruptly changed and voided past private sector investments." (U.S. Congress [1981])

The lesson of the Clinch River Breeder Reactor as we interpret

it is as follows. The government, once committed to a large-scale, technology-forcing demonstration project, faced great difficulty in turning it off. This observation has nothing to do with the wisdom of the nuclear power strategy for future energy development, nor of the breeder concept. Assuming that the United States must continue to depend heavily on nuclear power and, by 2020 or some such date, must have a commercialized breeder technology, the conclusion still remains that Clinch River should have been canceled several years ago. The attenuated time horizon for its need, the unanticipated technical problems, and the results of the Phoenix demonstration in France all should have led to a major reemphasis in the breeder reactor program towards research on components of the system where technical uncertainties remained. But once a commitment was made to demonstration, it proved difficult to reverse.

The source of the continuing commitment to the project has been Congress. In nearly every year since 1970, Congress has appropriated more for the project than was requested by the President. And, in the late 1970s, the Carter Administration tried to reallocate substantial portions of the Clinch River budget to the larger research program in civilian reactors generally and the specific research program dedicated to liquid metal fast breeders. In fiscal 1979, Congress more than doubled the President's request for CRBK, and in fiscal 1980 and 1981, when the President requested nothing for the Clinch River program, Congress continued to provide full funding.

In this case, Congressional allegiance to a particular

cost per pound of payload went up dramatically.

All of this became evident in the late 1970s. Between 1975 and 1976, the number of missions was cut back to 572, and ultimately in 1982 to 312. Cost overruns appeared to be modest at first, with expected development costs increasing about \$200 million per year (1971 dollars) from 1977 to 1979; however, in 1980 and 1981, these costs jumped another \$1 billion (1971\$). The date of the first flight was also being delayed: to March 1979 in 1973, to September 1979 in 1978, and eventually to the actual date of April 12, 1981. Thus, the system ended up being delayed by 33 percent, being 30 percent more expensive than was initially projected, and having 55 percent fewer missions. On a cost per mission basis, the cost overrun was about 150 percent.

In addition to the overrun in development costs, launch costs also escalated. The intent of NASA was to price the space shuttle launches so as to recover the costs of commercialization, but not the research effort. For launches for commercial organizations, foreign countries and agencies other than the Department of Defense and NASA, the launch price established by NASA was \$18 million per launch (1975\$) in 1977. DOD, because it was constructing its own launch facilities, was to be charged for the consumables only at a price of \$12 million. By June 1982, the commercial launch price -- calculated on the same basis -- had escalated to \$38 million (1975\$), an increase of 122 percent, while the DOD price reached \$28 million in 1983 (also 1975\$), an increase of 133 percent.⁵

5. "Shuttle Cost Impact." Aviation Week and Space Technology, (March 28, 1983): 13.

These cost-overruns led to drastic cuts in the most dramatically successful part of the space program during the period, the unmanned photographic missions to the planets (and the attendant scientific work that occupied most of the space on these missions). This was ironic, for one main purpose of the space shuttle was to reduce launch costs of deep space missions so that, among other things, the nation could afford more and better space science. But during the summer of 1979, as Congress became aware of severe shuttle R&D cost overruns, all of NASA's projects came under financial scrutiny. Representative Edward P. Boland (D-Mass.), chairman of the HUD and Independent Agencies Subcommittee of the House Appropriations Committee, suggested a fiscal 1980 cut of \$113 million for the space telescope and \$116 million for the Galileo Project to orbit Jupiter, remarking that "Galileo won't mind." 6 Congress did not cut either program, and it did grant \$485 million in NASA supplementals over the next two years, while increasing its fiscal year 1980 budget by \$220 million. Nevertheless, this action was not sufficient to absorb the shuttle overrun, and caused reductions or cancellations in other programs. It also led to the rejection by the Office of Management and Budget or the Congress of nearly all of NASA's proposed new starts from 1979 through 1982. In November, 1979, for example, three new programs were rejected by OMB:

- (1) The solar electric propulsion system, with proposed fiscal year 1981 funds of \$20 million

6. "\$600 Million Shuttle Cost Overrun Startles Congress." Aviation Week and Space Technology, (May 7, 1979): 18-20.

demonstration has had a dramatic impact on the development program. Indeed, in justifying continuation of the program, the goal of development was more-or-less abandoned. Current plans call for continuing the project only if utilities assume a far greater portion of the financial burden. This tactic may be a graceful way to exit from the CRBR field. However, we expect that CRBR proponents are right in predicting that any replacement is unlikely in the short run. Utilities are likely to evaluate the program on economic grounds, and at this point only an entirely new plan to manage breeder R&D could possibly make sense economically. Even this is doubtful, because of the decline in the potential fuel-saving benefits of breeders even if they work. Thus, even a redirected program probably requires heavy government involvement in financing it. The conditions that made such a program politically attractive in 1967 no longer hold. Opposition and concern have replaced optimism with regards to nuclear power technology, making the project controversial among the constituencies of many legislators. Current LWR capacity projections imply a need for breeders only far into the future, so that the demonstration phase cannot be economically justified for a very long time.

III. THE SPACE SHUTTLE

The original purpose of the space shuttle was to develop a commercially viable space transportation system that used reusable launch vehicles to carry large payloads at a cost per pound that was much lower than could be achieved with expendable launch vehicles. The commercial aim was twofold: to reduce the costs of launches for

civilian and military space programs, and to achieve low enough costs for business to find space commercially attractive for some manufacturing activities. In addition, the space shuttle served NASA's objective of keeping manned space flight the center of the space program, on the heels of the successful Mercury and Apollo programs.

Of course, building a reusable launch vehicle that was capable of carrying a manned crew and a very large payload was known to be very expensive compared to building an expendable launcher for smaller payloads. The plan was to make up the cost differential by using the space shuttle very frequently over a useful life of a dozen years.

When the space shuttle was proposed to Congress in 1971, the cost of developing it was estimated at \$5.15 billion. The first launch was scheduled for March 1978. The space shuttles were expected to operate for twelve years, undertaking a total of 725 missions.

Unfortunately, as work on the space shuttle progressed, unanticipated problems arose -- as might naturally be expected in any research and development activity that attempts such a great leap forward in technology. For one thing, expectations about the size of the payload had to be scaled back as it became clear that the space shuttle was going to have substantially less power at launch than was originally hoped. For another thing, the turn-around time on the ground had to be longer, and cost much more, than was initially expected because the vehicle was less resilient, and more vulnerable to its own snaking at launch, than was initially expected. Of course, smaller payloads and less frequent, more costly launches meant that the

and 3-year R&D runout of \$262 million (this effectively killed the U.S. involvement in the proposed joint U.S./European Halley's comet/Tempe 2 probe, which carried with it an additional NASA price tag of \$400 million over three years);

- (2) NASA involvement in a joint NASA/Defense Department/Commerce Department National Oceanic Satellite System, with proposed fiscal year 1981 funds of \$6 million and 6-year runout of \$179 million;
- (3) Shuttle Power Extension Package, a device to extend shuttle orbital stays from seven to 20 days, with \$17 million proposed by fiscal year 1981 and a 3-year R&D runout of \$150 million.

Augmenting these moves was NASA's continuation of transferring shuttle funds from production to R&D in an attempt to keep the first flight on schedule. An interesting feedback effect of these actions was to further reduce both the demand for launches and the capabilities of the shuttle system, two of the main reasons NASA was forced to scale back its 12-year mission potential to 312.

The rational business strategy circa 1978 would have been to reorient the program. More research was clearly going to be required on both the launch system and the spacecraft itself if the objectives of low costs and large payloads were to be achieved. Meanwhile, great progress had been made on expendable vehicles, which were providing

increasingly reliable and inexpensive service. Thus, continued primary reliance on expendable launch technology, with a space shuttle program designed more for research and development purposes than for commercialization, was a preferable option for all concerned: the military, NASA and potential private sector customers.

Instead, the government redefined its goals in the space shuttle program and proceeded to convert it to the only American launch system. The policy became the following: (1) to base the American launch system on the necessity to use men as pilots, rather than unmanned systems, as an end in itself, rather than because the former was a more cost-effective approach; and (2) to redefine the purposes of the space program as being those things that best suit the shuttle, rather than designing a launch system to perform the most desirable objectives. By the 1980s, all space science missions had to be designed to use the shuttle, even though that meant using a more expensive launch system (and thereby fewer missions for a given budget) and ruling out some of the more promising missions for which the shuttle was unsuited (even though they would be cheaper than the less desirable substitutes that the shuttle could handle).

The space shuttle program demonstrates the tenaciousness with which government demonstration programs cling to life long after their commercial appropriateness has been called in question. The cost to the American space effort may well be that we lose our leadership. While the U.S. has been developing the space shuttle, the European Space Agency has been developing the next generation of expendable

launch vehicles, the Ariane, and a June 1982 Office of Technology Assessment report stated that this and other foreign competition is beginning to threaten U.S. leadership in commercially profitable and politically significant space technology. In particular, the report notes that, due to the cost increases and schedule delays of the shuttle, a number of U.S. businesses have opted for the satellite-launch services of Ariane. In the U.S., private companies have announced their intentions to compete with NASA for launching communications satellites, using expendable launch vehicles of mid-1970s vintage. Meanwhile, the space shuttle has caused us to cancel our participation in one major multinational exploration project, the International Solar-Polar Mission, and to be the only country active in space that will not investigate Halley's Comet when it passes in 1986. The Japanese, Europeans, Canadians and Russians will all be there. Meanwhile, there is some chance that the space shuttle will still not be capable of launching its first spacecraft for planetary exploration, Galileo, in 1986.

Were the sacrifice of scientific objectives accompanied by a commercially usable vehicle, the cost/benefit calculation would at least have some chance of proving the program worthwhile. But the program has no significant benefits that could not have been achieved by a scaled down program: one or two space shuttles instead of five, one space shuttle port (at Kennedy Space Center) rather than two (the second is at Vandenberg), and continuation of parallel development of expendable vehicles. The space shuttle was pushed too fast and is now

not an attractive technology. Yet we have placed almost total reliance on the new technology long before it is ready. The impact on other elements of the space program -- both research and utilization activities -- is to retard progress, and perhaps cost the nation leadership in a major new technical arena.

IV. CONCLUDING REMARKS

The histories of the Clinch River Breeder Reactor and the spaceshuttle seem to bear out the theoretical analysis provided in Section I. Both were commercialization projects connected to generally popular issues in the late 1960s: manned space flight and nuclear power. Both were continuations of an interrupted developmental research project involving substantial public participation: light-water nuclear reactors (a commercialization project) and the Apollo program to put a man on the moon (a technical objective -- more like a weapons system). Both predecessors were widely regarded as successes, and both had substantial industrial resources behind them.

Both Clinch River and the space shuttle were initially sold to Congress on the basis of their economic attractiveness. The case for Clinch River was quite strong, given that one believed the projections of the growth of nuclear power, for breeders best solved the problems of fuel supply and waste disposal that were inherent in a large-scale nuclear energy industry. The space shuttle was more problematical; however, it one believed the cost, performance and launch demand analysis provided by NASA and the Department of Defense, the space shuttle was a reasonable proposition -- especially if one also believed

that public support for manned space programs would not wane after the excitement of the Apollo project died down.

Once the projects began, technical problems began to emerge that cast the initial justifications for them in doubt. It became apparent that both programs had pushed to the commercialization phase too quickly, that attenuation of both programs to undertake more research and experimentation was called for. It was also apparent that the demand for both technologies was going to be much less than initially was expected. Yet by the time of this realization, the size of both programs had become large. As the cost overruns and performance shortfalls mounted, Congress could not find the will to slow down, let alone kill, either project.

Instead of changing the program, the response of the political advocates was to change the objectives. For Clinch River, it became a ludicrous pronuclear issue: demonstrating that the U.S. government could overcome antinuclear activists and bureaucratic sloth to license, build and operate a breeder reactor having no economic or technical rationale. For the space shuttle, the change was more subtle. Instead of being a launch vehicle for achieving various objectives in space, the objectives in space were redefined to be things that were compatible with the Shuttle. In both programs the casualties were most of the research in the same general areas: other nuclear technologies, and space science and solar system exploration.

These are but two cases from among a much longer list of government commercialization projects. A more thorough and

comprehensive study of this category of programs must be undertaken before we can claim to have a solid understanding of the political economy of commercialization programs. Moreover, these two examples were implemented in a particular form -- a government-managed development program. Because this implementation strategy is most closely connected to the normal Congressional budgetary process, it has maximum vulnerability to the influences described in this paper. Several other methods of implementation have been tried with other programs: the creation of a quasi-public corporation (the Synthetic Fuels Corporation), government market guarantees to induce private-sector innovations (a minimum guaranteed price or a minimum guaranteed market from government procurement), targeted tax incentives for private demonstration projects (solar thermal energy and conservation), and tournaments among competing suppliers of technical advances (the photovoltaics commercialization program). Each approach must be examined to determine if its greater distancing from particularistic pressures on Congress provides distinctly different performance characteristics.

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