# THE CHARACTER OF RESEARCH AND DEVELOPMENT IN A COMPETITIVE ECONOMY

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My own interest in the character of research and development in the competitive economy stems from my major interest, during the past ten years, in military research and development. For one thing, as you know, most military R and D is conducted on contract by private firms: there is a continuing problem, which has never been very satisfactorily solved, of making the most effective use of R and D in private, competitive firms to advance military technology. Beyond that, it has seemed to me that the military services (and the government generally) might learn something from the way R and D is conducted and managed in the competitive economy that they could, to advantage, emulate. The competitive economy, because it is competitive, has some tendency to penalize and discard inefficient procedures and managements, and to encourage and promote efficient ones. All sorts of things interfere with the ideal functioning of Adam Smith's invisible hand, but it might work just well enough to justify some attention to the practices of firms and industries that have survived and prospered in a competitive environment.

Judging from the press there is general agreement that all is not well with military R and D, and rather surprising unanimity in diagnosing what is wrong. Apart from a small but distinguished group of dissenters (mainly from science and industry) practically everyone charges that military R and D is uncoordinated and inadequately planned, and plagued by duplication,

competition, secrecy, and waste. The remedies are alleged to be obvious: there must be strong central direction and coordination; more and better central planning; tough-minded decisions to eliminate duplication; suppression of inter-service and other competition; probably some "Czars" to knock heads together.

This diagnosis and prescription, apparently self-evident to officials, commentators, editorial writers, and most Congressmen, is really rather curious. As a nation we are presumably committed to free competitive enterprise. Anyone who proposed that we eliminate competition and duplication in research in, for example, the American chemical industry by central planning, coordination and knocking heads together would be denounced as either a socialist or a promoter of cartels. But whenever we think we have reason to be dissatisfied with military R and D we criticize it, as I will attempt to demonstrate, for precisely those features which it has in common with R and D in the competitive economy. Instead of taking the best practices in our more progressive industries as a guide for the military services, we plump for a highly centralized bureaucracy.

Let us take a hard look at the character of R and D in the competitive economy. It is not coordinated. There is no central planning or direction. No "Czars." No "weeding out" of unpromising projects at any level higher than the individual competitive firm. There is intense competition accompanied by secrecy and poaching, an enormous amount of duplication, and much obvious apparent waste. In fact, in addition to the lack of coordination among firms, a good many companies noted for their achievements in the R and D area (e.g., Bell Labs, G.E., parts of R.C.A., and DuPont) deliberately decentralize effective control of research to the laboratory or

research center level, each center receiving a block budget within which it has complete freedom and responsibility. Although the initiation of new research projects involving relatively large sums is subject to higher level review, these usually grow out of small spontaneous efforts in the laboratories, so that idea initiation and initial exploration is subject to a very responsive and decentralized decision-making apparatus.*

The antithesis could scarcely be more striking. In the competitive economy, there is complete decentralization and no high level planning. But the critics of military R and D want to centralize and plan at the top. There are three possible extreme explanations, which I would like to examine. The first is that the organization of R and D in the competitive economy is wrong -- inefficient and wasteful. The second is that the critics of military R and D are wrong. The third is that military R and D is so different in character from the R and D conducted by competitive firms that utterly different techniques of managing it are appropriate. I will conclude that the truth lies somewhere between the second explanation -- that the critics of military R and D are wrong -- and the third -- that the problems are different -- but perhaps nearer the second.

There is a good deal of strong prima facie evidence against the first explanation -- that R and D in the competitive economy is just inefficient and wasteful. The lack of high level planning in the American competitive economy does not result in anarchy: the forces of the market provide a

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* Some firms follow very different organizational and management principles for that part of their R and D that is on contract with the military. They are forced by the way the government administers contracts to more or less ape the government, and go over to a highly centralized, rigid, slow moving structure with all decisions funneled up and little discretion at the working level.

high level screening that seems to be reasonably effective in promoting efficiency. There are few American industries that are not technologically more advanced as well as more productive than their equivalents in Russia -- or indeed in any other country, including Western European economies which place more stress on coordination and cooperation and less on competition. The exceptions are instructive. They include some military areas (e.g., rockets and tanks) and some non-military ones highly dependent on basic research (e.g., metallurgy and, at least until recently, a good many chemicals and drugs). As we will see later, there are some reasons for believing that the competitive American economy is more proficient in supplying R and D at the engineering development end of the R and D spectrum than at the basic research end.

In my opinion the most important thing to understand about R and D is the dominant role played by uncertainty. Uncertainty in R and D is greater and more pervasive than many researchers and research administrators permit themselves to imagine. Predictions by "experts" of the results or usefulness of particular R and D projects are highly unreliable. Research seems to attract an optimistic breed. Despite the fact that developments almost always take longer and cost more than predicted (by factors of 2 to 50), and that most fail in whole or in part, the predicted schedules of new development projects are almost always taken seriously. Of course there are so-called developments that are highly predictable (like some marginal product improvements) but R and D resulting in significant scientific or technological advances is universally uncertain, with occasional happy and frequent unhappy surprises. In addition to technological uncertainty, R and D shares with other kinds of time-consuming investment what is called

environmental uncertainty -- uncertainty about the kind of new product that will be saleable or useful in the unknown environment of the future years in which it will be available.

What constitutes sensible behavior when we are confronted by gross uncertainties? No one has succeeded in defining it with precision to general satisfaction. But certain of its characteristics are well understood. It makes sense to hedge, to preserve flexibility. Where there are several possible paths to an objective -- say several competing R and D ideas -- each of which is uncertain, it frequently makes sense to try several. In fact it can be proved that under many circumstances the multiple path approach produces results more quickly and more cheaply than attempting in advance to choose the optimal path and concentrating all one's efforts in pursuing it. In R and D, particularly R and D involving significant advances in technology, there is frequently no substitute for a practice described by that naughtiest of words -- duplication.

All this is well understood -- implicitly and in effect -- by the competitive economy. In attempting to develop higher energy fuels or tougher tire treads a dozen competitive firms will try at least a dozen different ideas. Good industry labs will themselves frequently try out alternatives: firms competing with them may be trying out many more alternatives -- indeed some may be trying the same ones. The competitive economy has a good, perhaps the best, way to promote parallel R and D projects -- individual initiative, carrot and stick. Each firm tries the path or paths it likes best, receives a big prize from the market if it turns out to be right, and suffers penalties if it turns out to be wrong, or slow. The essence of this method is another naughty word in military

or governmental research -- competition.

Nobody has been able to determine just how much duplication in research is optimal. The competitive economy may well promote too much or too little in some circumstances. But some tendencies of the competitive economy are demonstrably right and deserving of emulation. There should be more duplication in R and D, the greater is the potential payoff. There should be more, the greater the uncertainty. There should be more, the cheaper is duplication. These are precisely the results achieved by the profit motive in a competitive economy.

I am far from arguing that all is perfect with R and D in the competitive economy. On the contrary I suspect that it has two major weaknesses, or biases, if we evaluate it from the point of view of the nation and its desirable rate of economic growth:

a. It probably spends too little, in total, on R and D as compared with, for example, investment in physical facilities

b. Within R and D it probably, like the military, spends relatively too little at the basic research and exploratory development end of the R and D spectrum and too much at the short payoff, prototype production or design end.

These probable biases within the competitive economy are fairly widely recognized, although there is some disagreement about the causes. It is sometimes argued, for example, that firms spend too little on R and D, and especially too little on basic research, because it is such a risky, uncertain business. This is not very convincing. Many businessmen like risks when possible rewards are high. There is no shortage of prospectors in risky mining ventures if the strikes are rewarding. Far more important,

in my opinion, is the great difficulty, in a competitive economy, in latching onto property rights in new ideas resulting from R and D. The firm that pays for the R and D is frequently unable to reap many (in some cases, any) of the benefits. It may not be in a position to exploit the particular discovery that it makes. Even if it is, patent law and secrecy provide very incomplete protection from its appropriation by competitors. This inevitably discourages expenditure on R and D by competitive firms, particularly expenditure near the basic research end of the spectrum, where results are less predictable, less likely to "fit" the circumstances of the sponsoring firm, and where patent protection is weakest, or non-existent.

I am not suggesting that the appropriate remedy for these shortcomings is exhortation or a tighter patent law or any other form of inducement to promote more basic research in the individual competitive firm. We can't expect the competitive firm to act against its own interests, and there are costs as well as gains in tightening the patent laws. More basic research is needed in the economy, but not necessarily within the competitive firm. Basic research is the special province and opportunity of the university, the government laboratory, and perhaps of the specialized research firm.

There is room for some experimenting with new institutional arrangements for doing research and development in the competitive economy, and there has fortunately been some experimenting (although not enough) during the past 15 years. In some industries (e.g., chemicals) patent law appears to provide a good deal of protection to inventions and therefore a fairly adequate inducement to the firm to undertake research. In others (e.g., the metal fabricating industries) patent law appears to be almost completely

ineffectual, and the most a firm can expect from a successful development is a short jump on his competitors. Research in industries in this group is obviously unattractive, and there is little of it that isn't financed in some manner by the government. Promising institutional developments include:

a. The specialized research firm (Arthur D. Little, Battelle, S.R.I., etc.)* that can drastically reduce the cost to the small or new competitive firm of exploring a new idea. The overheads of laboratories and their equipment and permanent staffs are a formidable obstacle to research and development by any except the largest and best established firms. When the costs of these overheads can, in effect, be shared with other firms, a great many more new ideas can be tested. This is one way in which the important economies of scale in research can be realized, in part, by smaller firms; and the ability of the established firm to take a long view can be matched, to some extent, by the new firm.

b. Various forms of industry-wide sponsorship or other pooled research. A fundamental obstacle to firm-sponsored research is, as we have seen, the difficulty the firm has in latching onto property rights in new ideas resulting from the research. In general, the broader the base of the sponsoring organization the more likely it is to be able to profit from the results. Thus, unless the idea is patentable, the small competitive firm is certain to lose any advantage quickly to competitors within the industry. Or the idea may be one which the particular sponsoring firm, for some reason, can't exploit at all (e.g., I have a coal company and develop an

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* Most but not all are organized on a non-profit basis.

economic method of gasifying coal, except that it won't work for the grade of coal coming out of my own mines). If the research is sponsored by an industry the chances are much greater that the new ideas can be exploited within the industry, and in a good many cases that they will be exploitable only within the industry. Although they will not improve the position of one firm in the industry in relation to others except by accident and at random, they will tend to improve the competitive position of firms in the industry in relation to firms in other industries. The example of industry-wide research with which I am most familiar is the N.A.C.A., which deserves a large share of the credit for the technological progress of the American aircraft industry. I am sure it would pay many industries to support substantial programs of industry-wide research. It is almost as unreasonable to expect much support for basic research from the small competitive firms in such industries as coal, machine tools, and textiles, as to expect it from farmers. In the case of agriculture we found another solution through government-supported research, which has strikingly (almost embarrassingly) the productivity of American agriculture in the past few decades. Government supported research is also helping the coal industry to find new products and processes to strengthen its position in relation to its newer and more glamorous competitors. The same economic theory that warns us against letting firms combine to restrict output or raise prices, suggests why it may be in the national interest to encourage them to collaborate in supporting R and D.

Military R and D may share both these weaknesses or biases with R and D in the competitive economy -- spending too little on R and D in general, and relatively too little on basic research and exploratory development.

Certainly it shares the second. In the government the riskiness of a research project is more of a deterrent than in the economy: the government official has to be cautious -- the penalty for a striking failure is almost always greater than the reward for a striking success. So is the fact that the payoff from research may occur only in the distant future: military research administrators normally rotate at three year intervals, and the political officials and Congressmen above them rotate fairly frequently too. In fact, everyone above the level of the laboratory seems to have some good personal reason for demanding quick results -- a circumstance unfavorable to basic inquiry.

The organizations responsible for military research also have difficulty latching onto property in their discoveries. Especially if these are fairly basic, potential enemies or the other services may be in as good or a better position to appropriate them. Patents are worse than useless in this case; secrecy provides only imperfect protection -- even from the enemy. But keeping the technological jump on the enemy -- even if measured in months -- may be even more important than keeping the jump on competitors in an economy. It may indeed be essential to our survival in the nuclear age. And this requires a broad program of basic research and exploratory development in the relevant scientific and technological fields.

It is apparent that we cannot simply transfer the desirable characteristics of private, competitive R and D -- its uncoordinated, competitive, duplicative characteristics -- to the military and governmental arena. For one thing, military objectives, although dimly and imperfectly perceived by the authorities, are more definite than the diffuse desires of millions of consumers which, directly or indirectly, the firms of a competitive economy

work to satisfy. This in itself imposes a requirement for planning and coordination that the competitive economy doesn't have. We have to provide intelligent, informed "consumers" or buyers of military research.

More important, no ingenious expert in the field of management has ever devised a half-way effective substitute within government for the incentives of a competitive economy -- the carrot and stick incentives associated with competitive firms risking their own money and subjected to the test of impersonal markets. In the military arena we find instead, in the case of R and D, government laboratories subject to civil service rules and cost-plus contracts with industry. There are better and worse ways of managing government laboratories and cost-plus contractors, some of which provide moderately effective incentives; but with so imperfect a solution to the incentive problem, with public rather than private funds being risked, some discretionary control to prevent waste and malfeasance has to be exercised at responsible levels of government.

Having said all this, it is my conviction that some of the competitive economy's strengths in R and D can be emulated by the military; that the basic trouble with military R and D is that it already leans too far in the direction of centralized bureaucratic solutions to its problems.

a. It is too highly centralized. Decisions get made at too high a level. Detailed budget control is exercised at too high a level.

b. Precise paths (with far too little flexibility) are dictated by administrators to researchers.

c. There is too little duplication, especially at the level of exploratory development, where it is cheap.

d. There isn't enough competition among laboratories and contractors, or even among the services. There aren't enough competitive purchasers of new ideas, or enough competition in exploiting them. Where there is competition among services or agencies, we tend to make rapid progress. Where we suppress it, we don't do well.

But almost all the proposals for improving military R and D are aimed at strengthening central planning and coordination, adding new layers of authority, and getting rid of what little desirable duplication and competition we have left. They would create neat, dead, bureaucratic monopolies.

That some of the strengths of the competitive economy can be emulated by governmental research organizations has been demonstrated time and again. Two of the best examples, ironically, are the Manhattan District Project and the wartime OSRD -- both hailed as ideals by our contemporary centralizers. The MDP typically solved its problems by letting each of half a dozen different firms or university labs try a different path to the objective, competing vigorously and in secrecy from each other. The wartime OSRD was perhaps the most loosely knit, decentralized, uncoordinated species of research organization that ever existed -- and spectacularly successful.

If we would let it, our competitive economy could teach us that in <u>all</u> R and D, including that sponsored by the military services:

a. The quickest (and frequently the cheapest) way to achieve many research objectives is to try multiple paths (some of which, in retrospect, are inevitably <u>cul-de-sacs</u> and apparently wasteful).

b. The greater the payoff and the greater the uncertainty, the more duplication and apparent waste make sense.

c. The cheaper the multiple paths, the more it makes sense to try. More should be tried in research than in development; more in exploratory development than in weapon system development; more in weapon system development than will eventually be procured and made operational.

d. The most expert predictions of the results of R and D are highly unreliable. Frequently the only way to learn whether an idea will work is to test it.

e. The person best qualified to choose a path (as opposed to an objective) is the person doing the research -- or someone very close to him.

f. There is no incentive quite as effective as the competitive spur. The problem in managing governmental R and D is not how to suppress competition, but how to divert it into more productive channels.