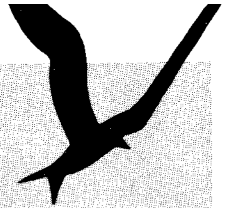

ENERGY

Our Energy Future: Time Horizons and Instability

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This is a pessimistic essay about facing the long-term problems of energy and resources in the United States. It is concerned with the effect of time horizons and sudden policy changes on U.S. responses to energy and resource scarcities. Two characteristics are critical for any policy or program designed to address those scarcities: first, a time horizon consistent with the nature of the problem—long enough to carry out the needed research and development and short enough to start meeting the anticipated scarcities as they arise—and second, stability for that research and development, so that new research can build on old, a corporate memory of the insights can accumulate, and a renewing body of scientists and engineers can evolve to use and further the new science and technology.

Since 1945, the United States has had a mixed score on achieving the two critical characteristics of timing and stability. Moreover, recent trends are not promising. A review of some events of the past 16 years will illustrate weaknesses in U.S. policy and planning.

In 1973, the shock of the sudden rise in energy prices threw U.S. citizens and industry into a turbulent economic and social readjustment. Although this particular crisis could not have been predicted, some experts had been advocating a reduction in energy demand and a search for new sources, based on chronic and endemic concerns about energy supply. These concerns went largely unheeded. Only the motivation of an acute transition into scarcity was sufficient to stimulate exploration into ways to use energy more efficiently.

The governmental response to the 1973 energy shortage, in contrast to that of the energy-intensive industries, seems, in retrospect, to have been myopic, characterized by an unwillingness to consider any response except increasing the energy supply. The perspective that determined the U.S. institutional responses—particularly the governmental response—was very much that of the U.S. energy suppliers. Consequently, policymakers underestimated the flexibility of the users, particularly the capacity for substitution and for reducing the demand for energy on the part of the major industrial users. Economic modelers likewise were naive about the ability of industries to improve their housekeeping quickly and to replace inefficient capital equipment with more energy-efficient equipment over a period of a few years. The modelers failed to realize what industrial engineers knew very well: that energy and capital might either substitute for or complement one another in the short term but, under the stress of an energy scarcity, capital would substitute for energy in the long term. Although an "energy conservation" program that supported some applied research and development was established by the Department of Energy, its achievements were hardly among the department's most distinguished. Fortunately, industries with the most to gain by reducing their energy demands did act in their own best interests and invented new technologies to accomplish large reductions in their energy requirements, typically 25 to 40 percent over a period of about five years.

At the same time, however, some managers, engineers, and economists were overly optimistic about how easily alternative energy sources could be found and made economical. Programs for nuclear fusion, new breeder reactors, magnetohydrodynamics, solar energy, synthetic fluid fuels from coal, geothermal energy, and other sources were accelerated and given a high priority. Then, as petroleum prices slipped, the motivation to support these programs evaporated, and many, such as the synfuels program, were cut back and eventually closed. Now, 16 years after the "energy crisis," the mix of U.S. energy sources has not changed significantly. Although the reasons differ from source to source—from public opinion for nuclear power to cheaper alternatives for solar energy—reducing or closing research and development programs as the immediate need for new sources dwindled has been a significant factor in the demise of many energy projects.

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It seems at first glance to make economic sense to halt research and development for a product already available more cheaply from some other source. Such a policy is correct, provided it is based on the answers to the right questions. But it is foolish to use the truisms of diminishing nonrenewable fuel supplies and rising costs of basic materials as mandates for headlong, frantic searches for new energy sources, substitute materials, and alternative lifestyles. The rhetoric of the 1970s made it seem as though long-term problems were the justification for creating the Department of Energy in 1977 and the administrative burdens that go with it. Now it is apparent that institutionalizing energy research and development within the U.S. government was motivated by a short-term political and economic problem: The world's oil markets were changed suddenly from primarily competitive to primarily oligopolistic by the formation of the Organization of the Petroleum Exporting Countries (OPEC) cartel. The shock of high prices demanded by the OPEC oligopoly forced a multitude of technical, economic, and political responses. One response was the quick, lavish subsidizing by the federal government of research and development for alternative fuels; another was the creation of administrative machinery to distribute the funding for that research and development. Because the fuel scarcity of 1973 was due to oligopolistic market forces and not to true scarcity of the physical resources (in the sense of the eighteenth-century English economist, David Ricardo), the acute driving force for alternatives disappeared when the price of crude oil fell, as it almost inevitably had to. But the cycle of sudden rise and slow decline of oil prices did impel renewed examination of long-term energy supply and demand.

The outlook for the long-term situation is one of reasonably extensive supplies of oil and gas for several decades and of coal for much longer. (If the highly uncertain hypothesis of the meteoritic origin of natural gas were to prove correct, natural

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gas resources would be at least as great as coal resources, probably greater, but probably significantly less accessible.) It has become clear that there are many ways to extend these resources, from improving the efficiency of extracting fuels from the Earth to improving the efficiency of the devices and processes in which the fuels are used. But it is equally clear that some day, possibly within the next two or three decades, alternatives to oil and, very likely, to natural gas as well will become necessary. Finally, the research and development to create economical processes for supplying those alternatives will take many years. Whether new energy sources come from liquid fuels, coal, portable solar energy, or batteries, the lessons of the late 1970s and early 1980s taught scientists and policymakers to expect an interval of 20 years or more, probably significantly more, from the beginning of a vigorous, well-funded research and development project for an alternative energy source to the time that the source can begin to contribute noticeably to the total amount of energy used. ("Noticeably" means a contribution closer to 3 to 5 percent of total consumption than to 30 to 50 percent.)

Thus, a long-term, stable program of research and development for alternative energy supplies is eminently sensible now, even mandatory, if alternatives are to be available when oil and, perhaps, natural gas resources begin to become scarce. Furthermore, such a program permits the option of placing heavy emphasis on the far less costly research side of research and development and of keeping many possible solutions open for the present, rather than having to commit now to an expensive program of development. Development can wait for a clearer understanding of how the alternatives will compete with one another and with conventional fuels.

Unfortunately, only the barest minimum of such long-term, relatively low-level research is being carried out. The Gas Research Institute in Chicago is using funds it receives from its participating utilities to conduct limited research programs, for example, but most research on finding alternative energy supplies and on reducing energy demand has simply stopped.

Superficial cost-benefit analysis argues that cutting off research was the right decision, particularly in the years of very high interest rates. The benefits of long-term research are simply too small to justify investment if future benefits are discounted at 15, 18, or 20 percent per year. More generally, if decisions regarding the continuation of research and development programs continue to be dominated by periods of highest interest rates, the smartest course is to drop long-term research altogether: The squeeze on long-term research from high interest rates in one period forces its termination or at least cutback; subsequent reconstruction of a research program in the next period of lower interest then becomes very expensive, far more costly than continuing the research without a break. The logical solution is to do without such long-term research from the start.

Impeccable logic, indeed. But this analysis overlooks some firms' practice of carrying out long-term research and introducing innovative processes. In that situation, firms that choose otherwise are inevitably driven out. The example of Japanese and then Korean steel makers and what they did to U.S. steel makers has been told over and over. The long-term worth of a firm is tied intimately to its long-term investment in research and development. The choice of whether to carry out long-term research depends on the long-term worth placed on the firm, which presumably reflects the real value—the productive capacity and competitiveness—of the firm. The recent history of decisions about the role of research in many U.S. industries has been testimony to flawed policies in which the long-term worth of the firm was simply not important. Occidental Petroleum, Allied, and Exxon all devastated research programs that had been established to make those companies competitive in the long run.

The emphasis on short-term research payoffs in U.S. firms is in no way limited to energy research. Some reasons for this emphasis are reward systems that fail to offer executives incentives to focus on the long-term health of the firm, coupled with inducements for financial manipulations such as leveraged buy-outs and the sale of junk bonds, which wipe out the cash supply necessary to maintain a healthy research and development program. Succumbing to new kinds of quick profit-making at the expense of its sustainability, U.S. industry has handed the future over to firms and nations that have resisted turning their industries into pieces in a Monopoly game.

Long-term industrial research in the United States could be stabilized by offering industries tax incentives for long-term research programs, for example. Executives could be encouraged into long-term planning by making the return on their retirement funds dependent on the firm's performance when the retirement payments are made. Similarly, long-term futures or shares to be delivered at a distant, specified future date, even to heirs, could be used as stock bonuses instead of current stock. At least firms can now collaborate in research programs of mutual benefit; this is probably a good step.

The survival of a highly technological society depends on finding ways to bring the time horizons for political and financial decisions into harmony with the natural time scales for research and development. It is virtually impossible to control the rate at which novel ideas appear from basic research except by controlling the number of creative people who are given the inducements to work in science and the wherewithal to pursue their work. At the next stage, proof-of-principle, the rate of progress can be controlled somewhat more by confining support to those projects that show greater promise than others. By the time the work is ready for development and deployment, the questions are well defined, and skilled technicians can transform the proven principles into practical devices and processes. The rate of progress here can be defined significantly by controlling the level of funding, whereas the first two stages—creative research and proof-of-principle—cannot be accelerated beyond some maximum rate simply by pouring money into the project.

Unless the initial decision to undertake a research and development project includes a commitment to see it through, there is always a risk that the accumulated knowledge and skill of a partially finished project will be discarded. If this happens, the entire investment is likely to be lost, because the cost of restarting the project is prohibitive. One way, then, to help steer the decisionmaking process toward more stable, more economical research is to include the cost of halting and restarting a program in estimating future costs and benefits. The high, very uncertain costs of restart must be weighted by the probability that the program will need to be restarted, another very uncertain quantity, to compare them with the cost of maintaining the program. Despite the difficulty

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of estimating such uncertain quantities, it will often be cheaper to keep the program active than to close and then restart it.

Without greater stability, research, together with its basic components of funding and staff, is vulnerable to sudden changes in support. There is already a pattern of swings both in support and in scientific manpower, with the amplitude of the swings growing with time. This instability is exacerbated by the mismatch of time scales for research and its supports, because each period of renewed support must correct for the last mismatch of what was needed with what could be provided. The supply and demand of scientific and engineering personnel is the clearest current example of these growing oscillations.

What if the United States fails to stabilize scientific research or bring the time horizon for policy into line with that for research? There are several possible outcomes. One is simply that the total cost of maintaining a technological society will be a little higher than otherwise. When the time of real fuel scarcity arrives, consumers and industry will have to pay the cost of living with that scarcity until alternatives are found. A more severe consequence, suggested previously, is that U.S. industries would continue to become less and less competitive in world markets, reducing U.S. status and comforts relative to those of competitor nations, as has happened in Britain and some other European countries. In this scenario, U.S. citizens would need to rely on others to provide them with the necessary alternative fuels. Still worse is the possibility that the current level of technology could not be maintained, with the United States unable either to develop low-cost alternatives or to buy them from others in roughly the quantity needed to sustain the status quo. In this case, U.S. society would revert to a lower absolute standard of material goods and services. Whichever of these scenarios comes to pass, it is unlikely that the United States will benefit from continuing to play with long-term research to meet long-term societal needs as if it were a luxury. The only available luxury now is the time to conduct the research before the needs for its products become critical.