Session III
APPLICATION OF SCIENTIFIC KNOWLEDGE

THE TECHNOLOGICAL RELEVANCE OF BASIC RESEARCH

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1. Introduction

The popular concept of technological development of scientific discovery is that of a linear progression from basic to applied research, which in turn, leads to technological application, and the marketing of a product. Many studies, however, question this orderly and unidirectional development. We especially lack good explanations of why and how "the connections" may be made within a matter of months or years – or the basic ideas may lie dormant for decades before application can be perceived and a new technology developed. At times, a technological breakthrough may advance basic research, thus generating new knowledge that can be applied to technology. Technological problems may also stimulate scientific research which permits the technological development which leads to additional science.

But commonalities override these subtle distinctions. There is general agreement that the procedures, the outcomes, and the objectives of "technology" differ from those of "science", that there are boundaries between science and technology to be overcome or dissolved; that communication across two separate bodies of knowledge must be improved and interaction accelerated.

The focus of this paper is on the interaction between science and technology: What do we know about different points in this space and about what is outside of it? The paper tries to establish a sufficiently general framework which accounts for interdependencies between science and technology advance throughout the frontier. An interpretation of these "scitech" advances – those technological improvements which involve scientific discovery – is attempted.

Scitech advance is what some believe to be the coming norm, as indicated in the NSFs 1983 *Five Year Outlook on Science and Technology.* [1] Creation and use of new lasers, the quick exploitation of recombinant DNA methods, the discovery of brain peptides, and the creation of polymetallic cluster catalysts out of surface studies are cited in the report as evidence of today's more rapid and more extensive application of scientific discovery.

The pace is also evident, the report goes on, in the emergence of a new biotechnology industry principly relying on scientific work done in the past ten years. Industrial development of very large scale integrated circuits depends on scientific work done almost in parallel – materials and solid-state science, new

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spectroscopies, and atomic and molecular physics. The pharmaceutical industry has altered its approaches to designing, creating and testing new drugs, governed by the new knowledge provided by recepetor biology of how signals enter and change the behavior of cells.

We are now in an era of quickening scientific and technological change, the report concludes, and the two intertwine ever more closely.

Of the $8.8 billion spent on basic research in the United States during 1981, industry spent about $1.5 billion – about one-fifth as much as the government with its large expenditures for defense and space research. Basic scientific research constitutes for most U.S. corporations only a small part of total R&D effort. This varies significantly across firms, however. For example, the research division of IBM employs some 2000 professional or technical personnel, one-quarter to one-third of whom deal with problems comparable to those of academic scientists. However, annual expenditures for basic scientific research at Merck, one of the largest U.S. pharmaceutical houses, amounts to only a few percent of the research and development budget.

We will suggest that for an organization with a wide technological base – computers, telecommunications, energy – there is a high correlation between the value of the technical advances generated by research and the scientific advances achieved by research. Certainly there are major qualifications to this. But for a company with a wide range of technology problems, the criterion "scientific value" is probably as good or better, for today's R&D management, than one that stresses the value of specific technologies.

The first section of the paper advances a "theory" of scientific discovery aimed at explaining why basic research is increasingly important in generating new technologies today. The paper explores the idea that technology development involves attempts to bring phenomena (with potential market or social value) within the range of basic science paradigms. Given "technological" problems, such as, for example, producing chemical compounds with certain properties or switching and amplifying electrical signals, we know certain technologies emerged with solutions to these problems. These were petrochemical processes and semiconductors. The model demonstrates how scientific discovery plays a role in these problem-solving activities.

The paper attempts to identify the multiple ways in which progress in scientific specialties influences technology development. Several aspects of progress are distinguished: theoretical advances in a specialty; improvements in "instrumentation" in the specialty which allow laws to extend the application of the science to new phenomena; and connections between scientific specialties.

We discuss how the rate of science into technology may be changing. The paper concludes by looking at research advance in several scientific specialties on the current search for an "ideal plant" technology.

2. A Theory of Scientific Discovery

The principle attempt to explain (rather than assume) the production of scientific knowledge derives from the view that scientific knowledge is a commodity, and results from an input-output process. Scientific advance or progress can be defined in term of a moving production possibilities curve, and/or in terms of increasing output. While assessing the output may be extremely difficult, there is a flow through basic science of information, generally resulting in research publications. This attempt, while representing some advance, fails to capture the entire complexity of scientific procedures. Excellent discussions of its "black box" approach to science (and the role of science in technology development) attest to this.

Roberta Miller writes that most analyses assume that inputs are invariant in their relationship to outputs such as scientific advance or economic growth. This assumption is usually coupled with the further assumption that the internal structure of the R&D process is also invariant. It would be more realistic to assume, she continues, that inputs are converted into outputs at rates and by processes that differ from field to field and, within fields, from discipline to discipline. "Production" depends not only on the input resource, but also on the efficiency and creativity with which they are used, and the structure of scientific and technological problems in the field.[2)]

The definition we suggest of science is, on the contrary, more inclusive, and we think much more accurate of the way science works. Science develops as a set of rules to solve "difficult" problems. Both "theoretical" rules (applicable to problems although not necessarily already applied) and "non-theoretical" rules (applied to problems) play important roles. Technologies can be achievements in the advance of a science through these defined problem solving activities.

A "model" which includes the relevant problems and the rules of inquiry into those problems, using the terminology of Thomas Kuhn, defines a scientific paradigm. A paradigm is at its start largely a promise of success discoverable in selected and still incomplete examples, Kuhn writes. "Normal" scientific advance consists in the actualization of that promise, achieved by extending understanding of those facts that the paradigm displays as particularly revealing, by increasing the extent of match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself.[3)]

Kuhn also uses the term paradigam to mean the series of "exemplars" or particular achievements within a science which guide future problem solving. One way to think about this is that paradigms provide the basis for individual researchers (and the managers who invest in their projects) to "gamble" on future problems. Ziman writes that if there is a conscious rationale to problem choice among scientists, it is undoubtedly akin to a gambling strategy: a stake of time and effort is to be risked in the hope of a pay-off. A potential research project is usually evaluated schematically as a question of a certain degree of *importance* whose answer might be obtainable with a certain degree of *difficulty*.[4)]

Scientific paradigms allow determination of "the value" of problems and "the probabilities" of solving these problems in the following way: A range of phenomena attracts the attention of a scientific community, either because the phenomena have some, perhaps, market or social value, or because they constitute "anomalies" for existing scientific paradigms; these are valuable problems to the community.5)

A paradigm is then developed that is "successful" in dealing with this limited range of phenomena. This initial success raises expectations that the same paradigm will (probably) be applicable to other phenomena that are, in some sense, "like" those in the initial successful application. These expectations then generate a variety of "research problems" whose value to advancing the paradigm-directed research program is "determined internally," and whose probability of solution depends on the guidance offered by the paradigm.

Even when the initial problems from which the paradigm starts are chosen for their technological (social or market) application, the problems that the paradigm-directed research program chooses for the further working out of the paradigm *may* not be. We can become even clearer about how paradigms assign "values" and "probabilities" to problems.

3. Assigning Probabilities to Problems

Two components are usually involved in assigning "probabilities" to problems: (A) translating the given description of the problem into the "non-theoretical" apparatus provided by the paradigm; and (B) finding a special theoretical law in the apparatus of the paradigm that "captures" the translated description.

Under "translating" is to be understood empirical work – design of experiments, instrumentation – that yields data in the form that the paradigm's non-theoretical apparatus is prepared to deal with. Whether translating is trivial or not, paradigm applications generally do both (A) and (B) and they generally do (A) better than (B).

Bradshaw, Langley and Simon, in a recent article in *Science* magazine, attempt to simulate some of the important processes of scientific discovery by a computer program. They provide several examples of relatively long time separations between (A) and (B). In modern science, they write, it has become common for "laws" to precede, sometimes by many years, the theory that could account for them.6)

Within this approach, paraphrasing Pasteur, we may explain why there is not science and technology: there is only science and the application of science. That is, some science (scientific paradigm) applications are technology phenomenon, that is, applications with social or market value.

Technology applications, further, can fall under scientific paradigms at different stages of development. "Data-driven" induction of Black's law, Bradshaw, Langley and Simon argue, could have been achieved simply by carrying out a succession of controlled experiments, from which a law emerged. In some sciences, at the opposite extreme, there exists a theory so powerful that laws can be deduced from it with almost no induction at all. But lying somewhere between pure induction from phenomenon and pure deduction from theory are sciences where "incomplete" theoretical constructs guide induction but fall short of permitting *a priori* deduction.

We can also identify specific attempt to bring phenomena within the range of intended applications of a scientific paradigm. We may say generally that paradigms generate probabilities of making translation into their theoretical and non-theoretical apparatus, and think of this as a "probability distribution" over all problems – scientific and technological. For scientific problems, these probabilities typically become higher during the "life" of a paradigm. But probabilities may differ significantly among the problems and change over time.

Intuitively, we can distinguish "degrees of ripeness " among those problems that we have a good chance of solving, at a given time, in a paradigm's historical development. Further, when we vary the paradigm and hold the problem fixed, the probabilities shift from relatively high to virtually zero. Problems ripe for one paradigm will typically be unpromising for another.

For technology problems, the "given" description of the kind of phenomenon we are trying to translate into the non-theoretical vocabulary of the scientific paradigm's conceptual apparatus may be too far removed from those the paradigm usually deals with. For some technology problems, we may attempt to model the technologically interesting phenomenon into different paradigms. All this leads to a great deal more uncertainty about which technology problems are ripe for which paradigm. But we should be clear that there is no sharp dichotomy between science and technology (problems) with respect to probabilities. What we have been talking about is most naturally viewed as characterizing a continuum.

4. Assigning Values to Problems

In addition to the probability, the value or importance of a problem enters into the gambling strategies of researchers. Not all problems are equally valuable. The greater uncertainty in applying paradigms to technology problems need not lead to inability to assess the value of these problems. The values here may be much more clear-cut than in the scientific situation. Values in the technology case can always be "externally" generated (through attempted market or societal assessments) as opposed to "internally" generated by the paradigm.

The values of problems "internally" generated by a paradigm seem initially much harder to assess. But we can begin to characterize the scientific value or importance of problems in terms of the development of paradigms:

- ○ Problems which extend the paradigm's applications to doubtful areas and unify scientific specialties are more valuable than those that do not.
- ○ Problems that improve the application of the paradigm to already-known applications are more valuable than those that do not. Existing applications can generally be improved by producing "special laws" with narrower scope. Among the ways of doing this are: determining constants appearing in them with greater accuracy and removing ambiguities in the form of the law. Such improvements are permitted by parts of the paradigm that postulate certain kinds of connections "laws" among its applications that function to relate intended applications to known-applications.

- ○ Intended applications of one paradigm that are also intended applications of another paradigm are more valuable than those that are not. They are still more valuable if the intended applications are known-applications. This reflects a value that scientists place on connecting their specialty to other established parts of science.

We may say paradigms generate a value distribution over scientific problems; these values typically become lower over the life of a paradigm. Emerging paradigms generate problems with high values (and with high degrees of uncertainty). Fading disciplines generate problems with declining scientific value.

One manifestation of this is scientist interest in the "newness" of scientific specialties. One study finds that 50 percent of the references cited in *Physical Review* papers (over the period studied) were less than three years old, and only 20 percent were more than seven years old.7)

Other evidence is provided by Holton who uses this development pattern of paradigms to explain scientist mobility. In his view, the amount of remaining valuable ideas in a new field is essentially the mirror image of the applications, with a suitable time lag. As the paradigm develops, more scientists begin to work in the field and the number of participants increases. As the number of valuable ideas becomes exhausted, increasing numbers of scientists leave the field for other areas.8)

One important question, is when to move to another field or, with respect to a research manager, when to shift personnel. Journal publication has a lag time, and there is a further lag, for a researcher to set up new experiments. Thus, by the time the peak of participation is reached, the field is already in the declining phase.

5. Correlations Between Scientific and Technical Value

Schmookler is often cited for his demonstration that U.S. technology in four important industries, railroads, agriculture, paper making and petroleum refining, resulted in response to economic demand and not from scientific research. Smookler did not think science was unimportant. Rather he looked upon modern science as constituting a sort of silly-putty clay out of which technology can be shaped. As he states, mankind today possesses, and for some time has possessed, a multi-purpose knowledge base. We are, and evidently for some time have been able to extend the frontier at all points.9)

If Schmookler is right, then we need not pay too much attention to scientific progress in order to understand the direction of inventive activity. If he is right, then science does not function as a major independent force in shaping the timing and direction of the inventive process. On the other hand, if Schmookler is wrong in this respect, then his analysis needs to be supplemented by a careful examination of the manner in which the development of scientific disciplines shapes and structures invention.

We can perhaps start by saying that paradigms can generate a value distribution over some technology problems, possessing certain characteristics: There is a distinction between bridge building and development of robots with sight, for example, in that the former is mainly a problem of how to take best advantage of existing information, while the latter involves also a good deal of "learning". In the one case, the technology problem is how to make the best use of known alternatives; the other requires new scientific knowledge.

Second, it can be further argued that for some technological problems, there is a correlation between technological value and scientific value: major technological advance proceeds from a scientifically valuable problem. In science the result of major change is a new paradigm or a significant paradigm advance. In technology one has a major innovation. Therefore follows one basis of the presumption that somehow scientific advance produces "technological" application. More appropriately restated, important paradigm-applications have technological value.

Examples of correlations between scientific and technological values are a part of the history of technology. The progress made in techniques of navigation in the sixteenth and seventeenth centuries owed much to the great demand for such techniques in those centuries. But it is also true that great demand existed in the same period for improvements in medicine, but that no such advances were forthcoming.

One explanation is that the state of mathematics and astronomy afforded a useful knowledge base for navigational improvements, whereas medicine at that time had no such base. Progress in medicine had to await the development of the science of bacteriology in the second half of the nineteenth century. Although medicine attracted considerable sums of money, medical progress was very small until the breakthroughs of Pasteur and Lister. Improvements in the treatment of infectious diseases required progress in a highly specific discipline – bacteriology.10)

Demand can assign technological value to certain problems, but the paradigm may not be ripe. Until major breakthroughs in biology, organic chemistry, and genetics allowed systematic regulation and manipulation of plant and animal growth in the twentieth century, science could provide only limited assistance to agriculture. The conclusion drawn from one study of about fifty major innovations was that mechanical innovations appear to require the shortest time interval, with chemical and pharmaceutical innovations next. Electronic innovations took the most time.

Much of technological advance results in changes in product performance in various dimensions – faster, safer planes, or robots that receive visual images as well as receive sound signals. Explanation of technological advance becomes very difficult. To (at least partially) understand technical advance, including an account of varying rates of progress at different periods in history, we seem to need to include scientific breakthroughs. Measures or indicators of the magnitude of advance over time in technologies correlate with major scientific discoveries.11)

It may even be possible to formulate some general characteristics of technologies which depend on scientific advance: technologies meeting highly specialized requirements; technologies requiring more complex solutions (optimizing among multiple dimensions); technologies with relations of substitution (rather than of complementarity) to existing technologies; technologies which relate to several disciplines.

More theoretically, Rosenberg writes that the analytical justification for the notion of distinct shifts in well-defined production functions seems to be that some breakthroughs in scientific knowledge bring with them whole new ranges of more efficient factor combinations for producing a commodity. There is, in effect, a "spillover" phenomenon, affecting several points on a hypothetical isoquant and not just one. Clearly, he concludes, this sometimes happens, although we need to know much more about the empirical evidence in support of such a view.

The (continued) rate of advance in a particular technology (improvements of the tradeoffs in its dimensions) depends on the possibilities for scientific advance in its accompanying paradigm. Even if the technology race in the semiconductor industry had produced a smaller number of survivors and a more concentrated industrial structure, Levin writes, it is not obvious that innovation would slacken.12) When we isolate the independent forces which jointly influence both market structure and innovative performance, these forces include the inherent scientific opportunities confronting the industry.

The consensus view of experts in semiconductor technology, for example, is that there are no fundamental physical limitations to the further pursuit of miniaturization over the next decade. There are eventual thermal constraints on the density of circuitry contained on a chip, which will ultimately necessitate a transition to superconductor technology for some applications.

The rate of substitution of one technology by another technology (even a clearly superior one) requires comparisons between relevant scientific paradigms. Take the example of the oil-powered internal combustion engine. Changing oil prices put an increasing pressure on oil substitution and energy saving. The scope for substitution however was limited by the relevant sciences which defined the range of possible technological advances.

The argument of this paper is that, if we want to explain the historical sequence in which different important consumer demands or social needs have been satisfied via technology, we must consider the progress of science along lines determined by its "internal" logic. Historical evidence confirms that inventions are rarely equally possible in all commodity classes. The state of the various sciences makes some inventions easier and others harder. We must consider the manner in which scientific knowledge grows and the manner in which this growth shapes inventive activity. The electronics and chemical industries are closely related to scientific disciplines. In the case of aviation, an entire new discipline was developed.

Some industries were, in a sense, developed close to science; others formed their own science base. Even here however there are problems that are less interesting from a scientific point of view.13) Our question is more specific: Does technology call attention to problems that play significant roles in scientific development?

Chemical corrosion, as an example, is a very costly phenomenon occurring on bridges and in nuclear reactors. Detailed knowledge of the process is essential to the design of corrosion-resistant materials or corrosion-preventing coatings. There is the possibility of expensive, empirical, trial-and-error solutions giving way to ideas based on understanding of the relevant phenomena at the atomic level.

Basic surface studies are providing extensive new knowledge of the various individual atomic and molecular processes that together constitute the phenomenon of heterogeneous catalysis. Gaps in our knowledge persist, however, particularly because we cannot apply atomic surface analysis techniques during the high-pressure catalytic process. Still, it is not unreasonable to expect that the knowledge surface science is providing at the atomic level about surface chemical reactions will lead to improved catalyst design, which now depends in many instances on semiempirical methods.

But some claim that technology deals not with the general and the universal of science but with the specific and the particular. In agriculture, much knowledge is location-specific. Technological change deals with small increments to the stock of knowledge which are, from a strictly scientific point of view, uninteresting. What seed varieties will grow best in a specific geographic location with its unique combination of rainfall, soil chemistry, topography, etc.? What characteristics can be genetically engineered into new seed varieties to improve future crop performance? In the exploitation of mineral resources, the enormous variations in the richness of mineral deposits pose unending technological problems as we move from the exploitation of high quality resources to the exploitation of resources of progressively lower qualities. Techniques such as beneficiation have made possible the exploitation of low-grade taconite ores.

Have these problems provided phenomenon upon which disciplines have built and generalized? They deal with the detailed characteristics of a material or process under very specific circumstances. One answer is that the scientific laboratory manufactures exemplars of natural phenomenon and materials to fit scientific theories. The development of the transistor, for example, awaited the availability of high-purity germanium and, later, silicon to fit theoretical models.

Another answer is provided by the histories of fields such as the geosciences and botany. Geosciences, a field that only a few decades ago seemed intellectually mature and comparatively uninteresting, was virtually reborn in the exploration for and harvesting of oil, natural gas, coal, and uranium. Ecological concerns in similar ways revolutionalized botany.

Some argue, however, that recent basic research has grown too abstract from human experience to relate to technological phenomenon: astrophysics, parts of atomic physics, parts of nuclear physics, condensed matter physics, mathematics, and some of molecular biology.14) The argument applied to particle physics says that it is dealing with energy transformations too high to apply to conceivable macroscopic environment, short of recreating the big bang. How can advance in particle physics result in a change in energy availability? Ernest Rutherford is reported to have said (five years before the demonstration of fission by Hahn and Strassman) that anyone who expects a source of power from the transformation of these atoms is talking moonshine. The real danger, history shows, however, is when scientific speculation loses contact with realities and ceases to have an experimental base. This is science gone astray.

Finally, let us compare Watt's utilization of the theory of latent heat in his invention of the separate condensing chamber for steam engines and Marconi's exploitation of developments in electromagnetism with A. Carothers' work which led to nylon and Schockley's work which led to the transistor.

One interpretation of the kind of comparison made here seems to be that expectation of financial returns motivated individuals to pursue a particular scientific problem in the latter cases. The underlying scientific knowledge was won in efforts specifically aimed at providing the basic understanding needed to achieve further technological advances. Carothers' basic research at Du Pont which led to nylon was financed by management in the hope that improvements in the understanding of long polymers would lead to new and improved chemical products. Shockley's Bell Laboratories project was undertaken in the belief that improved knowledge of semiconductors would lead to better electrical devices.

But breakthrough scientific research with the explicit objective of practical payoff is far from a recent phenomenon. When sponsored by organizations such as Du Pont or General Electric, the distinguishing aspect of basic research is emphasis on certain disciplines where advance is judged likely to yield solutions to practical problems. But many famous scientists did at least part of their work with exactly this in mind; Pasteur's work and the germ theory is one illustration of a long tradition of basic research aimed at facilitating practical advances in the field of medicine. There are several examples in the U.S. and in Germany of nineteenth century companies putting chemists to work on various problems. Generally, however, the large private companies with the basic research laboratories are latecomers.

In the earlier cases, the scientific research that created the breakthrough seems "autonomous" to the inventive effort. There was lag between the scientific discovery and the technological innovation. Attention then focuses on how to decrease flaws in communication networks and imperfect information flows, and on characteristics of potential adoptors and on resistance to change. But studies of this kind generally beg the question of the time required for scientific discovery to take place. Paradigms require intervals to develop. It is very important that we cease talking about either individual pieces of research or the state of science as a whole, and begin thinking in terms of scientific specialties. A central problem is to trace out carefully the manner in which differences in the stages of development of individual sciences can influence technological advance.

It is unlikely that any amount of money devoted to research in 1800 could have produced antibiotics, any more than a satellite capable of orbiting the moon. Scientific progress is, at times, completely inelastic – zero output at all levels of input. On the other hand, the purely market-oriented approach virtually assumes the problem away. The interesting situations lie in the vast intermediate region.

6. The Case of Transistors

Why should we look at the development of the transistor . . . again? The history of the transistor explodes or throws clear doubt on several assumptions widely held about the relevance of basic research to technology. These include: Technology develops from innovations in instrumentation to the almost total exclusion of theoretical advance, and mostly from mature rather than cutting edge science. The application process of science to technology involves transfer or communication of knowledge. The technological value of problems drives the application of science to technology. That is, technology rarely develops from science guided by the intellectual opportunities in a field.

Further, we claim the science driven model of technology development, as of the transistor, is more frequent than commonly supposed, and leads to technologies with higher-values. Finally, development of the transistor clearly demonstrates the variables – uncertainty, scientific value, technological value – that theoretically determine the way research decisions should be made, research effort should be organized, and research policy should be determined.

The transistor replacement for vacuum tubes came at least partially from a long history of experimental work with solid-state materials.15) As far back as 1874, a German physicist reported a peculiar flow of electrical current in certain kinds of minerals, which became known as semiconductors, because they conducted current better than insulators, but not as well as conductors.

Despite their unusual electrical properties, semiconductors remained for the most part a little understood phenomenon for scientific investigation. One exception came during the early days of radio, when the semiconductor material, carborundum was used to convert the oscillating electron signal that came in through an antenna into a useable one-way flow of current. When vacuum tubes were developed, however, they proved more effective at this conversion, and tubes could also amplify current. With these technological advantages, most of the practical work with solids was discontinued.

But scientists remained interested in the unusual properties of semiconductors. Conceptual advances in quantum mechanics during the twenties gave first real understanding of how electrons in metals were free to move and conduct electricity. In 1931, physicist H.S. Wilson published a theory of how electrons and holes in semiconductors gave these materials their electrical qualities. One historian writes that it is interesting to one who is not a physical scientist that by the mid-1930s, Wilson's article contained the essential ingredients for a rather complete understanding of semiconductors. But almost all scientists missed some of the essential points. Similar instances, he concludes, are legion.

With the advent of World War II, war work interrupted study of the phenomenon, and although solid state silicon detectors were employed in radar devices, they could rectify only high-frequency signals, and vacuum tubes were still required for amplification.

In January of 1946, semiconductor research was resumed at Bell Laboratories. It was felt, Nelson says, that advances in the understanding of semiconductors, including a better grasp of the quantum mechanical model, had set the stage for major breakthroughs and that the techniques of making crystals to close specifications promised materials which could be produced to fit the theoretical model. That is, the research project dealt with both the uncertainty of translating phenomenon into a paradigm's non-theoretical vocabulary and that of finding a theory to capture the translated descriptions.

In December 1947, Shockley observed "some effects" resulting from an experiment with a device that contained gold contacts and a germanium semiconductor base. What Schockley demonstrated a few days later was "the transistor effect": the movement of electrons in a semiconductor material could be controlled through the influence of an outside electrical field. Although primitive by today's standards, this demonstrated the transistor effect clearly enough to generate investment in further research to predict exactly how a given transistor would behave in an electronic device. Only then, were solid-state transistors able to perform most of the functions of vacuum tubes.

This is informal story-telling; but it seems to follow enough of a "logic" to suggest how to "simulate" in some detail how the possibilities of scientific advance (theoretical and experimental innovations) led to the eventual dominance of transistors over vacuum tubes.

The development of new products and processes is not a random process on the entire set of scientific possibilities. Scientific paradigms in different stages of development can focus technologies in defined directions. Within technologies, the effect of scientific advance might differ radically. A low possibility of further scientific advance and unsolved scientific problems may stimulate the search for a new technological path. Science does not simply enter up front, with technological development to be finalized by economic constraints or advantages. On the other side, the sense we have today of a greater number of science-driven technologies may be due to the adoption of scientific paradigms in early stages of development. These offer high uncertainty, but great scientific promise.

7. Rate of Scientific and Technological Change

But is the amount of scientific content in today's technology increasing? Are more knowledge-intensive products or the processes through which they are produced becoming the order of the day?

The Five Year Report concludes: "The implication is that we are now in an era of quickening scientific and technological change and that the two intertwine ever more closely." On the one side, as competition in advanced technology markets became more intense, the importance of basic science to technology multiplied. On the other, the lag between basic research and technological application appears much shorter now than the 30 years' gap found by a 1960s study. In addition, the role of basic research is increasingly to germinate (molecular beam epitaxy from surface science), and not only to improve (the creation of polymetallic cluster catalysts out of surface studies). Overall, more advanced technologies are being created by research faster in today's world.

There is evidence that more attention to the "quality" of goods has played an increasingly important role in international competition, and U.S. declines are not solely due to defective labor/management and marketing strategies. This situation manifested itself vividly in the case of automobile imports to the U.S. Technical change which requires "learning" appears more important as a result

Production efficiency is important for commodities, such as electrical power, sugar, or cement, where products are of a pure and uniform nature. But learning generally prevails with complex diversified roles for products and where functional specialization and/or psychological and taste components have important bearing on the product's value to the purchaser (e.g., machinery and electronic equipment, and consumer goods). Instead of more efficient quantity improvements, we increasingly see requirements for sophisticated and diversified techniques and design improvements, criteria of convenience, and consumer appeal.16)

Further, this pertains also to producer goods. The pressure for diversified, user-oriented consumer goods forces more complex situations with much more exacting specifications on equipment. It becomes more critical whether the machine will perform the task required reliably and precisely than how much the machine costs. A machine with a purchase price of $50,000 that fits smoothly into the production line is to be preferred to a $5,000 bottleneck.

One factor contributing to increased complexity is that manufacturing has become more process-oriented than assembly-oriented. Process manufacturing is easier to automate and is more productive, but also requires more involvement with the fundamental properties of materials. For example, in microelectronics, as the manufacturing of microcircuits is pushed down to tiny dimensions, puzzling phenomena occur which require new scientific explanations to advance progress.17)

Second, "learning" is the strategy for averting competitive pressures. For example, as the market for computer chips becomes increasingly flooded, the advantage turns toward the firm that develops attractive user-oriented software. Here "learning" can permit the maintenance of favorable prices even with a flood market.

Also, the development cycle for technologies appears increasingly science-intensive. Some of the controlled nuclear fusion plans laid out in the mid-to-late 1970s afford a good example: testing of magnets in the final developmental reactor was specified to the month, fifteen years in the future, when in fact the required fundamental physics and material science and the engineering limits remained obscure.

Are there future technological trends with increased science-content? According to Davidson, it is time to "go macro" to rebuild the U.S. economy. Davidson means civil-enginreering projects as large as any this planet has heretofore seen. He describes "planetran", a supersonic subway that could cut travel time between the

East and West Coasts to 20 minutes. He speaks of constructing cross-continental tunnels with tolerances that would certainly test the state of the art.[18)]

There are also future military/defense trends to take into account. The overwhelming part of current defense R&D increases are for development, conventional weapons and equipment, for example. But there is also the much discussed and controversial "starwars" proposal. One scientist described the challenges of the multitiered ballistic missile defense (BMD) system as equal to or greater than those posed by the Manhattan Project in each of nine different research areas. It seems certain that if the effort is launched, it will significantly change the mix of development versus basic research supported by the Department of Defense.

On the other side of the equation, let us briefly examine whether the rate of science into technology is increasing? Derek Price has stressed that applied science tends to be older science, the science that excited the basic scientist of a previous generation. The sense we have today of time collapse may be due to the generation of technology applications by scientific paradigms in earlier stages of development. Today's technology may increasingly by "high" technology, at the forefront of a scientific paradigm. Even at their inception, some paradigms, we have argued, are driven by problems that are of practical origin.

The by now classic example of time collapse is in genetic engineering. The fundamental experiments leading to genetic engineering were performed between 1971 and 1973. The first insulin gene was cloned in 1977, and the first genetically engineered insulin, was approved by the FDA for sale in late 1982. Less than nine years elapsed. This is compared with the thirty-two year lapse between discovery and its application, for example, in the case of the pacemaker.

One commentator on the biological sciences, however, writes that the basic biological sciences are so strongly connected with medicine that *a priori* one expects to find basic biological science to move into experimental medicine with great rapidity, so rapidly that the distinction between applied and basic is very blurred. For example, the work on suppression of immunological mechanisms by radiation and the limited use of this technique in organ transplantation came almost simultaneously.[19)]

The situation in biotechnology is also described in a recent comment by James Watson. He says: The elucidation of the double-helical structure of DNA in 1953 created no stir outside of the small band of scientists who were active in the field. We did not foresee any immediate practical consequences. Only the work of Boyer and Cohen in 1973 changed all this.

Even taking the time interval as approximately 30 years from discovery to a bioengineering industry, has biology generated applications more rapidly than other fields? To some, the answer appears obvious. What strikes one observer most about modern biology is how the new viewpoints unified the subject. The genetic code appears to be universal. Protein synthesis seems to be valid in almost every life form-in bacteria, in mice, in men. This unity suggests that biological mechanisms are likely to have a greater degree of application than large parts of modern physics, or astronomy, or mathematics.

But recent increasing capability to generate more technology applications appears broader than the biological sciences. The parameters of scientific paradigms vary greatly, and the variation has important consequences for the process of technological change. The relative highest trade-off of computers as opposed to manual computation is best measured in powers of 10. The relative ultimate highest trade-off of mechanized fruit picking as guaged against manual picking is probably not far from unity. One historian notes that if transport technology had progressed from stagecoach to the Concorde as rapidly as electronics technology has progressed since the transistor, the Concorde would carry a half million passengers at twenty million miles per hour at a cost of less than one cent per passenger.

A technological trajectory is sometimes represented by the movement of multi-dimensional trade-offs among relevant technological variables, such as the trade-offs between energy consumption and horsepower in internal combustion engines or between speed and density of the circuits in semiconductors. Progress becomes the improvement of these trade-offs, as in many technological forecasting models. But underlying scientific paradigms define this progress through the physical/chemical/biological laws exploited by the technology.

Some combination of the slope of the curve ("interesting" ideas versus time) and its height relative to that of the competing paradigm establishes the strength with which new science enters, gains scientific attention, and the extent of the changes affected by the introduction of the paradigm.

In addition to the biological revolution, some areas of physical science, we know, well-represented in the 1960s, dropped substantially in producing discoveries in the 1970s: applied mathematics, crystallography, and nuclear physics. Other areas, however, in the 1970s, increased their rate of discovery: high-energy physics, to a large extent, but also phase transitions, lasers and fibre optics, and photochemistry.20) These areas explain some of the increase in technical advance during this epoch.

Further, the current status accorded to high energy physics as the "most fundamental" area of science was earned during this time. For example, one physicist writes: The behaviour of matter and energy underlay all action in the world. In time astronomy, chemistry, geology, and even biology become extensions of physics.

In the 16th and 17th centuries astronomy was considered the most fundamental science, and maintained its high status throughout the 18th century. Chemistry and geology began to attract attention in the 19th century. Both became regarded as fundamental in the sense that they dealt with important problems and provided a firm basis for advances in other sciences.

Only in the 20th century, did physics achieve its high status. Soon after quantum mechanics had elucidated the electronic structure of the atom, the accelerator provided a new world of phenomenan. By the 1960s, elementary particle physics had become the most prestigious and lavishly supported area of science, and in many respects it retains that status today. While many may reflect on its theoretical nature, the paradigm apparently is very powerlful in terms of its power to connect other (more applied) sciences: nuclear, atomic or solid state physics, chemistry.

Some speculate about what might replace elementary particle physics as the most fundamental area of science. One candidate, Brush writes, would seem to be cosmology – the study of the development and structure of the universe as a whole – which has enjoyed a spectacular revival in the last two decades. Others are biology and psychology. A third direction is that physicists begin to emphasize the unification of forces rather than the discovery of smaller particles. This would actually strengthen the argument for the fundamentality of high energy physics, since many more areas of science depend on these forces than depend on the properties of quarks.

There is the final question of the role of science-push technologies in explaining the much-discussed long and cyclical waves in the performance of economies. An essential step in a theory of long cycles is the demonstration of the mechanisms through which particular changes in technology exercise sizable changes in performance. The wide impact of these technological innovations, Rosenberg and Frischtak write, needs to be understood in terms of the strength of both backward and forward linkages.21) We at least raise the possibility that these technological innovations can be strongly linked backward to the rate and direction of major scientific discovery.

8. Management, Organizational and Policy Issues

Let us grant: (1) there is a good deal more technology in science and science in technology than commonly thought. (2) Scientific as well as technological activity is directed toward problem-solving. Further, the criteria scientists use to select problems – importance and uncertainty – encourage technological application. The degree of application depends on the particular scientific paradigm, and the parameters of its development. At the least, technological problems initiate scientific advance, and play important roles at the cutting edge of science. (3) In sum, science is not silly-putty in the hands of technological problems, even highly-valued ones. Rather, to understand technological change, the rate of development of the underlying scientific paradigm is essential.

These processes of science and technology argue that for high-technology companies, the criterion of "scientific importance" may be better than one that stresses the value of specific predicted practical inventions. At the extreme, technological progress may only emerge, if governments and companies permit the best research to be fully supported.

But Jordan Baruch editorializes, in a recent *Science*, that policy-making efforts require that we examine the cultures of science and technology in addition to the processes.22) When scientists act as problem-solvers, he says, they adopt the technology culture and serve the technology constituency. The reverse is also true. Thus, he concludes, while the public may be the technologist's (as well as the scientist's) ultimate constituency, industry – and its rewards – set the technologist's culture.

We also read a good deal today about the conflicts between scientific ethos and industrial objectives and standards. A high ratio of industry to government R&D support is taken as a significant indicator of the ability of a country to achieve productivity advances.

But some U.S. industrial laboratories are noted for their steps to avoid establishing a decision and control system which would run against the traditions of the scientific community: freedom of research choice, decentralization and strong link with the academic world mark these systems. Industry scientists teach at universities, and are sought by university facilities. The quantity and quality of scientific publications is matched only by the best of universities.

Abernathy, in his survey of the factors contributing to U.S. competitive decline during the 1970s, writes that the use of various financial control devices often overwhelmed the personal contract found in innovative organizations like Hewlett-Packard, 3-M, and Bell Labs. In these innovative companies, the executive officers seem acquainted with all the company's technological projects if not with all the individuals involved. The president of a major one of these companies had a practice of participating in the design-review meetings of all the projects of the company.23)

But clearly it is in the interests of companies to stress the most promising areas of science. From the point of view of Bell Laboratories, Nelson writes, the magnetic properties of material were almost certain to prove more important in communications than advances in botany, and probably more important than advances in organic chemistry. "There were no geneticists working at the Laboratories. There were a few organic chemists." The conclusion he reaches, however, is that the choice of the research area of the scientists in the research department was seldom subject to strong executive pressure. Rather the hiring process was used to direct the allocation of research effort.24)

The firm's policies also recognized that in basic research, the number of persons among whom coordination is required is small at any one time, and the individual scientist is likely to know quite well what his colleagues are doing. Further, as to the promise of a line of research, the working scientist is likely to know much more than the laboratory management possibly can know.

It is also commonplace to note today that the institutional structures (both industrial and intellectual property rights such as patents and copyright) surrounding industrial development are the antithesis of those which advance science. The policies guiding science and its publication practices strongly conflict with those in technology. Technology is developed in secret. Publication is anathema. The drive for secrecy is so strong, Baruch writes, that early public policy created the U.S. patent system. Society went so far as to grant a monopoly to the technologist in exchange for revealing the technical knowledge embodied in the patent's disclosure.

But the transistor and other major technological advances of this century were too big to be kept secret. Increasingly, in addition, no one single firm is responsible for all the work on any single innovation. Trade secrets are carried by employee mobility, and despite sensational cases, such as the IBM-Hitachi litigation, most disputes over trade secrets are quiet affairs between corporations and their former employees.25) Further, the "strength" – the degree to which invention should be rewarded to encourage disclosure – of the patent system has been under continued discussion for the past century.

Some say that concentrated industry structure necessarily results from technology advance and inhibits the flow of information among firms conducive to new entry. Dosi's reasoning is that progress along a technological trajectory is likely to retain some cumulative features: the probability of future advance is therefore related to the position that a firm or a country already occupies vis a vis the existing technological frontier.26) Access to technical information by new firms (countries) is prohibited or at least too costly.

An interesting case refers to countries lagging behind in a certain technology. If technical advances are cumulative, and if oligopolistic structures can appropriate these technological leads, "imitative" technological policies are not sufficient to produce convergence between countries starting from different technological levels. Government intervention aimed at "catching up" must affect technology and capital flows, and the structure of industry.

But, for example, in the semiconductor industry, Levin writes, even if the technology race had produced a smaller number of survivors and a more concentrated industrial structure, it is not obvious that the rate of entry would slacken. Severe constraints on appropriability characterize the semiconductor industry. Important aspects of proprietary technology, such as circuit design, are not patentable under U.S. law. Even where patents are available, they offer little protection because cross-infringement is so widespread as to render most patents unenforceable in practice. Reverse engineering has been relatively simple, and interflow employee mobility is legendary. Finally, a few months of lead time with a new product have been sufficient to insure adequate reward to innovative activity.27)

What is the case though, Lewin continues, is that R&D costs at or near the frontier of semiconductor technology have escalated substantially. The R&D experience of the large established firms, the complexity of the technology, and the cost of assembling the required research personnel and equipment are the formidable barriers to entry. It is therefore likely, he concludes, that new entrants will play a rather different role, for example, custom design and fabrication in the advance of semiconductor technology.

The essential problem, on the other hand, with interventionist policies is that they may make entry more difficult: diminish the information exchange, the cooperation in research, and the agreement between old and new firms essential to covering the costs of research. Anti-trust laws in the U.S. currently appear to be giving way to the necessity of joint-ventures between firms to undertake costly research. Overall, to make public policy regarding science and technology on the basis of their differences, as some suggest, is to deny growing understanding of these processes. It is also to deny a good deal of evidence that "successful" policies in terms of technical advance and change have taken into account the realities of science-driven technology.

9. Emerging Technologies

What assistance can the theory of scientific and technology processes, presented in this paper, provide to science and technology management and policy? One of the most vital is to contribute to accurate leading indicators of technology, ones which allow sufficient time for corrections to be made by the private or public sectors. We suggest that patterns of scientific research may serve as technological lead indicators.

Agricultural bioengineering provides an example. For one, several dozen Forture 500 companies are now investing hundreds of billions of dollars in research in this area. We also read about the commanding lead of the United States over its industrial competitors in biotechnology, as concluded by a recent study by the Office of Technology Assessment (OTA). But the report is less conclusive about competitive advantage in the future: will it depend on developments in bioprocess engineering or on innovations in genetics, immunology, and other areas of basic science. If the former, what is needed is more federal funds for genetic applied research, together with money for training grants.28)

Further, difficulties in university spinoffs in biotechnology are increasingly contrasted with the untroubled ones in microelectronics and computer technology. One explanation is that biotechnology start-ups are still largely in the research stage and are making their money on research contracts, not on the specific product ideas that advanced so many of the successful microelectronics ventures.

Let a methodologist dream about a series of case studies of scientific research to produce early technology indicators. These would focus on the variables – uncertainty and importance – that theoretically determine the way scientific problems should be chosen. The purpose of these case studies would be to determine the values, or frequency distribution of values, these variables take on, in the science associated with a technological problem. The values of the variables would be plugged into the theory and the correct problems and policies deduced. Third, an organization, or a number of organizations, would be studied to see to what extent their policies corresponded to those which the theory indicated were sound.

For example, a primary problem in agriculture is to develop plants with increased resistance to disease and drought, which are also acceptable to consumers in terms of costs and taste. These objectives can be approached currently by a number of "emerging" and traditional technologies, cross-breeding, and the biotechnologies of gene splicing, and protoplast fusion. Each of these technologies would solve the problem to some extent, i.e. output per gallon of water, or remaining plants per year, etc.

For each technology, there is associated an area of scientific research which revolves around a central set of scientific problems or questions, accompanied by assignments of importance and probabilities. Let us suppose that we can characterize these assignments. Expert analysis (peer-review) of the scientific problem domain is one approach.

We know, for example, that support of plant biology over the last few years of the NSF budget shows dramatic changes. The most relevant area to genetic engineering of plants is genetic manipulation. Thus studies on vectors (the vehicles that carry desirable genes into plants) and gene-splicing (the splicing of DNA sequences in genes) are included. Introduction of DNA through protoplast fusion is also a new thrust. In 1977, only two percent of the twenty million dollar budget for plant biology went to genetic manipulation studies. By 1981, the slice had increased to thirteen percent of NSFs thirty-three million dollar expenditure for plant research.

The literature of the domain and its citation and cross-citation patterns offers a second approach. Available bibliometric analysis shows biotechnology research is not one tightly ordered discipline, but a number of disciplines being brought into contact for the first time. The European Federation of Biotechnology sees it as the common overlapping zone of three interacting disciplines: biochemistry, microbiology, and process technology. On a much more disaggregated level, analysis of cross citation patterns of the plant biotechnology literature shows distinct differences in the growth, age and national origin of different approaches: cross-breeding, DNA-sequence analysis, and molecular aspects of protein processing in plant seeds.29)

Capturing the actual thinking and work of researchers through computer-simulation is a third possibility. The Bradshaw, Langley and Simon work illustrates alternative paths to achieve explanatory scientific laws. The simulation approach offers an opportunity to capture the routes that scientific discovery can take, and the kinds of mechanisms that can be used to traverse these routes.

We have currently no measure of absolute or relative technological progress available to us; there is widespread agreement about this. Technology is a complex domain with interlocking problems and competing sets of solutions. For all of their practical and explanatory importance, neither scientific growth nor technological change are definitely understood. But it is apparent that technology is even further from being clearly understood. With some exception, the technology literature ranges between anecdotal examples of how various inventions and innovations were made and formal economic studies that are steady-state analyses of a subject that is usually far from being at steady state.30)

We have tried to put to rest in this paper "black box" input and output measures for the growth of science and its productivity. We have also argued that science and technology are more than only loosely coupled bodies of knowledge. Both of these assertions imply that measures of scientific advance are likely to be increasingly adequate for questions regarding the status of technologies.

10. Some Conclusions

Pasteur observed that "in the fields of observation, chance favors the mind that is prepared." Perhaps he would have agreed to add that only the mind that is prepared takes chances. The requisite for technological advance is neither the extreme of accidental discovery (as science is often portrayed) nor mission-oriented programs. The improved understanding of science and technology we have attempted shows that the ability to estimate uncertainty is an essential ingredient. We have also argued that scientific and technological values can be estimated and that today they seem closer to convergence. This has implications for priority-setting at the National Science Foundation and for employment policies at major U.S. corporations.

Can we be less sure in today's technological environment that Einstein would have been unhappy at Bell Laboratories, and Bell Laboratories would have been unhappy if their scientists were as "intellectually pure" as Einstein? We have no firm answer. When Gulliver visited the mythical science-oriented society of Laputa, he found scientists to be of no use. They were involved in only useless (and even counterproductive) activities: softening marble for pillows or inventing ways to prevent the growth of wool on lambs in order to propagate naked sheep. Swift might have been going along with today's tendency to measure scientific worth in terms of direct utility. This paper hopefully has reversed the tide by putting on firmer ground the theory underlying the relevance of basic research to technology.

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INCREASED RELIANCE OF MODERN INDUSTRIAL TECHNOLOGY ON SCIENTIFIC OUTCOMES

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Industrial technology is based on both traditional experience and scientific knowledge. However, modern industrial technology, especially process technology for materials and semiconductor devices, depends heavily on scientific outcomes. As today's society moves toward the information society, public demands have increased the popularization of technologies whose difficulty is an order of magnitude higher than those in the industrial age. The degree of industrial success depends largely on the degree to which scientific outcomes, both from the natural sciences and other areas, are applied to industrial technology.

1. Linkage Between Industrial Technology and Scientific Outcomes

Since the time of the industrial revolution, the degree of close linkage between industrial technology and scientific outcomes has progressively increased, and the time span from a major discovery of new scientific knowledge or phenomena to a major technological innovation has considerably shortened. Recent studies by many scholars on major technological innovations indicate that the trend demonstrates a systematic relation. Most engineers recognize that major discoveries and innovations in their professional fields are concentrated within particular periods; however, they also think that such concentrations are coincidental and that events are quite random in appearance. In fact inventions and innovations are particularly heavily influenced by social environments and coincide quite well with logistic curves. Scientific discoveries and major inventions create environments for increased research and development relating to basic technologies to apply scientific outcomes to meet public demands. These activities generate new industrial possibilities and eventually contribute to economical growth. These relations are graphically summarized in Fig. 1. The top curve indicates a simplified perturbation of the world economy. The second group of curves indicates a bunched phenomenon of major technological innovations in major industrial areas, while the third group of curves is a similar one for major discoveries and inventions. The accumulated inventions and innovations in a specific period follow a logistic curve. The quantitative constants for invention and innovation curves determined from the logistic curve are tabulated in Table 1.[1] The time spans between each innovation center and invention center

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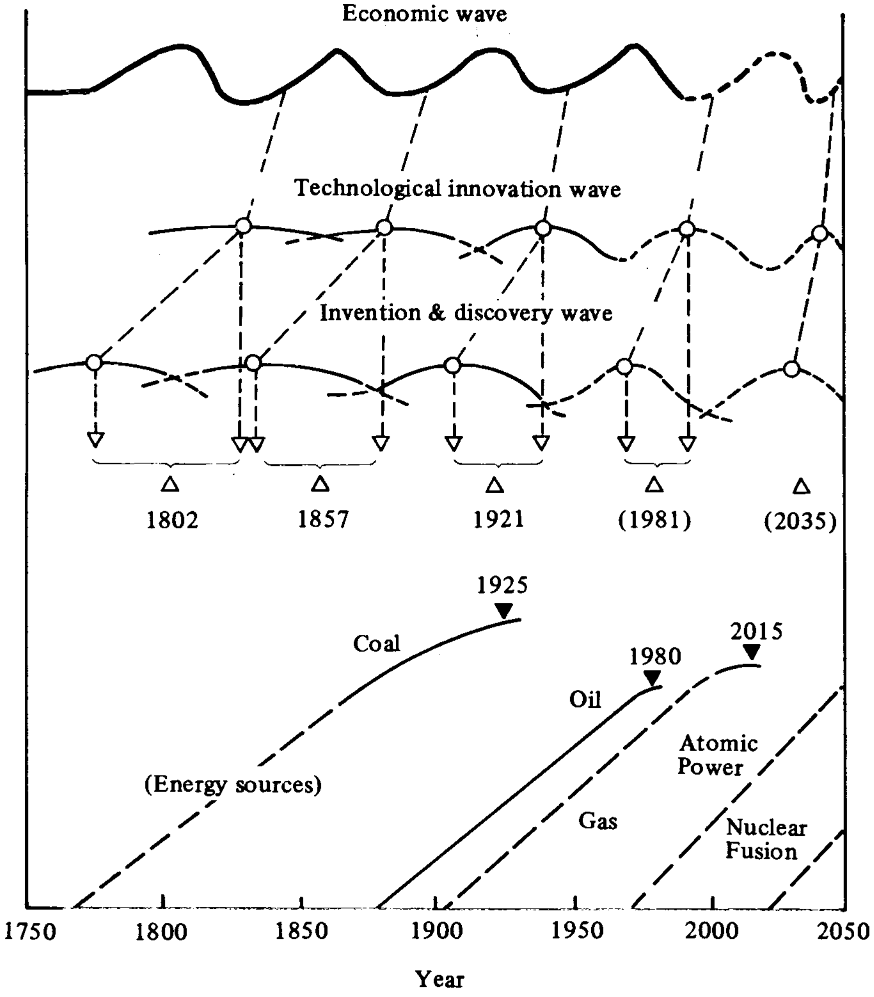

Fig. 1

Table 1. Relationship between invention cycles and innovation cycles.

	Cycle 1		Cycle 2		Cycle 3		Cycle 4
Innovation Center Point	1828		1880		1937		(1992)
Invention Center Point	1775		1833		1904		(1969)
Midpoint of the Cycle	1802		1857		1921		(1981)
Δ T between Invention and Innovation Centers	53y		47y		33y		(23y)
Innovation Time Constant	47y		33y		23y		(16y)
Invention Time Constant	120y		85y		55y		(38y)
Δ T between Innovation Centers		52y		57y		(55y)	
Δ T between Invention Centers		58y		71y		(65y)	
Δ T between Midpoints		55y		64y		(60y)	

remain around 50 to 70 years, but the time spans between innovation and invention centers and the time constants have decreased by $1\sqrt{2}$ every time the cycle is renewed.

Another observation on the linkage between research and application usually suggests, as shown in Table $2^{2)}$, that the highest linkage is needed in the architectural phase (quadrant I); a high linkage in the revolutionary phase (quadrant II); and less linkage in the regular and niche creation phases. In the architectural phase of a major innovation, which corresponds roughly to the phase from the invention center point to the midpoint between the invention and innovation cycles in Table 1, the linkage between basic research and the applied research is very high, but the scientific area in the linkage is usually limited. As a major innovation advances in the revolutional phase, which corresponds roughly to the phase from the midpoint to the innovation center in Table 1, development becomes a major part of the activity, and the scientific area in the linkage is considerably expanded. The expansion is especially conspicuous for innovations that require extremely complex processes, such as microelectronics and optoelectronics.

Until about the 1960s, a single invention alone could creat significant business, and most products could be assembled with the supplies manufactured by specialized industries without close coupling with other industries. However, the trend of innovations has rapidly deviated from the conventional trend as society has moved rapidly into the information society; the impact of a single invention is no longer as significant as it used to be, and innovations based on the integration of numerous inventions and technologies are gradually dominating current industrial activities. Hence, modern industrial technology, not only product technology but also process or production technology, is an integration of numerous scientific outcomes from various scientific disciplines. The nature of modern industrial technology thus demands a wide distribution of capable scientists and engineers, from basic research to production engineering. This fact is often overlooked in many industries, resulting in the loss of competitive power in spite of their excellent R&D activities in the frontier phases.

Many European countries still retain excellent basic research and creative powers, but many industries have not been able to fully utilize such outcomes for industrial technologies. Hence, they are losing the competitive power in Phases II and III in Table 2. The U.S.A. gained competitive power in Phase I during the

Table 2. Linkage differences by evolution of technology

Low	High
IV Niche Creation Phase (market innovation)	I Architectural Phase (major innovation)
III Regular Phase (production innovation)	II Revolutional Phase (incremental innovation)

middle of this century; this was in addition to its traditional power in the other Phases noted in Table 2. However, there is some indication that the U.S. is losing competitive power in Phase III, especially for popular markets that make up the foundation of a social economy.

Japanese industrial technology has been widely rated as an imitation of European and American technology and as having some edge only in Phase III. It has also been said that Japanese contributions in terms of technological innovations are mostly in process innovations with almost none in product innovation. Japan has adapted science and technology energetically from the West since the Meiji restoration, in order to improve its living standards. The adaptation is not a mere imitation or copy. Instead, it is the absorption of basic knowledge and technology for useful applications, where scientific process is inevitable. There is also a learning process before the creative power can be developed. Therefore, Japan has historically adopted the policy of promoting the learning of anything useful for improvement of social life without regard to the national origins of their development. Any learned technologies are well digested, based on scientific research outcomes, and further refined by pursuing mission-oriented basic research efforts. In the past, most research activities in industrial, national and academic laboratories were mission-oriented ones in this respect. Hence, even though the centralized coordination was poor for university-industry cooperation, most scientific outcomes have been effectively coupled to industrial applications.

The current success of Japanese industry with higher productivity and product quality is usually attributed to the high quality of workers, QC activities, utilization of robotics and so on. However, an area generally overlooked is the close cooperation between scientists and production engineers to fully utilize scientific outcomes to improve productivity and product quality. The production automation of modern products, such as high quality steel, semiconductor devices, fine-ceramic devices, video tapes, etc., demands detailed scientific knowledge of materials and device physics and chemistry far exceeding that necessary to invent and simply manufacture new products. Without these detailed fields of knowledge, automated mass-production produces defective products. Most researchers often ignore these important scientific problems, either because of their ignorance resulting from poor communications with production engineers or because of looking down upon such problems as being practical low level science. Fortunately, Japanese industry has had fewer problems in this respect, because of its being in the catching up phase, and has managed to distribute capable scientists and engineers at every front from research to production, thus resulting in the effective application of scientific outcomes to every phase of industrial technology development.

In the next sections of this paper, two cases of application of scientific outcomes to industrial technologies are presented. The first case is the development of process technologies for VLSI and three dimensional ICs. The second case is the development of pattern recognition and voice synthesis technology and machine translation technology for reducing communication barriers for handicapped persons and the public in ideographic oriented cultures.

2. Process Technology for VLSI and 3D IC.

The invention of the transistor has created a major new industry, the semiconductor industry, and revitalized other industries. Semiconductor technology has advanced very rapidly, owing to numerous scientific outcomes and strong market demands. For the past thirty five years, the semiconductor industry has experienced four major innovation phases for every ten years: the first phase is the transistor age or discrete device age; the second phase, the IC age; the third phase, the LSI age; the fourth phase, the VLSI age. These innovations have been quite revolutionary from a social impact point of view. However, from a scientific point of view, beyond the discovery of the transistor phenomenon, the following innovations are simply evolutional. It can be said that the following innovations have been made possible by improvement in circuit design technology and especially in process technology. However, these technologies were also heavily backed by numerous basic scientific research efforts.

A characteristic of modern semiconductor device technology is that it is a comprehensive technology, ranging from very basic material physics and chemistry to device and circuit technologies and further to systems and software technologies, as shown in Fig. 2. It is no longer able to be handled by a single genius nor by a single division. It requires broad cross-organizational cooperation to develop new VLSI products. However great the genius and however innovative individual technologies might be, if they are isolated and cannot be implemented systematically with other necessary technologies, they are merely isolated inventions and cannot be regarded as VLSI technology. Therefore, VLSI technology development and production technology development are inseparable. In order to improve product quality and reproducibility, very basic understandings of material physics and process chemistry have to be advanced.

As shown in Fig. 2, the transistor is a device that fully depends on the material

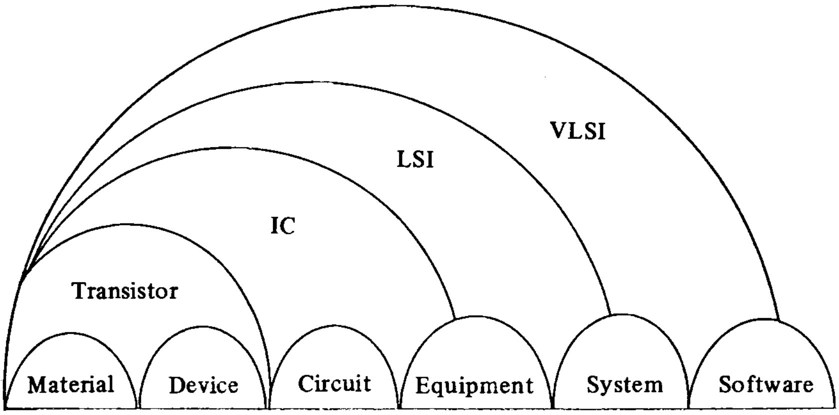

Fig. 2

characteristics of a semiconductor. This is distinctively different from traditional devices and machineries, in which materials are also essential parts, but play only passive functions. Hence, engineers had a relatively broad selection of materials to choose from, purchased parts as finished items and assembled them into whatever devices they were working on. However, transistor device engineers had to engineer semiconductor and associated materials. Material and device are no longer separate entities; they are completely integrated and inseparable. This trend has increased dramatically while moving upward in complexity toward VLSI. Up to the IC age, a capable engineer or a group of a few specialists could develop almost all technologies, down to the final production process. However, from the LSI age and beyond, it has become very difficult, if not entirely impossible, to develop new products without closer cooperation between numerous scientists and engineers of different disciplines.

In the VLSI age, in one step in design rule advancement, 256 k RAM (256,000 bit random access memory) to 1 M RAM (1,000,000 bit) for example, almost all process technologies and materials should be reexamined and most technologies refined to assure product quality and reproducibility. This is why we say that the quality is built into the product from the research stage. During these processes, all possible scientific problems are studied with the cooperation of scientists and engineers in companies, cooperating industries and universities. Many of these studies are very basic. Many scientists often say that this basic research is an incremental exploration of knowledge. However, the impact of such knowledge is so large that invisible penetrations of such knowledge and technologies into other industries revolutionize their process technologies and product quality. Such knowledge is often revolutional and opens completely new fields of scientific study.

Evaluations of Japanese VLSI research projects abroad are quite mixed. Some rate high, because furiously competing companies cooperated for a specific target in VLSI product development. Some rate low, because only limited numbers of new technologies were created. However, both observations are not equally quite correct. The project specified specific VLSI product goals. However, this specification was to define possible technological barriers and to select proper research subjects. The developments of final products and closely related technologies were left completely to the responsibility of individual companies. The cooperative effort was to develop the common basic technologies and to explore the necessary scientific knowledge needed for such developments. Even in Japan, cooperation among competing companies is almost impossible as technological development moves closer to product levels. This is not only due to business competition, but also due to differences in fine details of process technology, which has been accumulated for many years and integrated into major automation systems.

Basic scientific knowledge and technology are commonly applicable to any company or industry. Their impacts on products depend largely on the receptivity of scientists and engineers in industry. This is especially so in an industry based on complex process technology, such as the VLSI industry. Major scientific discovery and invention are even more important for technological innovation. However, as the technology advances and social needs become ever more complex, the majority of technological innovations are based on the integration of many discoveries and inventions, together with evolutional technologies of a variety of professions. Even new industries have to be largely supported by traditional technologies and industries, revitalized by new scientific knowledge and technologies. Therefore, communicating among scientistis and engineers in different organizations and professions is becoming increasingly important. This communication is the most important basis for cooperation and, hence, for linkages between scientific outcomes and industrial applications.

Many wish for closer cooperation between universities and industry and direct applications of university research outcomes to industrial products. However, the probability of such direct applications is becoming increasingly small in an industry based on complex process technologies. Even when the university outcome is extremely attractive, most designs and process technologies must be completely reexamined and modified to be integrated into the industrial technologies. This process is extremely time consuming and a waste of resources on both sides. Hence, effectively managed industrial research laboratories are primarily oriented towards applied sciences and basic technologies for possible innovations of new products and processes. When a possible new product is developed, the laboratory and the production division jointly organize a development project from the beginning. Therefore, the feedback cycle of posing problems and solutions is increasingly shortened. This kind of project management between universities and industry is extremely difficult and often even undesirable. Hence, industry should expect scientific knowledge, ideas and basic technologies from universities and university-industry cooperation should be in the basic research areas.

As a candidate for post-VLSI fields, the development of three-dimensional IC (3D IC) is under progress as a part of Japanese national projects. The 3D IC concept is simple and natural. In order to extend the integration scale limit for a VLSI, which is a two dimensional IC, the idea of 3D IC is to add second and third VLSI layers on top of 2D IC by continuous processes. The idea is simple, but the technology needed to achieve this goal is extremely difficult. Mountains of new process technologies and materials must be invented. Individual inventions may not be revolutional from a scientific point of view; however, such inventions are essential in developing the 3D IC. The ultimate goal of 3D IC technology is to develop intelligent sensors and processors that simulate human senses. Here, extended basic knowledge and numerous technologies are needed. Without new scientific outcomes, even a simple 3D IC is difficult to develop and the intelligent sensor is a mere dream.

3. Intelligent Processing of Speech

To meet advanced demands from the public in the information society, there must be progress in machine functions in terms of incorporating human intelligence into the machine, so that it ever more closely approaches human functions. In this context, the most important aspect of modern technology becomes the man-machine interface.

Machines that read characters started out by reading printed letters and now can decipher handwritten script. A machine that recognizes characters in books, processes them as sentences, and reads by synthesized voice is now under development for the blind. Machines that can recognize human speech are already on the market. These devices are able to recognize single syllables or individual words, and we will soon see devices that can recognize continuous speech by anyone, not just the voice prints for a given individual.

Vision, hearing and speaking are the natural means the human uses to communicate. They are so natural that the public demands similar functions of machines, in order to make the machine more friendly to humans. However, it is extremely difficult to simulate such human functions through technology. Such demands are especially high in Japan, because of the ideographic written language. The keyboard, which is a common means to communicate with machines at present, is very unfriendly to written Japanese. These are several reasons why Japanese industry has been working on the development of pattern recognition technologies, such as character and voice recognition and voice synthesis. Unfortunately, only a few universities have begun basic research on voice recognition in Japan. However, the industry has maintained close communication with scientists in engineering and medical schools. The scientific outcomes have been fully applied to advance the voice recognition machine.

Although man-machine communications technology is very difficult, an even greater difficulty is associated with how to break the language barrier by technological means. Today, humanity is working steadily towards world peace. Even so, unfortunate incidents are still happening between countries. A large portion of these are caused by conflicts of opinion or misunderstandings brought about by cultural and linguistic differences. Global cultural and linguistic uniformity may look desirable, but such uniformity would be unrealistic. Any policies that will destroy regional cultures that have taken thousands of years to evolve should not be adopted, either through technological immaturity or out of short-term economic interests. Some technological means should be developed, by which the people of the world can better understand one another and progress towards peace while leaving cultures and languages to the process of natural change.

As a step toward this technological goal, speech recognition and synthesis technology and machine translation technology have been under development for many years. Machine translation of technical sentences is a realizable target, since the technology is internationally common and the sentences are more logical and have less ambiguity. However, other sentences, especially arbitrary conversations, are extremely difficult to translate with current scientific knowledge. Nevertheless, the development of automatic translation and interpretation machines is a vivid dream throughout the world. As a first step, if all the people in the world could simultaneously hear and see what is said by a speaker at the other end of an international call and have it interpreted by machine translation into their own mother tongue (Fig. 3), people could communicate more readily with foreigners, hence immensely increasing the opportunities for greater understanding. It is desirable that further developments lead to simultaneous interpretation machines that can be carried in the pocket.

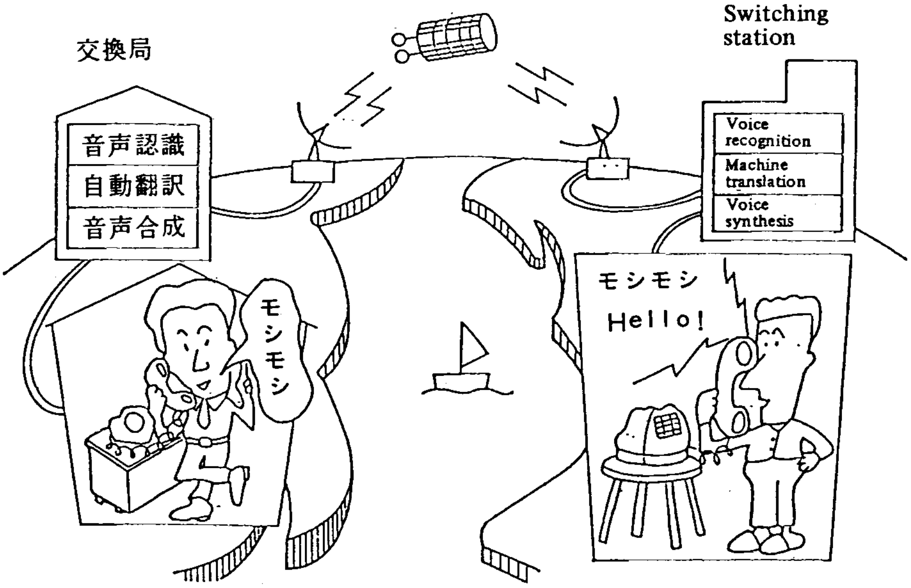

Fig. 3 Automatic interpreting telephone

For the achievement of such goals, scientific outcomes from not only the natural science areas but also from the social sciences and humanities from all over the world must be applied to the development of the necessary technologies. In Japan, communication and cooperation links among different disciplines have been weak. In order to increase the efficacy of such links, the Japan Society for the Promotion of Science has just organized a professional study committee for improving communication and finding common scientific problems to aid the advancement of intelligent processing of languages. The committee members consist of scientists from different disciplines, such as linguistics, psychology, computer science and microelectronics.

Technology in the information society must be linked much more closely to human and regional cultures. In this respect, science policy must change, based on such social trends.

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PUTTING SCIENCE TO WORK IN A MULTI-INDUSTRY CORPORATION

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Abstract: The selection of significant technological problems to solve, the conduct of imaginative and fundamentally sound scientific research programs to solve them, and the timely and efficient transfer of the results into industrial practice, represent challenges of both corporate and national significance. In this contribution, some lessons learned from attempts to meet these challenges in the central laboratory of a diversified U.S. company are described and discussed.

Introduction

Much has been written, and countless discussions held, on the problem of translating fundamental scientific knowledge into improved industrial productivity. Solving this problem is the perennial challenge of all industrial R&D managers, and solve it they must if their companies, and indeed their nations, are to remain economically competitive.

However, there is no one, "universal" solution: an approach proved effective given the resources of an AT&T or a Toyota may be quite inappropriate for a smaller company, and most industrial companies are considerably smaller than such giants.

Martin Marietta is a medium-sized, diversified Corporation with annual sales of about $4 billion (B). It ranked No. 108 in the Fortune 500 Directory of U.S. companies in 1983.1) In that year, about 30 U.S. companies had sales in excess of $10 B, and some 260 companies had sales of between $1 B and $10 B. Thus, in this context, Martin Marietta is a typical, substantial company . . . but not a giant. Martin Marietta Laboratories is an organization of about 250 people whose task it is to apply science and technology to corporate objectives across the whole spectrum of Company activities. Since these range from construction aggregates to computer systems, and from aerospace and defense systems, extractive and fabricative metallurgy and cement to chemicals, this is a non-trivial assignment! From our efforts to meet this challenge has come some insight into the factors that permit an industrial laboratory of modest size to be a useful contributor to the present and future well-being of its parent company.

Some of our findings will be presented here (see also2)). And to provide a modest data base for our views, four projects that involved the transfer to production of improved products or processes based on scientific studies

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* Martin Marietta Laboratories.

conducted at our Laboratories will be described. Of course, not all of our projects are as successful as those we shall discuss. This select group belongs to that category of situations in which a practical problem arises, or suddenly becomes important, no evident answer exists, and a scientific project addressing the fundamentals of the phenomenology is required to solve it. Once the solution is clear, technology transfer is relatively straightforward, because the plant staff are already waiting for the results . . . and usually wondering why you took so long. This is the "problem pull" situation, somewhat akin to "market pull" in product development.

On the other hand, research can also provide solutions for which no already identified technological problem exists. Given a working knowledge of the company's operations, however, it may be possible to identify an application that can lead to improved productivity. Technology transfer in this instance, i.e., the "science push" situation, usually is more difficult for a variety of well documented reasons, including the plant manager's natural reluctance to "improve" a process already working quite well.

A third category, in which both the problem and its solution already exist . . . but have yet to be brought together, may also be noted. In this situation the scientist serves as the midwife, rather than the parent of the solution, and while transfer is not necessarily difficult, it is, as in all cases, time consuming.

It should be commented that certain prerequisites must be met before any industrial R&D laboratory is likely to be effective. These include:

(i) stable funding and a supportive and patient management. Without these, the staff will not be able, or psychologically prepared, to develop the scientific information base needed to respond in a prompt and innovative manner when called upon. Because funding from operating companies tends to be unreliable, and focussed upon short term goals, this prerequisite may require that a substantial portion of the financial support for the laboratories be provided from corporate headquarters.

(ii) state-of-the-art equipment. Modern analytical equipment, especially that exploiting recent advances in automation, though expensive, is a *critical* investment, given that funds also are provided to support skilled operators and regular maintenance. In the hands of imaginative scientists, such equipment can increase R&D productivity substantially, helping provide rapid and practical solutions to otherwise unresolvable problems. Analytical equipment and techniques such as STEM, SIMS, Auger spectroscopy and NMR are becoming necessary and standard tools in competitive industrial labs these days.

(iii) a staff containing talented scientists. We have found that the better the scientist, i.e., the greater his intellectual capacity, curiosity, and problem solving capability, the more successfully and responsively will he (or she) rise to challenges outside his area of expertise . . . which is where most important industrial problems seem to occur. In our Laboratory, we have scientists and engineers whose skills range from geology and cement chemistry to applied mathematics, and include materials scientists, organic chemists, biologists, physicists, and experts in artificial intelligence. But, given the technologies encompassed by our corporation, this talent is spread too thinly in every important area. At first sight, this would appear to be an organizational weakness; yet it has turned out to be one of our most important strengths. The reason is that to attack any problem of significant magnitude we have to put together a team of people with diverse backgrounds. And it is remarkable how often, by cross fertilization, such heterogeneous groups come up with innovative and cost-effective solutions. The point is that the staff is used as much for its intellectual capabilities as for its specific technical expertise.

Unfortunately, the ability to identify solvable problems, to solve them, and to transfer the results to practice in a smooth and efficient manner is *not* a skill usually acquired at the same time as a Ph.D. In most cases it takes on-the-job experience, further education in economics, marketing and psychology, plus persuasiveness and persistence. In our organization it seems to take five or so years to turn a graduate scientist into an effective "industrial" scientist.

The four R&D projects we have chosen to illustrate how science can be put to work in industry relate to: (A) the development of an infrared signal detector for a missile guidance system; (B) the production of durable adhesively-bonded aircraft components; (C) the development of advances in stone blasting technology; and (D) improvements in the process used to make periclase (MgO) from brine and dolime. These "cases" will serve to introduce some of the other factors we believe are important to technology transfer in industry.

Case A: Development of an Infrared (IR) Detector

One of Martin Marietta's product lines is missiles for air-defense systems. In this case, the guidance system for a particular missile utilizes two single-element infrared detectors attached to the tail fins, Fig. 1 (b). These detectors, made from the semiconductor material mercury-cadmium-telluride (HgCdTe), serve as the receivers for a ground-based, infrared laser beam that carries the guidance information to the missile. Unfortunately, the vendor contracted to supply the detectors was unable to meet either a time deadline or operational specifications. The Labs' challenge was to design and build several novel detectors, transfer the basic fabrication technology to a production facility where several hundred detectors could be produced, and then work to improve each step in the process so that the necessary yield and performance could be achieved . . . against a two month system test deadline.

Fortunately, the basic knowledge that made accepting this challenge feasible had been developed within a long-term research task aimed at understanding the fundamental parameters that limit the sensitivity of HgCdTe photodiodes to IR radiation. Techniques had been developed for computer modeling the physical behavior of a semiconductor-surface oxide "system", and for measuring and controlling some of the bulk and surface properties that affect the systems capability for detecting IR signals. Surface properties had received particularly extensive study, because no coating for efficiently eliminating the stray surface electrical charges that cause variable performance had yet been developed for

HgCdTe.

Conceptually, the detectors required were quite simple, but the necessary high-performance could be achieved only through careful control of intrinsic and surface chemistry, as well as precise dimensional design of the device. This is where the computer model, incorporating much diligently acquired physical data and mechanistic understanding, provided a critical advantage. In a matter of days, we were able to input the desired operating parameters for the detector and its preferred geometry, and deduce specifically what the thickness and stoichiometry of the substrate HgCdTe should be, and how thick, and of what type, should be the coating. As it turned out, a simple anodic oxidation treatment produced a coating with performance sufficient to meet the specifications.

Using this information, eight operating detectors of the type shown in Fig. 1 (a) were designed, built and delivered within a few weeks. In practice, this required some of our physicists to work night and day through the Christmas vacation to meet the deadline. But it was an exciting and stimulating experience for them.

The fabrication technology was transferred as follows: Soon after the Labs' work commenced, our production operation sent an engineer to work with the Labs' team, to carefully document the processing steps as they were developed, and to act as the liaison person between the Laboratories and the factory line. This circumvented any "not invented here" (NIH) syndrome, since "their" man was involved in the Labs efforts. Meanwhile, the Labs' management were in active communication with the plant management, recommending the fixtures and tools, etc., needed to assume production. Team-work was of the essence, and the Laboratories, as possessor of knowledge critical to system success, was readily accepted as being on the team. None of the territorial problems sometimes associated with technology transfer arose, in part because the need to meet a difficult deadline was foremost in everyone's mind.

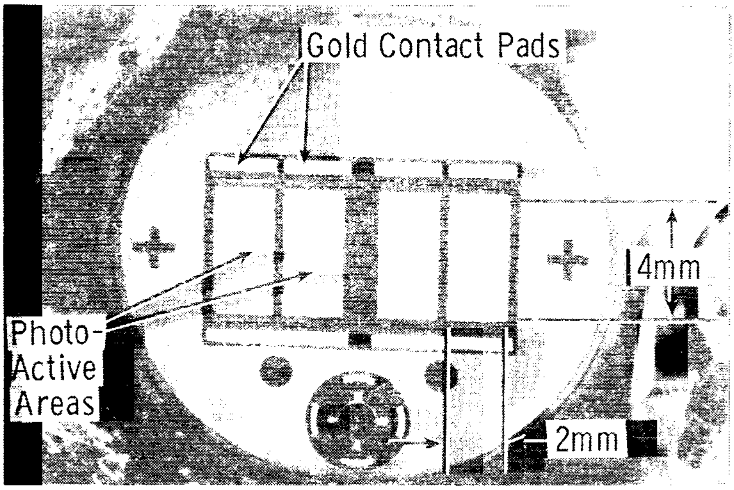
The project was successful; the deadline was met, and the detectors now work with a sensitivity twice that called for in the specification.

The lesson to be learned from this case is that it is important to *establish and maintain a solid science base* in areas underlying the basic technologies of your company, so that your staff can respond in timely fashion to both evident and unexpected technological problems. At Martin Marietta we have traditionally conducted long-term programs, sometimes funded by contracts won competitively from government organizations, in such fields as surface and solid state physics and chemistry, mechanical properties and fracture behavior, and applied mechanics. One of these "carrier-wave" programs proved to be the critical resource in this case.

The development of such a science base has other benefits too, not the least of which is that it attracts the bright, young scientists who are your future problem solvers.

Creating the environment to sustain and develop good industrial scientists also is very important. The need for stability has been mentioned, but equally

(a)



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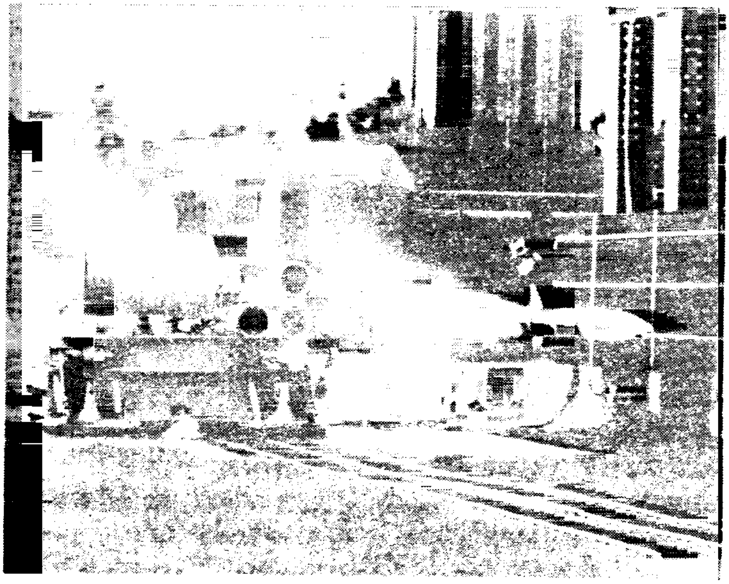

Fig. 1. (a) Mercury cadmium telluride detectors developed at Martin Marietta Laboratories for a laser beam-riding anti-tank and air defense missile. Each wafer included two detectors mounted on a sapphire substrate. (b) Test firing of the laser beam-riding missile. The IR detectors are attached to the tail fins and serve as receivers for a ground-based infrared laser beam.

important is the establishment of high standards of performance, high expectations for achievement, and a sense of excitement and urgency.

Communication with the scientific community at large must also be encouraged. This includes the presentation and publication of non-proprietary results, and responsible involvement in professional society activities. Such actions permit your staff to become members of those subtle networks within which information is transmitted quickly and quietly, usually a year or two ahead of it becoming common knowledge. Such a temporal advantage, usually gained only on a quid pro quo basis, can provide a useful competitive edge for the company.

Vigorous discussions should also be encouraged, or even mandated, between the Labs' staff on all projects, with proprietary and need-to-know barriers to internal communication being minimized. Sometimes the solution to a problem exists in the mind, or on the bookshelf, of a colleague just down the corridor. But unless he knows that the problem exists, he cannot help.

Given talented scientists and generically relevant programs, it is then important for management to emphasize the need for flexibility and responsiveness on the part of the scientific staff in serving their corporate patron. We find that first-class basic research scientists usually make first-class industrial scientists. This seems to be related to their superior ability to handle complex, multi-variable problems. Time, money, and market opportunities simply become additional variables in the equation, and do not seem to cause undue concern once their role and significance is appreciated.

Finally, the best approach to solving a technological problem is that used in conducting pioneering basic research, i.e., a combination of inspiration and profound consideration of fundamental laws, concepts and mechanisms. Simply reaching for the textbook is likely to result in an unimaginative and incremental solution – not the visionary solution that turns a problem into a competitive edge.

Case B: Adhesively-Bonded Aircraft Components

A few years ago, the aircraft components manufacturing division of Martin Marietta requested the Laboratories to investigate a problem which, for several consecutive summers, had led to an unacceptably large number of rejections in the production of adhesively-bonded thrust reversers for large jet engines. The problem as presented was as follows: metal bondments, approximately 2.5 meters in diameter, and consisting of aluminum skins adhesively bonded to an aluminum honeycomb core, were failing to meet an inspection criterion that required a certain force be necessary to "peel" the skin from the core. In a few cases, the peel strength was only 10% of that required to pass inspection, Fig. 2.

No one had a clear idea of what the problem really was. Explanations ranged from poor quality adhesives to an inspector with bad breath! Fortunately, at that time the Laboratories was in the midst of expanding its surface analytical capabilities, and such equipment as a Scanning Transmission Electron Microscope (STEM), an X-ray photoelectron spectroscope (XPS), and an Auger Electron Spectroscope had recently been purchased. Using these tools, two significant

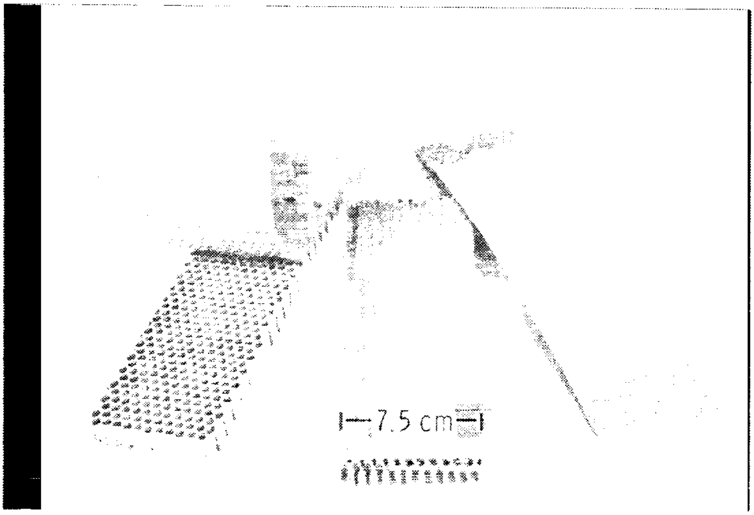

Fig. 2. Peeled bondment showing strong (left) and weak (right) adhesive bonds. The slick surface and low curvature specimen on the right results from near-instantaneous failure at the adhesive-metal interface, rather than failure within the adhesive, as on the left.

observations were made: first, using Auger and XPS to analyze the surface layers of readily peeled skins, an unexpectedly high concentration of fluorine was detected. Acceptable parts did not show its presence. Second, use of the STEM at high magnification (50,000 times) revealed that "good" and "bad" surfaces exhibited very different morphologies. Specifically, a microscopically rough and porous surface was characteristic of good bondments, Fig. 3 (a), whereas poorly bonding surfaces were smooth, Fig. 3 (b).

The significance of these differences was not appreciated at first, and attention was focussed on the role of the fluorine contaminant. But attempts to reduce peel strengths by intentionally contaminating the aluminum surfaces with fluorine compounds prior to bonding were unsuccessful.

Consequently, attention was focussed on the observed differences in surface oxide morphology, and it was realized that the micro-roughness might be involved in "interlocking" the metal parts with the adhesive. Indeed, we then found that the function of the acid etches used to pre-treat the aluminum skins prior to bonding was not merely to clean the surfaces, as generally thought, but rather to produce a rough oxide surface that the adhesive could penetrate and hang on to. Needless to say, we were somewhat surprised to realize that large aircraft components were literally held together by 100 Å thick aluminum oxide whiskers!

The relationship between the fluorine contaminant and poor bondments then became clear, as follows: After being etched in an acid bath, the metal surfaces were rinsed with fluoridated tap water. Exposure to the high summer humidity during storage prior to bonding then resulted in the formation of HF on the

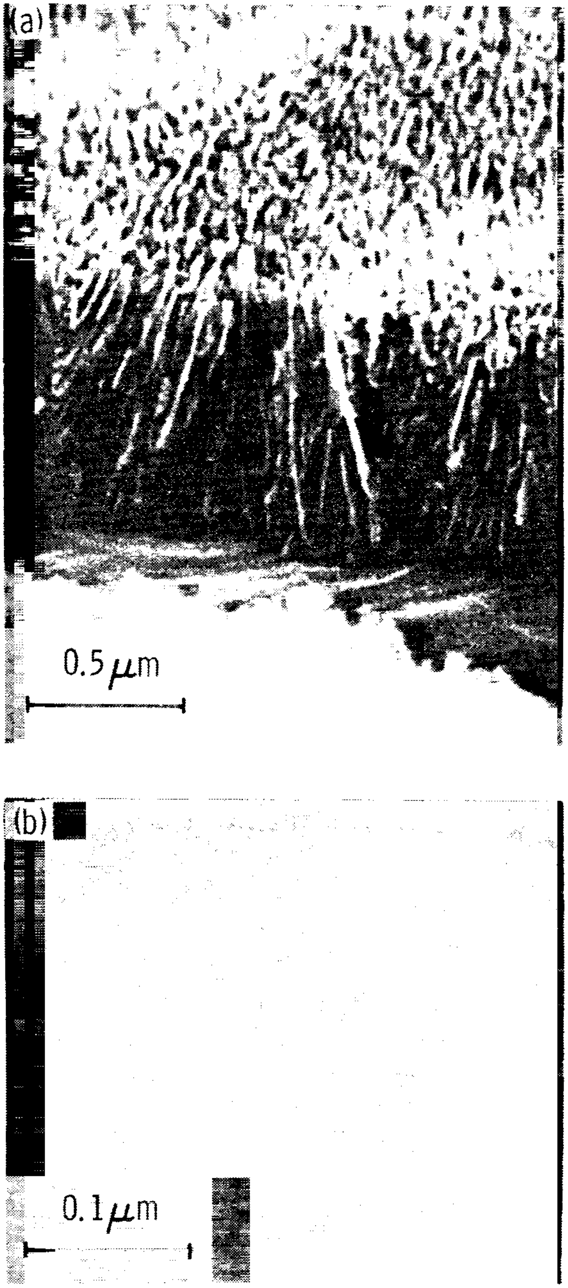

Fig. 3. (a) STEM-revealed structure of a good bonding surface. The aluminum oxide "whiskers" formed during the acid bath provide mechanical interlocking with the adhesive. (b) A poor bonding surface. The aluminum oxide "whiskers" have been dissolved by hydrofluoric acid, leaving a smooth "ungraspable" surface.

surfaces ... and this strong acid slowly etched away the protruding oxide fingers, leaving the surface smooth. The result was the summertime occurrence of bondments that failed the peel test.

The corrective action was obvious. Nowadays, distilled water rinses and an air conditioned environment have eliminated the problem, with consequent cost savings and improvements in productivity. But the work leading to this solution has revolutionized fundamental thinking about the nature of adhesive bonding3)-5) , and subsequent studies have led to the development of chemical inhibitor washes that reduce the in-service degradation of the whiskery oxide structures by moist, chloride-containing environments, and so enhance the lifetime of complex, bonded aircraft components.

The lesson to be learned here is that it is important to *identify the correct problem*. In our experience, it is remarkable how often the problem one must actually solve turns out to be quite different from that first presented to you. Defining the real problem by investigative analysis can lead to the identification of fertile areas for basic research, i.e., when you realize that you are unable to solve the problem because you don't understand the fundamental issues involved. In case B, the real problem was that we did not know the essential mechanistic difference between a good adhesive bond and a poor one.

A corollary to this lesson is that it is equally important to decide whether or not the problem, once identified, is significant enough to have your staff spend time trying to solve it! Certainly, whenever one of your scientists visits an operating plant he will surly uncover a problem that is considered by one of the engineers to be extremely important. If he accepts the latter's judgement without further review, there is a fair chance that, on returning with a solution, he will find that the engineer has been reassigned and the problem has "disappeared". To establish which problems really are important, and are likely to remain so, you must speak first with the plant manager, or someone of equivalent stature. If he says that such and such a problem is affecting his plant's performance, and especially if he indicates that he is willing to pay for your efforts to solve it, then there is a real likelihood that the problem really is worth working on.

When your staff believes they have found the answer to the problem, it then becomes important that they discuss their findings and recommendations first with the engineer, not the manager ... for it is not unlikely that your staff have solved only part of the problem, and that their solution will require substantial modification before it can be usefully employed. The engineer's subsequent personal contributions are likely to be crucial to successful implementation.

If, on the other hand, you present your solution first to the manager, then you can anticipate subsequent difficulties. In all probability he will immediately contact the engineer and ask him to explain your solution. Because the engineer has yet to hear about it, he will be at a disadvantage, and his discomfort may well be reflected by his being less than fully cooperative during implementation.

The guiding principle in such cases is as follows: when identifying problems, start at the top of the managerial hierarchy and work down; when implementing solutions, start at the bottom and work up.

Case C: Advanced Blasting Technology for Stone Quarrying

One of our most satisfying projects in technology transfer relates to quarry blasting, where the application of instrumentation developed for the aerospace industry has made possible useful productivity improvements in a basic industry. When the Laboratories began its "carrier wave" program on the fundamental factors involved in efficient quarrying, most scientific studies of blasting were focussed on static models of the gas pressurization of radial cracks, and blasting practice involved the use of very brief delays (10–25 milliseconds) between charges to accomplish the desired fragmentation. The principal motivation for further research was to minimize the impact of the escalating cost of explosives.

Early on in our studies, two problems were identified. First, the fragmentation mechanism previously thought to be dominant, namely gas pressurization of radial cracks, was found to be only a minor contributor. Second, the use of high speed cinematographic techniques, developed to study the flight behavior of missiles, revealed for the first time that the erratic performance of the delay detonators used to set off the explosive charges was seriously compromising blasting efficiency.6)-8)

Research to clarify the fragmentation mechanisms of explosively-loaded solids was then undertaken in conjunction with scientists at the University of Maryland. This work led to the discovery that fragmentation in rocks occurs primarily as a consequence of the interaction of stress waves with preexisting flaws. Over the next few years, the effects of varying the delay time between explosions on rock fragment size, and on the amplitude and frequency of the resulting ground wave vibrations, were quantified.

Our findings have had a number of impacts on the quarrying industry. One has been the development by explosives manufacturers of new and more reliable chemical detonators. Concurrently, small independent businesses have begun to develop electronic sequential blasting machines that permit more sophisticated and efficient blasting patterns to be designed and used. Another outcome has been the capability for changing the frequency of ground vibration, moving it out of the resonance range of a typical home so that it is less annoying to neighbors, without reducing fragmentation efficiency.

In this case, technology transfer was accomplished by direct demonstration in the quarry, with test or production blasts being monitored or supervised by the Lab's team of geologists, Figs. 4 (a) and (b). Moreover, as each new glimmering of understanding was gained, it was promptly evaluated in a field test. Thus, knowledge was transferred incrementally, and by immediate and visible demonstration of its benefits. It was necessary to pursue implementation in this vigorous and high-profile manner not only to justify continuation of the program to a plant management not intrinsically enthusiastic about R&D, but also because of the unskilled nature of the quarry workforce.

Incidentally, and not surprisingly, our findings and the more efficient blasting procedures resulting from them were accepted and introduced more rapidly into

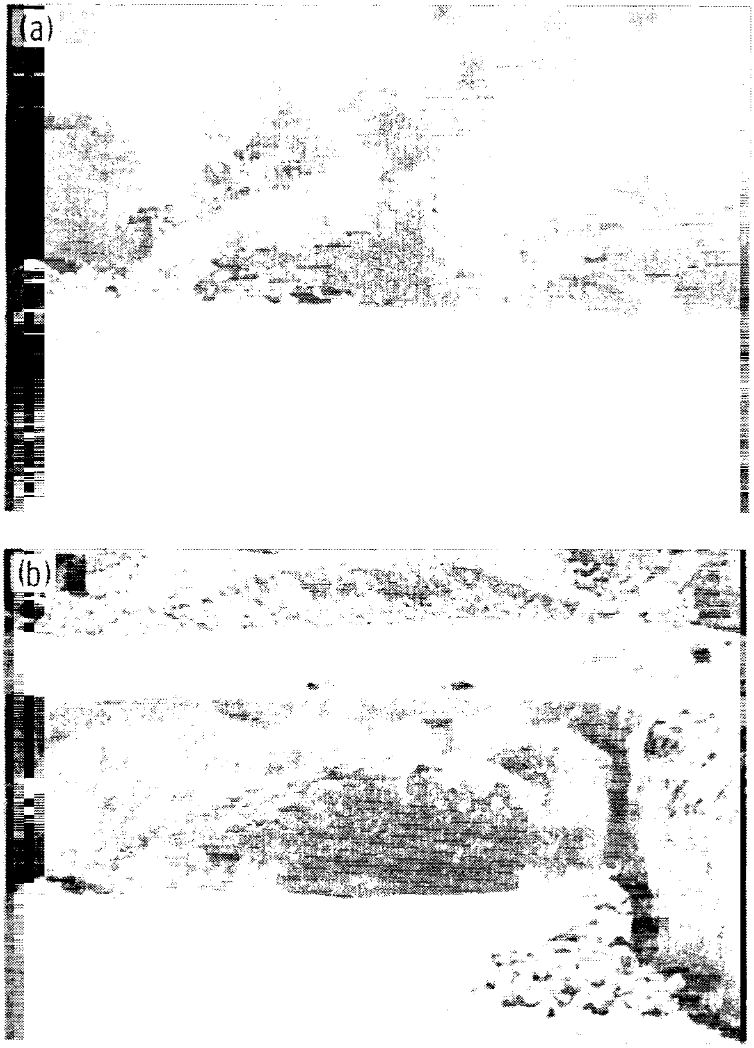

Fig. 4. (a) A badly designed quarry blast, with poor fragmentation and much flyrock; (b) A good shot: on flyrock, good fragmentation, and an excellent muck-pile. Bench height about 10 meters.

the more sophisticated mining communities involved with copper, iron ore, and oil shale.

Even so, within a few years 80% of our plants were using sequential blasting machines. And blasting patterns incorporating the longer delays necessary for maximum development of explosively-induced fractures (50–100 ms) were then introduced . . . followed by patterns that reduce ground vibration. By late 1982, productivity increases of 40% through the first crushing operation were being documented, and 15–20% at the truck loading stage. Both operations are significant cost centers, accounting for three to five times the cost of drilling and blasting. Thus, our original goal of reducing the cost of explosives turned out to be less significant than our actual impact on downstream production processes. Somewhat ironically, if we had actually reduced the quantity of explosives used per shot, we would not have been able to exploit our newfound understanding of the mechanisms of fragmentation.

The lesson learned in this case is the value of on-site, personal demonstration in technology transfer. In general, we have found that *technology is not transferred by the written word.* Of course, for purposes of management information and permanent documentation, it is important that your scientists write reports that are brief and clear, and that any action called for is set out unequivocally. But reports simply are not the mechanism by which understanding is translated into practice. Labs results and insight are best conveyed verbally, personally, and by on-site demonstration. The transmission of your scientist's own enthusiasm for the results of his labors can prove to be *the* vital element in their acceptance by plant operators, and so determine their eventual use.

In this regard, we have found that the establishment of "liaison scientists" to be a useful stratagem. Such persons spend extended periods at the plant and, in time, become recognized as someone ready, willing and able to help solve plant technical problems. The liaison scientist becomes a person with a recognizable name and face, and part of the team . . . *their* team, and hence accepted. Such credibility is invaluable in technology transfer, and was especially so in this case. Over the years the Lab's geologists spent many months in the quarries, and the cameraderie so developed resulted in excellent cooperation.

Case D: The Manufacture of Periclase from Brine and Dolime

Periclase (MgO) is used extensively by the steel industry for refractory applications. In the U.S. it is produced by reacting $MgCl_2$-containing brine, obtained from beneath the Great Lakes, and high purity dolime ($MgO \cdot CaO$), a mineral in good supply in this region.

In the mid-70's, one of our plants using this process was experiencing problems in productivity and product quality related to the settling, filtration and washing behavior of the magnesium hydroxide intermediate product, known as the slurry. Especially perturbing was the observation that a competitor's slurry used occasionally in the plant could be handled without difficulty, and consistently yielded a higher density of the final sintered product, a marketing advantage. Accordingly, the Laboratories initiated a program to develop sufficient basic understanding of the plant's process to correct the operating problems and produce a product at least as good as that of the competition.

A liaison scientist was assigned the problem, and using transmission electron microscopy (TEM), he soon identified one cause of the difficulties. The crystalline form of the intermediate product, magnesium hydroxide, was found to vary from time to time in the process stream – being primarily prismatic during normal operations, but platelike during process upsets. Because the platelets packed more densely, they caused problems in settling, filtration, and washing.

However, spotting the cause of the problem and solving it were significantly different challenges, because at that time no one knew what the influence the various operating parameters (pH, chloride ion concentration, temperature, etc.) on crystal morphology was.

Accordingly, the scientist and plant staff developed an operating flow chart, quantified the procedures used by the plant, and identified the key processing parameters. Subsequently, laboratory studies established the fundamental relationships between the processing parameters and slurry particle morphology. We then appreciated that the desired product could not be produced using the existing operating procedures and equipment. Consequently, new types of reactors were developed, a dolime pre-treatment process added, and some of the liquid recycle routes altered. These changes helped substantially. But, meanwhile, market forces and environmental factors dictated a change in the product mix, and this entailed producing slurries to much tighter compositional tolerances.

At this point, an interdisciplinary team of chemists, fluid dynamicists, and electrical and chemical engineers was formed. Together they developed a mathematical model of the new process, identified appropriate control strategies, and specified, acquired or fabricated the necessary control equipment. Finally, after about seven years, the desired productivity and quality goals were achieved.

Incidentally, the competitors' slurry, once hailed for its superior quality, now does not meet the plant's more rigid specifications and so cannot be used. The price of progress!

The lesson to be learned in this case is *the need for patience and persistence.* It may take only a few hours for an innovative scientist to come up with the potential solution to a problem, and perhaps only a few months to work it out. But it may take several years, a number of failures, and a lot of money before the results of his labors come to fruition and begin to return a profit on the R&D investment. This fact of industrial life seems to be true even with successful projects, as in the present case. The blasting studies likewise were conducted over a period of several years . . . three to seven years being not untypical for the lifetime of such projects.

Sometimes a delay between the time the Lab's own work is done and that when the results begin to be applied arises because there are only certain times when new developments can be introduced into a functioning plant, e.g., during down-time for maintenance, or when some new piece of equipment is being brought on stream. Such "natural" times rarely seem to coincide with those at which the research projects are completed. So it is important to explain this fact of life to the aspiring industrial scientist before he begins to suspect that such delays occur because management is not really interested in exploiting his efforts.

It is also important for an industrial scientist to learn to anticipate success, and reflect on such questions as, "If my research is successful, what will the plant manager have to do? How much money will he have to spend to utilize my results? Can he affort it soon . . . or when?" Sympathetic recognition by the scientist of the problems of the plant manage consequent upon being the (presumably) happy recipient of some new and superior technology will surely make its transfer a little easier.

In Summary

Not all scientific projects aimed at solving industrial problems are successful. In fact, most fail. Sometimes this is because Mother Nature places some apparently insurmountable barrier between the scientist and the solution. Sometimes the solution is not translated for financial or operational reasons. Sometimes conflicting personalities get in the way, or competitive attitudes arise between the central and operating company laboratories. Hopefully, however, consideration of some of the hard-won insights presented here may help industrial R&D managers improve their batting average in transferring new scientific understanding into more efficient industrial practice.

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THE ROLE OF UNIVERSITIES IN RESEARCH AND DEVELOPMENT

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1. Development Pattern of New Technology

There are various kinds of channels through which knowledge transfer occurs between universities and industry. I would like to focus on one particular aspect of this transfer: that of production techniques in the mechanical industry. There have been many recent innovations in this field. New methods of material processing such as laser cutting are progressing rapidly, along with robotics, flexible manufacturing systems (FMS), computer aided design and computer aided manufacturing. These and other developments have been influential in improving the level of automation, and their technologies are now extensively used in industry. If we look at the origins of these techniques phenomenologically, we find that some originated in universities and some in industry. For example, the first FMS, accompanied by many new concepts, was invented by an English engineer named D.T.N. Williamson at the Mollins Company in England. Mr. Williamson's project started in 1950, using a computer, robot-like equipment and other apparatuses, the concept of which is still new and effective today. Unfortunately, it was too new in 1950, and the company did not succeed in selling even one system. When the concept was presented at a conference in 1967 it was greatly admired, and most of the audience believed it marked a new era in production technology – that is, the automation of small-batch production. But this was only a dream, which must have turned into a nightmare for Mr. Williamson and his colleagues who had to wait more than 20 years, almost to the end of the 1970's, before FMS technology became practical.

Another example is computer aided design. Mr. I.E. Sutherland, a graduate of MIT, presented a paper about a CAD system in 1963 in which he predicted that all drafting boards would disappear in 5 years, replaced everywhere by computer graphics. That was the dream. But again, it turned to nightmare. Like FMS, it took more than 20 years for CAD to become practical, and even today industry employs an army of drafters in every field.

Regardless of whether a new technological concept is born in the universities or in industry, it seems to take 15–20 years for the dream to become a reality.1) This is not surprising in the case of basic research. But in the case of production technique, where research has an explicit purpose, this 20-year gap is harder to

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explain. The dream-nightmare-reality pattern of technological progress poses some interesting problems. New concepts are new for a number of reasons: they are not yet generally accepted, people don't know their influence, the elementary techniques are not refined, no one knows their cost performance, etc. In short, people have very poor knowledge about a given concept or technology when it first appears. In order to realize the dream of a new technology, a prodigious amount of new knowledge must be generated. This knowledge must illuminate such factors as basic technology, economic aspects, social acceptability and psychological influence.

2. The Role of Researchers in Universities

Many dreams have disappeared and been forgotten, but some have attracted people's attention. Researchers in universities play an important role here. A single researcher or laboratory cannot confirm the validity of a newly proposed technology. This is especially true in production technology, because it incorporates many different elementary techniques. It is essential that many researchers from different fields contribute to the development of a new area of investigation. Each of these contributing researchers then creates a research topic related to the central theme but incorporating his own specific interests. Sometimes these individual topics seem to bear little direct relationship to the original concept, but it is important to explore many directions in the beginning, since no one can definitely identify the necessary elementary techniques at such an early stage. This kind of exploratory activity often takes place in the universities, since researchers in those institutions enjoy more freedom in selecting their topics. It should be pointed out that no one can intentionally undertake an effective research project in a strict way in order to shift a technology from its dream phase to reality. We must rely upon the intuition of individual researchers who may approach the problem from a wide variety of perspectives.

For example, the first FMS by Mr. Williamson, who was a brave man, shocked many researchers in production engineering. It was only a machine system to fabricate mechanical parts out of aluminum, but after he presented his system to the public, many researchers where stimulated to pursue work in related fields such as computer control of machine tools, automatic handling of workpieces, layout of work stations, structured analysis of machines and reliability of machine elements. All of these fields were essential before Williamson's central idea could be realized as a commercially feasible product.

CAD has a similar history. Since 1963, when Mr. Sutherland first showed a prototype, the number of researchers in related fields has continuously increased until now there are nearly 50 university laboratories engaged in fundamental and application research on CAD information processing.

The development of computer and graphic displays which have made remarkable progress in the industrial sector have, of course, had a great influence upon the realization of CAD technology. This alone, though, was not enough. The role of computational geometry, a popular technical field primarily developed in the universities, was very important in bringing CAD into general use.

If we look at the development of industrial robots, we find a similar phenomenon. The first commercial robot appeared in the United States in the 1960's, but again there were 20 years of nightmare. It was 1980 before industrial robots became truly commercialized. Another example is the Joseffson Computer, except in this case the nightmare period has just begun. The important point is that the nightmare phase is too long, resulting in social loss. The following section presents a proposal for solving this problem.

3. Policy for a Better Developmental Process

We have three parameters:

(1) initial concept
(2) elaborate survey on the proposed technology
(3) large national or international project

When these three parameters resonate within a society, we can expect a shortened nightmare period for a given technology. A good example of this kind of project-oriented process is the case of maintenance automation, which will be described here in some depth. In industrially developed countries, a great deal of effort has been devoted to improving the efficiency of production. High productivity has been substantially achieved in a wide range of industrially manufactured products, from large structures, chemical plants and railways down to small household goods. Moreover, new products are constantly being added. In Japan, however, where there is a shortage of land, raw materials and energy, there are people who feel that this burgeoning growth cannot be continued indefinitely; but this is not the case. On the contrary, the need for maintaining the products already produced will inevitably increase.

We have made surveys of this phenomenon in different sectors, but there is another point which is more serious: the productivity of maintenance is decreasing. An example is shown in Table 1.

Table 1.

	1975	1976	1977	1978	1979
Passengers-kilometers + transported tons-kilometers ($\times 10^9$), (A)	109.3	109.6	113.4	116.1	118.6
Maintenance cost ($\times 10^9$ yen)	162.7	183.9	204.5	215.3	233.1
Deflator (1975 = 100)	100	106.7	112.4	117.1	119.4
Maintenance cost ($\times 10^9$ yen) (price in 1975), (B)	162.7	172.4	181.9	183.9	195.2
(A)/(B)	0.672	0.636	0.623	0.631	0.608
(A)/(B) (1975 = 100)	100	94.6	92.7	93.9	90.5

Annual decrease in production per maintenance cost

This is the conclusion. The total amount of maintenance cost will increase rapidly (it is currently at 50 billion dollars per year) while the productivity of maintenance will continue to decrease, resulting in a large maintenance burden for Japan in future years. In order to solve this problem, automation will be a key technology.

Certain projects have already been undertaken to clarify the maintenance problem and explore possible solutions. In 1978, an informal research group was established at the University of Tokyo for the purpose of constructing a general-purpose maintenance robot. This group was formed on the basis of an intuitive understanding of the nature of the maintenance problem which seemed likely to become a serious burden in the future.

The group consisted of three professors and three associate professors whose fields were robotics, kinematics strength of materials, control theory, metrology and artificial intelligence. The possibility of such a robot was intensively discussed among the members of the group, and the group was granted a grant-in-aid from MOMBUSHO.2)

In 1979, a project was implemented with members from universities and laboratories, sponsored by the Machine System Promotion Association. The objective of the project was to carry out a feasibility study on maintenance automation.

This project was redirected in 1981, in order to construct a prototype of a maintenance robot. The project has entered the final stage and a prototype with spatially designed double-rotating wheels, a nine-degrees-of-freedom manipulator and an intelligent brain has been nearly completed.

In 1981, Prof. Shimo organized a project where economists and maintenance engineers developed a methodology of maintenance research and revealed important results about the economical status of maintenance in various sectors in Japan.3) There were other maintenance issues studied from various points of view: technological, economic, social and psychological.

Finally in 1983, MITI started a national project on robotics (JUPITER). The main purpose of this project is to construct three prototypes.

(1) A robot which maintains equipment inside the containment vessel of a nuclear plant.

(2) A robot which diagnoses and repairs undersea structures.

(3) A robot which performs repair and rescue in disaster areas.

These are all maintenance functions in a broader sense. When maintenance jobs are done manually, it is usual that workers are exposed to a dangerous and uncomfortable atmosphere. Therefore, maintenance robots will be an important resource not only from the economical point of view but also from the humanistic point of view. If we look at the technical characteristics of a maintenance robot, we can expect to find many aspects that are completely different from conventional industrial robots which require a great deal of high technology. This may create efficient channels between this project and university research, although the project is supervised by MITI, and universities are under the jurisdiction of the Ministry of Education, Science and Culture (Mombusho).

Since 1975, it has become popular to conduct research on robotics at many universities in Japan. This research is categorized as follows:

(1) Research aimed at the realization of human-like functions, from a mechanical aspect.
(2) Same as above, but from the aspect of information processing and control.
(3) Research on advanced robots for practical use, including industrial use such as assembly, inspection, etc, and welfare use such as nursing in hospitals.

As far as research on robotics is concerned, research without any explicit practical purpose has predominated over other types of research in Japanese universities. Many researchers in universities are interested in developing particular robots which perform as human beings. For example, pedestrianizing robots (two-foot pedepulators), manipulators which perform knotting, rotating a crank, grasping a paper cut etc. have been of major interest to them. Many delicate sensors and actuators have been developed in these investigations. These robotics technologies developed in universities have not been applied to practical problems in industry, and thus fewer studies have been carried out on efficiency, economics and reliability. But, these technologies have played important roles for the realization of the JUPITER project because it is believed that they can be key technologies in developing the practical robots aimed for in the project. Thus, we can expect a spontaneous transfer of knowledge from universities to industry.

Before finishing, I would like to say a few further words. In democratic countries, I believe that technologies should be for the people, and at the same time by the people. Therefore, dreams should be the people's dreams. People's dreams will be the key to create an efficient-project and will be accompanied by a fluent flow of ideas and knowledge among different sectors.

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POLICYMAKING FOR EARTHQUAKE PREDICTION IN AMERICA AND JAPAN

W. Henry LAMBRIGHT*

Introduction

How does a society govern the emergence of a new technology? How does government affect the cycle from research to development to demonstration to operations? Who influences government policy along the way? How? These are generic and recurrent questions. They are relevant to any number of technologies, and any number of governments. They are questions particularly relevant to those technologies that fall most directly under governmental cognizance; that is, those in which government is both the primary developer and user. Here, government's impact and responsibility are greatest. Its policies largely determine the pace and direction of these technologies.

Consider the case of earthquake prediction, and how two different governments – Japan and the United States – are managing its introduction to their respective societies.[1)]

The Technology

By earthquake prediction is meant the forecasting of time, magnitude, and place of a particular quake. Is there a technology that can do this? The answer is yes and no. Yes, there have been predictions, including one successful prediction of a large destructive quake in China. No, the technology is not ready in the sense of providing a means to predict operationally, i.e., routinely and reliably. Earthquake prediction is far from the status of a service, such as weather prediction. Yet, in China, earthquake predictions are issued frequently, if not routinely. For China, earthquake prediction is "ready enough" to be considered a usable technology. Other countries disagree.

The ambiguity lies in the phrase "ready enough." Some technologies are not "ready" in a strictly scientific sense. There are innumerable technical questions left to be answered, and more research, development, and field testing are needed to perfect the technology to move it to operational status. However, those technologies may be viewed as "ready enough" if the problem to which they are being applied is so grave that more is lost from not using, than using. Those "ready enough" decisions are the kind of value judgments that bedevil

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technological innovation in many areas. Where the technology is in an emergent or "coming" category, it is quite possible various countries will make different decisions as to its readiness for use. Similarly, within a given country, different participants in a decision process will disagree. The decision is scientific and political, involving interconnected issues of capability, desirability, cost, and risk.

"It's a serious dilemma," says C. Barry Raleigh, director of Columbia University's Lamont-Doherty Geological Observatory and previously coordinator of a United States Geological Survey (USGS) earthquake prediction group. For a number of years, he has said, "we will be in the awkward in-between stage where we have gathered the right information, but we will not have had sufficient experience to make a very clear statement about what it means, and I think everybody who's very close to this program has a considerable sense of unease about it."2)

There are horrendous problems of scientific and governmental accountability raised where earthquake prediction is concerned. A false prediction can sometimes be worse than an actual quake in terms of economic disruption and psychological stress. Scientific reputations can be damaged and government officials can lose their jobs if they are not careful. On the other hand, failure to predict a destructive earthquake when the technology at hand suggests its possibility (or probability) has its own risks, and issues of responsibility.3)

As a technology, earthquake prediction is analogous to certain emerging medical techniques. It entails a combination of scientific understanding, sophisticated equipment, and human judgment. They add up to a technological skill. The skill is improving and may be applied under extreme circumstances. Indeed, the technology may alleviate pain and suffering even though the underlying scientific reasons are not fully understood. But, there is also a chance it will not work at all, or can worsen a situation. It is, therefore, ready enough in some cases and not in others. It can be made readier, if appropriate decisions are made. Often, in the application of a novel medical technique, the decisions are made by a doctor and patient. In a situation like earthquake prediction, societal decision making is involved.

Earthquake prediction is a public technology in every sense. It must be governed as it develops and as it is used. This paper represents a first step in examining how two nations – America and Japan – make those "ready enough" policy decisions with respect to this emerging technology. In dealing with these countries, we are addressing questions of national policy toward the same evolving technology. What is the national policy? How has it come to be? What forces have influenced its shaping along the way? Where is it headed? And what broader significance does the case of earthquake prediction hold for the way these two nations make policy for science and technology in general?

Present Policy

Present policy for earthquake prediction in Japan is enunciated most clearly in the Large-Scale Earthquake Countermeasures Act of 1978 (LECA), and supplemental legislation. United States policy is found primarily in the National Earthquake Hazards Reduction Act of 1977 (NEHRA), and its amendments. Both these acts address earthquake prediction specifically. The Japanese policy is that earthquake prediction technology is ready enough to be used under certain conditions. Accordingly, it lays out in detail responsibilities for issuing the prediction and warning and obligations by governments at different levels for earthquake prediction, prediction response, and preparedness planning. It sets forth policy applicable specifically to those areas designated by the Prime Minister as being "areas of intensive measures." That designation makes them a focus of intensive scientific activity aimed at short-term prediction. It makes local and prefectural governments in the area subject to national regulatory policy. In 1980, additional legislation made such areas eligible for federal financial assistance to prepare for the impacts of the prediction and the earthquake itself.

Japan has grandly and in detail made public policy for earthquake prediction. Under that policy, the Tokai region, in east-central Japan between Tokyo and Nagoya, is the first area of intensive measures. Japan has made the national decision to predict and prepare for the Tokai earthquake.4)

The United States policy, as established initially in NEHRA, is far, far less detailed but equally ambitious. The act also assumes the technology is ready enough. The key policy choice can be summed up in the provision of NEHRA that calls for:

> . . . the implementation in all areas of high or moderate seismic risk, of a system (including personnel, technology, and procedures) for predicting damaging earthquakes and for identifying, evaluating, and accurately characterizing seismic hazards.5)

This is a call for an operational earthquake prediction system in areas requiring such intensified measures. The goal is similar to that expressed in LECA. NEHRA does not point out who does exactly what in terms of prediction, warning, and response. Under previous legislation, however, the USGS is given the role of issuing geological hazard warnings to governors of affected areas and other relevant parties.6) Subsequently, in 1980, the National Earthquake Prediction Evaluation Council (NEPEC) was established to advise USGS in determining "when and/or whether to issue prediction statements or other information pertinent to the potential for a future significant earthquake . . ."7) Also in 1980, NEHRA amendments made the recently created Federal Emergency Management Agency (FEMA) responsible for developing prototype earthquake prediction response plans for areas of the nation at risk.

In Japan, the prediction agency that performs the USGS role is the Japan Meteorological Agency (JMA); the response and preparedness agency that performs the FEMA role is the National Land Agency (NLA). There is also a science advisory committee to JMA somewhat analogous to NEPEC. This is called the Hanteikai. There are thus similarities in national policy and administrative structure. However, there are considerable differences.

The most obvious difference in policy is the fact that LECA places authority for issuing warning, based on prediction, on the Prime Minister. The decision to predict is thus given the highest possible political/policy significance in lodging responsibility there rather than leaving it ambiguous or with a lower administrator, as though this were just another hazard warning. The other notable difference is the authority to enforce that is lodged in the Japanese national government, but which is not mentioned in NEHRA. Finally, there is the matter of money. There is no equivalent in NEHRA or subsequent amendments to Japan's special financial law passed in 1980, which provides federal subsidies to those areas requiring intensified preparedness for earthquake prediction.

Policy in Japan and the United States is thus alike and unlike. As implemented, the policies become even more dissimilar. The legislative policies provide a legitimation in both countries for creating an operational earthquake prediction system. In implementing LECA, Japan has created a prototype operational system for an expected great (8+ magnitude) earthquake – Tokai. The United States has not done this. It is still in the research and development stage, moving toward a feasibility experiment for a relatively non destructive quake. The Japanese program is thus much bolder. Whether it is wiser remains to be seen. The purpose here is not to judge either policy, but to explain how each came to be, and the issues arising from the way past policies have been formed and present policy implemented in the two countries.

Forces Shaping Earthquake Prediction Policy

The course of policy in both countries can be seen as resulting primarily from the interaction of scientists and policymakers. By policymakers is meant those high ranking elected and appointed officials who chart the basic course of the nation. Some policymakers are also scientists, at least by original training. This fact gives them a potential integrating role. Certainly, integration is needed because, as communities, scientists and policymakers are different.

For scientists, there is never enough data, information, equipment, and time. Policymakers care much more about actual prediction than creating the capability for prediction. They want results, not research; technology, not development. For them, there is the need to know when, where, and with what consequences a quake will strike. They want yes and no answers, not probabilities. Their priority is to know if they will have an earthquake problem while they are in office. If so, they want prediction now. If not, they have priorities other than a technology still emerging and needing policy assistance.

These are central tendencies. Individual scientists and policymakers vary. But these basic differences in orientation, especially time perspectives, between the long term scientist and the short term policymaker, are real. They hold in both Japan and America. They color relationships with respect to setting a policy course for earthquake prediction. They matter in how much is funded in terms of R&D and whether the technology is moved quickly or slowly along the continuum from research to development to demonstration to operations.

It is the interaction of scientists and policymakers that provides the common thread in the evolution of earthquake prediction policy in both the United States and Japan. Scientists determine what can be done, but policymakers decide what ought to be done. The combination of can and ought determines how "ready enough" earthquake prediction really is.

Initiating a Research Program

In the early 1960s, scientists in both the United States and Japan were pushing for a major, long term national research and development program in earthquake prediction. In Japan, there was a national decision to move ahead. In America, there was something much less.

A key event in the beginnings of the Japanese program came in 1962. A group of leading Japanese scientists was convinced that recent understandings of plate tectonics and related fields were making it possible to believe earthquake prediction could become a reality. This group put together a report, published in 1962, called "Prediction of Earthquakes – Progress to Date and Plans for Further Development." It became known to Japanese scientists as the "blueprint" of earthquake prediction research. The blueprint described what the Japanese scientists regarded as the best way to achieve prediction of earthquakes, and provided the guiding principles for an earthquake program in ensuing years. According to Rikitake: "The most important point emphasized in the blueprint was to make every effort to obtain basic data for possible prediction rather than to hasten the actual forecasting." At the end of a ten year research program, it was felt it could be determined how feasible earthquake prediction could be.8) This was to be a research program in emphasis. The goal was new understanding, better theory. No specific funding level was mentioned.9) Whatever the scientific merit of the proposal, it took a while to persuade policymakers to adopt this program.

In 1963, a subcommittee for earthquake prediction was established by the Geodetic Council, Ministry of Education, which was responsible for administrative coordination of geodetic and geophysical research in Japan. Also, in this year, the Science Council of Japan (equivalent to the U.S. National Academy of Sciences) recommended that the government take action in promoting earthquake prediction research. In 1964, a destructive earthquake hit Niigata City. Policymakers were thus reminded of Japan's extreme vulnerability. In 1965, the national government initiated the proposed long term program in earthquake prediction research.10)

Funds came from various agencies, and were spent by government laboratories and universities. Among the principals were the Japan Meteorological Agency, Geographical Survey Institute, and Tokyo University. The Geodetic Council provided a measure of coordination through a five-year plan for the overall effort.

While Japan was making a decision, U.S. scientists were sustaining a disappointment. There was hope for action following the great Alaska earthquake of 1964. In the wake of that event, the President's science adviser appointed a panel to consider the status of earthquake prediction. This panel, headed by

Frank Press, proposed in 1965 a $137 million ten-year program. As in Japan, the orientation was research, including research instrumentation for monitoring purposes.[11] The proposed program did not get very far in an administration increasingly preoccupied with overcommitments in Vietnam and resulting economic troubles.

Not helping the scientists' cause was the scramble among competing bureaucratic interests for anticipated new earthquake prediction funds. Instead of cooperating to help get a program decision, USGS and Environmental Sciences Services Administration (ESSA), (the predecessor organization of National Oceanic and Atmospheric Administration (NOAA)), openly competed.[12] They competed in vain for a national program that failed to emerge. Some new activities did get under way, and existing ones enlarged, within these agencies. But they did so in the context of individual agency decisions, rather than because of a national R&D commitment to advance the field of earthquake prediction.

Japan Shifts from Research to Development

Japan was under way with its new program, and it was thinking long-term research. No sooner was the program started than scientists were provided with an unusual opportunity for advancing their skills. During 1965–67, a swarm of earthquakes occurred near Matsushiro. These were small tremors and occurred at relatively close intervals of time. Because they persisted for two years, scientists were able to deploy instruments, maintain them, and test various prediction theories. Much was learned in a real world laboratory.

At the same time, scientists were seeking basic knowledge from field experiments. Policymakers, particularly at the local level, were hoping for information pertaining to future larger shocks. A subcommittee on earthquake prediction of the Geodetic Council responded by appointing a special group to assist Matsushiro. The scientists did not predict short-term earthquakes, with exact statements of time, place, and magnitude. But the group did indicate its expectation of an approaching "dangerous period (usually a range of a few months), a rough idea about location and possible maximum magnitude. The situation was much the same as long-range weather forecasting."[13]

This experience taught the scientists the utility of having observation networks in place before actual earthquakes occur. Also, they learned that "to some extent, it was possible to predict even though nothing certain was known about the underground process." As a consequence, "the violent activities in April and August 1966 were successfully foretold."[14] The scientists experienced success. It had to bolster their confidence, as they returned to a traditional research mode.

Then came the Tokachi-oki earthquake of 1968. This was a large (7.9 magnitude) destructive quake. It caused policymakers to press the scientists to speed up their movement from research to prediction. Following discussion at the Cabinet level, the decision was made to reorient the Japanese program from research to development. By development is meant the change of orientation from one guided by the quest for new knowledge to one guided by the need to refine what was known so as to predict as soon as possible. As evidence of this change, the word "research" was dropped from the earthquake prediction program's name.[15] Basic research did continue, but the program's emphasis was changed.

There were also changes in budget and structure. The budget increased. Three new centers were established to accelerate the processing of data. Each was under a different organization: Geographical Survey Institute, Japan Meteorological Agency, and University of Tokyo. A new organization was created, advisory to GSI, called the Coordinating Committee for Earthquake Prediction (CCEP). Composed of thirty specialists drawn from universities and government, CCEP would meet every three months and review data and determine long term likelihoods. Japanese scientists were not yet ready to promise short-term predictions of exact time, place, and magnitude. CCEP would designate certain areas for three levels of observation: special, intensified, and concentrated, depending upon suspected probability of an event, and social importance of location. CCEP was not authorized to issue an "order," so, when information was provided slowly, it was not always possible to conduct timely observations.[16] Japan was thus in its "second generation" earthquake prediction program as it entered the 1970s.

The U.S. Road to NEHRA

In 1971, the interest of United States policymakers in earthquake prediction was rekindled somewhat by the 6.3 magnitude earthquake in San Fernando, California. The General Accounting Office (GAO), an advisory unit to Congress, produced a report in 1972 noting that "fragmentation of federal responsibility and the lack of national goals made it extremely difficult for the federal agencies supporting earthquake research to launch a coordinated attack on the nation's earthquake problem."[17]

Along with USGS and NOAA,[18] there was the National Science Foundation (NSF) increasingly active in the field. GAO asked the Office of Management and Budget (OMB) to clarify the situation. By 1973, budget reductions helped determine NOAA's role. Under financial pressure, NOAA decided to terminate its prediction program altogether.[19] NSF, meanwhile, focused on fundamental geophysical research and the social science aspects of earthquake prediction.[20] USGS thus had earthquake prediction, as an applied research and development program, to itself. Unfortunately, the USGS budget did not permit it to move very broadly and quickly. Its earthquake prediction budget was running at $3 million at this time.[21] It concentrated limited funds on building a seismic network in places where problems might be most expected, such as along the California San Andreas fault.

By the mid-1970s, the scientific community in the United States was growing increasingly optimistic about earthquake prediction, in part because of scientific advances in this country, in part because of what it was learning from activities abroad, in Japan, China, and the Soviet Union.[22] In 1975, the Chinese predicted the Haicheng earthquake, an event that Frank Press, President of the American Geophysical Union, hailed as "one of the major events in the history of geophysics."23) Then came evidence in 1976 that a 4500 square mile area of California had uplifted, an uplift centered near the town of Palmdale, where the San Andreas and Garlock faults intersected. A similar uplift had been noted in Japan prior to the Niigata earthquake of 1964. Was what was called "the Palmdale Bulge" the precursor to a great California quake? Scientists and policymakers were concerned. USGS issued what was later termed a hazard watch – far from a prediction but a first step in that direction. The administration reprogrammed $2.1 million in USGS funds to intensify seismic monitoring in the area.24)

It was in this new atmosphere that scientists again pushed for a national decision. In September 1976, a study under the auspices of USGS and NSF called for substantial increases in earthquake prediction research funds.25) The National Academy of Sciences also called for a national commitment to a long-term program aimed at developing a reliable and effective operational earthquake prediction capability.26) The next year, 1977, the National Earthquake Hazards Reduction Act became law. This legislation reflected, in part, policymakers' willingness to go along with what many scientists seemed to be saying was needed and possible. The United States appeared even ahead of Japan, at this point, in establishing a policy for implementing an operational capability.

Japan and the Tokai Decision

Japan, however, was moving in the same direction, on its own. The trigger here was the highly publicized statement in August 1976 of a young scientist, Katsuhiko Ishibashi, that the great Tokai earthquake could be imminent. Tokai had been designated an area of intensified observation by CCEP just two years before. Ishibashi had focused his work on Tokai. Based on seismic gap theory, historical records, and current observations, he placed the point of rupture in the Suruga Bay, where it would impact the heart of the prefecture of Shizuoka, killing thousands.27)

In October, the Ishibashi view was given credibility by a distinguished scientist and senior member of CCEP, Toshi Asada, in testimony before the Diet. In November, CCEP formally reviewed and endorsed the Ishibashi statement. However, it found no indication that precursors leading to a Tokai quake had yet begun. In December 1976, the Geodesy Council called for changes in its third five-year earthquake prediction plan, then under way. It proposed moving to what Ishibashi had termed "the main round" – namely, the attempt to predict the Tokai earthquake. The Japanese scientists, like their American counterparts at this time, had been stimulated by the Chinese success. If China could predict, then surely a technologically advanced nation like Japan could also.

The Geodesy Council proposed an intensification of various observations and surveys in the Tokai district for long-term prediction, and the establishment of a continuous watch system to catch the shorter term precursors when they came. In addition, it recommended a special committee of leading seismologists to evaluate anomalous phenomena for short-term prediction.[28)]

The new priority earthquake prediction now had among policymakers was shown by the creation of a headquarters for the promotion of earthquake prediction at the Cabinet level. In early 1977, the headquarters designated JMA as the agency responsible for predicting the Tokai earthquake. This meant that all other agencies and universities with work in Tokai were obligated to telemeter their data to JMA. This agency would receive and analyze the data on a 24 hour basis. In April, CCEP created a special committee (Hanteikai) to advise JMA on earthquake prediction. In 1978, legislation was passed to add further legitimacy to these measures and to go beyond them via the Large-Scale Earthquake Countermeasures Act.

As in the United States, scientists initiated the policy process and high ranking elected and administrative officials channeled it in directions they deemed appropriate. Scientists and policymakers in the two countries had thus produced policies signaling the technology was "ready enough" to move toward operational use.

Implementation in Japan and the United States

The most striking difference between Japan and America in earthquake prediction is revealed by their respective implementation of national policies. Japan's scientists and policymakers have united and remained behind the goals of LECA. They have focused enormous efforts on one particular fault. To an American, the Japanese approach is analogous to the Apollo moon shot. The difference is that the scientific knowledge was there when the U.S. made its Apollo decision. In the case of prediction of the Tokai earthquake, the knowledge-base is not as complete.

Scientists who will do the actual prediction are those on the Hanteikai. They are among the most eminent in Japan and they are confident. The policymakers have indicated to them that they will provide a buffer by: (1) being the actual source of warning to the public; and (2) enduring the costs of mistakes – false predictions. What they do not want is a failure to predict the Tokai quake. They want a Haicheng, not a Tangshan. Tangshan was the 1976 earthquake the Chinese missed, with the result that over 600,000 people died in one event.[29)]

By 1985, Japan will have spent the equivalent of $1.7 billion in federal funds on preparedness measures in Tokai. These funds are being used for strengthening buildings, widening roads for evacuation, educating school children in prediction response, creating walls to block tsunamis, etc. The prefectural and local governments are also spending hundreds of millions in related efforts. Plans are written at the local level, coordinated at the prefectural level, and brought into a regional coherence by the National Land Agency. The Hanteikai will predict whether the quake will come in a few days – or hours. Planning is based on having a short-term prediction. The long-term prediction – a de facto one – was supplied in 1976 by Ishibashi.

Keeping up the momentum for the Tokai demonstration is exceedingly difficult. Public awareness is present, but there is an inevitable tendency to relax or go through preparedness motions as time passes. Then, also, Japan has suffered other quakes since the Tokai decision. The Akita quake of May 26, 1983 took 102 lives, many of them children.

There is thus pressure to expand the prediction effort to other areas. But there is only so much money; there are only so many scientists willing and able to perform the Hanteikai role of meeting every month and being on constant call. Yet, the pull from policymakers representing other earthquake prone regions to have their areas designated for intensified measures is considerable. The scientists argue that there has to be this first attempt to predict a destructive quake – a prototype test – before the process can move to an operational status. Even then, certain quakes may be literally unpredictable, beyond science at this point. The Tokai quake is predictable in the Hanteikai scientists' view, owing to special circumstances intrinsic to this particular fault and the historical record of precursory phenomena.30)

There are other problems that have arisen in implementation, however, that have not been resolved. These include the role of the media, and the issue of the "intermediate prediction." The media, following long negotiations, have agreed to give the government thirty minutes from the time the Hanteikai is called into emergency session before announcing publicly the fact that this circumstance has taken place. That will give the government (presumably) time to get emergency personnel in place to handle possible evacuation and other measures. The Hanteikai has developed criteria for emergency meeting and those criteria are such that if they are called into emergency session, the meeting almost certainly will be for the purpose of prediction. The media do not like the notion of a 30-minute moratorium, and are restive.

Another dilemma that has not been fully resolved is that of the intermediate prediction. The Japanese have decided not to issue an intermediate prediction – a prediction perhaps of several weeks or months – in part because it is feared such a prediction may actually cause more economic disruption and public anxiety than policymakers can handle. There may not even be precursors such as to permit a prediction on scientific grounds. However, no one knows exactly what will happen, and the expectation on the part of the media is that the behavior of the Hanteikai (frequent meetings at irregular times) and policymakers will tell them when an unannounced set of intermediate precursors has been detected. What does the media do? Will there be leaks? This is a no-man's land of science, technology, and public policy and one with which the Japanese are still grappling years after the passage of LECA.

The point to be emphasized is that policymaking for an emergent technology presents novel and perhaps unresolvable problems. Having set upon a new course, Japanese scientists and policymakers have the problems that go with addressing frontier issues. The Chinese government has apparently not been bothered by the disruption caused by intermediate predictions. But this is another matter entirely for the government of a complex industrial democracy with strong (and also vulnerable) economic interests. Japan will be first to find accommodations to the potential issues, or provide the initial demonstration to the world that there are no satisfactory solutions to some problems. That is the dilemma of being first.

The United States is not the pioneer in earthquake prediction technology. The 1977 policy gave it the option of playing that role, but neither scientists nor policymakers were ready for the burdens such a role implied. The most important reason is that the threat of an imminent great earthquake in California has subsided. This does not mean it subsided in an objective, scientific sense. It means it subsided in a subjective, political sense. The Palmdale Bulge had triggered concern all the way up to the President in 1976. By 1980, however, people (including policymakers and scientists) were growing more accustomed to it. Scientists were noticing changes, but still did not know what those changes meant.31) The USGS earthquake hazard watch was not withdrawn. What was happening was that a potential emergency was becoming a long-term problem with great uncertainty.

In 1980, the reality of the long term threat was underlined by a report issued by FEMA, which was based on work done by USGS and the National Security Council. The FEMA report said that there was a better than 50% chance of a catastrophic (8.3 magnitude) earthquake in southern California within 30 years.32) This translated into a 2 to 5 percent annual probability. The great southern California earthquake would be the worst disaster to hit the U.S. since the civil war, according to the FEMA report.

The nature of the problem was unprecedented in America. Perhaps as many as 13,000 people could die from a San Andreas quake. It was a frightening prediction. But it was not a short-term prediction. It was long term. The sense of urgency present in Japan was not present in the United States. In Japan, scientists and policymakers behaved – and even spoke – as though the great Tokai earthquake could occur tomorrow. In the United States, that behavior has reflected an implicit consensus that the great southern California earthquake will not do so. It may be that Japan is right and America wrong; or America right and Japan wrong. The fact of the matter is that the United States has implemented the 1977 NEHRA in a leisurely fashion. There has been progress on both the prediction and preparedness fronts, but it has been incremental and disjointed.

The 1977 act had implied the technology was "ready enough." One year later, the Office of Science and Technology Policy (OSTP) said in a report on implementing NEHRA that: "Not enough emphases can be placed on the fact that for the next several years, at least, earthquake prediction will be primarily in a research and experimental phase." At the same time, OSTP said: "The occurrence of short-term precursors can lead to a reliable warning just prior to the event, making it possible to take action to reduce losses."33) There were obviously two paths that beckoned: (1) the R&D; and (2) the prototype demonstration of an operational system. With enough funds, both paths could be taken at the same time, as the Japanese were doing. In the U.S., however, the money for doing that was not there.

The USGS earthquake prediction budget was well below the $19.4 million the USGS-NSF study of 1976 had said was the *minimum* for a viable program. Worse, the coming of the Reagan Administration led to cutbacks in the USGS budget.

The USGS earthquake prediction effort, then $16 million, was set to be cut in half in FY 1982. Furthermore, signals were given that the entire program might be killed the next year. This threat was removed following a considerable outcry from scientists and appeals to Congress and the Executive Office of the President. USGS wound up getting $13.9 million in FY 1983, and the budget has been relatively constant since then. USGS has found itself caught between pressures from scientists who wanted to do R&D and at least some legislative policymakers who wanted operations.

The U.S. Senate Committee on Commerce, Science, and Transportation feels that implementation "of a system . . . for predicting earthquakes" under NEHRA should proceed. The committee stated in 1982 that it "believes that the Congress and the administration need to make a definite commitment to a prototype prediction system in one or more high risk areas by FY 1988. Such a system could provide short-term warnings of an impending earthquake, save innumerable lives, reduce injuries, and possibly reduce substantially the property damages from a major earthquake."34)

What USGS is doing – perhaps to get ready for that FY 1988 decision point cited by Congress – has been to launch what it calls the Parkfield Prediction Experiment.35) This focuses scientific effort and instrumentation on a section of the San Andreas fault midway between San Francisco and Los Angeles. The experiment is named after a small town (population 34) adjacent to the fault. Small quakes strike Parkfield fairly regularly, approximately every 22 years. It is expected that the next Parkfield earthquake will strike in 1988 or thereabouts. Parkfield thus provides an opportunity to test equipment and concepts in much the way Matsushiro served the Japanese. It is a way to test the feasibility of what scientists currently know and can do. It is also a potential confidence-builder for the big step to operational demonstration – the prediction of a destructive quake in southern California.

USGS is studying what it would take to develop a prototype operational system. A recent report calls for 36 sophisticated crustal deformation observatories on or near the San Andreas fault. Current funding has permitted the development of two less sophisticated sites, the one at Parkfield and one at Pearblossom to the south. That leaves 34 to go, along with "the complete revamping of the present seismograph network and it's telemetry line and a quadrupling of the frequency of regional deformation surveying."36) The cost would be $100 million, a great deal of money in the context of present U.S. thinking about earthquake prediction.37) That context is obviously geared to typical small or moderate earthquakes, not the great earthquake that looms ahead. Present incremental decision making is not adequate for the prodigeous leap from one context to the other.

There is another problem in the implementation of NEHRA. This is the lack of program coherence. In Japan, earthquake prediction and preparedness are linked closely at all levels of government. In the United States, this is certainly less true. A structure has been created by FEMA and California government for prediction response, but that structure seems quite independent of the scientific activity led by USGS.

The focus of scientific prediction work is Parkfield, where there are few "people problems" to interfere with the attempt to develop and demonstrate the technology. The focus of prediction response work is the Los Angeles basin, where the Southern California Earthquake Preparedness Project (SCEPP) is active. This effort was established by FEMA and California. A major reason SCEPP was created was to help FEMA fulfill its congressional obligation to produce prototype prediction response plans for areas at risk. SCEPP has found that it labors in an environment in which many, including scientists, fail to take prediction seriously. SCEPP is doing good work in terms of preparedness, in general, but its specific signature (the social innovation it was to introduce) was prediction response. It is hard to elicit a pull from many potential local government and private sector users if there is no push from scientific producers.

There is thus a sense of disjointedness about the American earthquake prediction effort. Indeed, the whole is less than the sum of its parts. A recent report by the General Accounting Office suggests that one problem is lack of coordination in implementing NEHRA generally. Since FEMA has been made lead agency for implementation, it gets a good deal of blame. GAO suggests, in the case of prediction, FEMA indicate what priority this subject should have in the overall scheme of earthquake preparedness.38)

FEMA is a non-technical agency and will (or should) rely on USGS for a judgment. USGS is ambivalent, knowing that earthquake prediction desperately requires more R&D funds, but, on the other hand, it has a public responsibility to predict the next great southern California earthquake, or at least try to do so.39) More money would solve the dilemma of the long term need and (possibly) short-term necessity. But that is unlikely, barring a return of the Palmdale Bulge40) or occurrence of an earthquake more destructive of life and property than Coalinga.

Such an earthquake might be what it takes to congeal the American effort. Even in Japan, it took a strong (magnitude 7) earthquake that killed people in the Tokai district to provide the final trigger for LECA's enactment. That earthquake occurred in January 1978, and LECA became law in June of that year.

Conclusion

The relatively uncertain approach to policy exemplified by the U.S. in earthquake prediction is typical of federal science and technology policy. Indeed, it is typical of American policymaking in general. It is two steps forward, one step backward, a zig, a zag, and a constant question of who is in charge. The nondecision of 1965, accompanied by bureaucratic competition over a non-program, is not unusual. Nor was the remarkably broad and ambitious decision seen in the 1977 NEHRA atypical. That there has been a substantial difference between policy as written in 1977 and policy as implemented in the 1980s is also customary. For those who want to know what policy is, there is the need to study funding follow-through and administrative practice as well as original legislative intent.

The Japanese approach to policymaking may also be revealed in the earthquake prediction case. It is remarkably steady, starting with the scientific plan – the blueprint – in 1962. It has progressed from an orientation favoring research, to one characterized by development, to one favoring demonstration. This program has probably moved much faster than scientists would have liked. The politicians and administrators – the policymakers – have decided that the technology was "ready enough" to move from one stage to the next. Japanese scientists have themselves not entirely discouraged the view that they could predict even though they may not understand the underlying geophysical dynamics. It may well be that there has been more public support for scientific research as a result of the effort to actually predict. Also, the scientists, while bending with political necessity, have done so by choosing the quake to predict. Tokai is a predictable quake, in their view. Their biggest problem at the moment is the pull by certain policymakers to expand the program to a nationwide operational effort. Here, scientists and policymakers so inclined differ as to what is possible and desirable.

Taken as a whole, the Japanese program may be characterized as relatively strategic – goal-oriented, stable, and comprehensive. The U.S. program is sporadic, in contrast. It is sometimes goal-oriented and sometimes the goals are not clear at all. It is unstable, especially from a budget standpoint. And it is disjointed. Given enough money, sporadic approaches to science and technology programs can still succeed. They do so to the extent they can adjust goals to changing political moods. This may be more necessary in the United States than Japan, which has had a relatively stable political setting for its science and technology programs.

A broader implication to be drawn from the earthquake prediction case is that sporadic programs can lean on strategic programs when the former are lagging. It is quite likely that the technology transfer process in earthquake prediction has been a help to the United States. This does not mean that everything Japan is doing in earthquake prediction technology and policy is transferable. However, one country which is giving a field one priority can learn from another nation which is giving the same field a higher or steadier priority.

This is an important point, since many policymakers in the United States are under the impression that the technology transfer process is one where America is the loser in its dealings with Japan. This does not appear the case in earthquake prediction, and probably is not in other fields. It is obvious that nations need to work with one another, since there are not enough resources for each to give priority to every subject. Indeed, the very nature of strategic policy assumes choice, not only in how a program is carried out, but also as to which programs are to be carried out at all, and at what level of effort.

America needs to be less sporadic and more strategic than it has been. It has its own examples of strategic approaches to public programs – mainly in the defense and manned space areas. Where it must be sporadic in policy approach, it had better find a strategic partner. Japan is a good place to look.

NOTES

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2) Robert Lindsey, "Quake Predicting Worries Scientists," *New York Times*, November 22, 1981, p. 25.
3) Dennis S. Mileti, et al., *Earthquake Prediction Response and Options for Public Policy* (Boulder, CO: Institute of Behavioral Science, University of Colorado, 1981).
4) A good review of LECA and Japanese policy is found in Southern California Earthquake Preparedness Project (SCEPP), *Japanese Earthquake Prediction/Preparedness Program* (Van Nuys, CA: SCEPP, 1982).
5) General Accounting Office, *Stronger Direction Needed for the National Earthquake Program*, Washington, D.C.: USGPO, July 26, 1983, p. 36. The Act also calls for R&D. But it nevertheless suggests that the time has come to go operational. The implication from the hearings was that policymakers wanted it sooner than later. NEHRA goes beyond earthquake prediction and includes provisions for other kinds of earthquake mitigation measures.
6) Deborah Shapley, "Earthquakes: Los Angeles Prediction Suggests Faults in Federal Policy," *Science* 192, May 7, 1976, p. 537.
7) James F. Davis and Paul Somerville, "Comparison of Earthquake Prediction Approaches in the Tokai Area of Japan and in California, *Bulletin of the Seismological Society of America* 72, No. 6, pp. S382.
8) Tsuneji Rikitake, *Earthquake Prediction* (N.Y.: Elsevier, 1976), pp. 33–34.
9) Rikitake states that it was unofficially estimated that if a sum of 10,000 million yen (about $30 million U.S.) were spent for promoting the program, that within 10 years' time, it would become possible to see whether or not prediction of an earthquake were feasible. As it turned out, approximately this amount was spent between 1965–1975. Funds started small and went up in ensuing years. This sum pertains to research programs per se. The expenses for salaries for personnel working on the program, for constructing observatory buildings, etc. were covered by other sources. The way Japan computes funds for its earthquake prediction program and the way the U.S. computes funds are quite different, making direct comparisons difficult. Ibid., p. 38–39.
10) Ibid. p. 35.
11) Luther Carter, "Earthquake Prediction: ESSA and USGS Vie for Leadership," *Science* 151, January 14, 1966, p. 181.
12) Ibid.
13) T. Hagiwara and T. Rikitake, "Japanese Program on Earthquake Prediction," *Science* 157, No. 3790, August 18, 1967, p. 768.
14) Ibid.
15) Crustal Dynamics Department, *Activities of the Coordinating Committee for Earthquake*

Prediction (Tokyo, Japan: Ministry of Construction, Geographical Survey Institute, 1982), p. 13.

16) Rikitake, p. 37.
17) U.S. Senate Committee on Commerce, Science, and Transportation, Subcommittee on Science, Technology, and Space, *Hearings on S126,* 95th Congress, 1st Sess., April 19, 1977, (Washington, D.C., USGPO, 1977), p. 22.
18) In 1970, ESSA was reorganized into NOAA.
19) Ibid.
20) Most of NSF's earthquake hazards work was not in prediction. Rather it was in earthquake engineering research.
21) Allen L. Hammond, "Earthquake Prediction: Progress in California, Hesitation in Washington," *Science* 187, February 7, 1975, p. 419.
22) NSF was indirectly contributing to advances in America by sponsoring scientific interactions with Japan.
23) "Earthquake Prediction is Coming," *Mosaic* 8, No. 2, March/April 1977, p. 2.
24) Deborah Shapley, "Earthquakes: Los Angeles," p. 537.
25) NSF/USGS, *Earthquake Prediction and Hazard Mitigation: Options for USGS and NSF Programs* (Washington, D.C.: USGPO, 1976), p. 26.
26) Office of Science and Technology Policy, *Earthquake Hazards Reduction: Issues for an Implementation Plan* (Washington, D.C., USGPO, 1978).
27) Katsuhiko Ishibashi, "Specification of a Soon-to-Occur Seismic Faulting in the Tokai District, Central Japan, Based Upon Seismotectonics," *Earthquake Prediction – An International Review,* Maurice Ewing Series 4 (1981), p. 301.
28) Ibid.
29) Richard A. Kerr, "Earthquakes: Prediction Proving Elusive," *Science* 200, April 28, 1978, p. 419.
30) Davis and Somerville comment that whatever the source of the Japanese scientists' optimism, "it is not premised upon the existence of an already established successful prediction as a precedent, so that it must rely upon some modicum of faith." Davis and Somerville, p. S379.
31) Richard A. Kerr, "Concern Rising About the Next Big Quake," *Science* 207, February 15, 1980, pp. 748–749.
32) FEMA/NSC, *An Assessment of the Consequences and Preparations for a Catastrophic California Earthquake* (Washington, DC: USGPO, November 1980), p. 3.
33) Office of Science and Technology Policy, pp. 32–33.
34) Davis and Somerville, p. S381.
35) Richard A. Kerr, "Stalking the Next Parkfield Earthquake," *Science* 223, January 6, 1984, pp. 36–38.
36) Richard A. Kerr, "How to Catch an Earthquake," *Science* 223, January 6, 1984, p. 38.
37) General Accounting Office, p. 35.
38) General Accounting Office, p. 43.
39) Ibid., p. 38.
40) By 1983, the Palmdale Bulge was apparently no longer present, and there were critics who said it never was a reality. Rather, it was a matter of possible measurement error. Richard A. Kerr, "Does California Bulge or Does it Jiggle?" *Science* 219, March 11, 1983, pp. 1205–06.