This PDF is a selection from an out-of-print volume from the National Bureau of Economic Research

Volume Title: Capital Formation and Economic Growth

Volume Author/Editor: Universities-National Bureau

Volume Publisher: Princeton University Press

Volume ISBN: 0-87014-197-X

Volume URL: http://www.nber.org/books/univ55-2

Publication Date: 1955

Chapter Title: Innovation and Capital Formation in Some American Industries

Chapter Author: W. Rupert Maclaurin

Chapter URL: http://www.nber.org/chapters/c1312

Chapter pages in book: (p. 549 - 578)

INNOVATION AND CAPITAL FORMATION IN SOME AMERICAN INDUSTRIES

W. RUPERT MACLAURIN

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

ECONOMIC growth is so broad a subject that in one sense almost every specialty can be included in it without any effort; yet it does seem fruitful to make a *major effort* to focus the relevant specialties on the factors influencing growth as such. The most serious difficulty is to provide a framework in which the various contributions can be additive.

Since my assignment was as a companion speaker to Usher, I should like to stress my belief that our respective approaches are complementary rather than conflicting.

1. *The Process of Invention and Innovation*

Usher's distinguished book on *The History of Mechanical Inventions*[1] has greatly helped us to understand the process of invention by showing the nature of the continuous stream of new techniques making for evolutionary change. Viewed from a high perspective, the dams and waterfalls in this stream disappear and all that confronts us is a ribbon of water descending steadily toward the ocean.

The process of invention *can* be fruitfully studied from the standpoint of a continuous flow of ideas. Yet it is equally valid to think of the process in terms of discontinuity. In the last 100 years many major inventions have been applications of somewhat earlier breakthroughs in fundamental science. One way of looking at the history of science is to recognize that new conceptual schemes are exceedingly difficult to create,[2] that men capable of creating them are rare, and that there is a random element in the timing of the appearance of such men. It is true, of course, that the introduction of major new conceptual apparatus has frequently been preceded by a substantial whittling away at older concepts. But I do not believe that the process is automatic. An intellectual field may remain dormant for a long period because no man of exceptional vision rises to offer a new and challenging approach.

[1] Abbott Payson Usher, *The History of Mechanical Inventions*, McGraw-Hill, 1929.

[2] Cf. J. B. Conant, *On Understanding Science*, Yale University Press, 1946.

This paper will discuss some case studies of innovation, in order to bring out sharply the nature of technological discontinuities and some of the reasons for them. The material will be examined in an essentially Schumpeterian framework. My main concern is with the pulsating character of innovation and with the entrepreneurial and technological factors leading in particular sectors of the economy to growth, maturity, decay, and regeneration.

I am aware that the term "innovation" lacks precise definition. Yet I prefer it to Usher's "emergent novelty," primarily because I am impressed by the importance of the entrepreneur as a key figure in innovative change and wish to stress the distinction between invention and innovation. The entrepreneurial skill required to introduce a major innovation is rare. Economists have too often concluded that, granted an advance in technology and a strong economic incentive, innovators will automatically appear. On the contrary, as in science, significant opportunities frequently lie available for years, awaiting the right combination of creative vision and action. Then, once the initial difficulties have been overcome, the imitative process can be carried out much more rapidly by men of lesser stature. Later, when the original idea has been fully exploited, there may again be a long wait for new men and new money to advance to another stage.

To my thinking, the Schumpeterian hypothesis linking innovation to long (Kondratieff) cycles in business activity is fruitful for an analysis of economic growth. While we lack adequate statistical data for a long enough period of time to arrive at any proven conclusions, we can say that there have been waves of high investment activity associated with particular fields; and there are factors in the process of invention and innovation that might lead us to expect the wavelike movement to persist.

Thus if fundamental new concepts occur only at intervals, such a basic scientific fact may well affect the pace of technological change. We need to know more concerning the relationship between the propensity to develop pure science, the propensity to invent, and the propensity to innovate[3] before we can conclude that any wavelike force exists;[4] but in the meantime the hypothesis deserves further testing.

[3] See W. W. Rostow, *The Process of Economic Growth*, Norton, 1952, and my article "The Sequence from Invention to Innovation and Its Relation to Economic Growth," *Quarterly Journal of Economics*, February 1953, pp. 97-111.

[4] The reader who wishes to pursue this point further is referred to my *Invention and Innovation in the Radio Industry* (Macmillan, 1949), in which con-

Any analysis of technological change and economic growth needs also to take account of certain shifts in the demand for innovations that occur as the more obvious material needs of society are satisfied and we advance to higher standards of living.

In a mature economy, like that of the United States, opportunities for investment are likely to become increasingly concerned with tertiary industries rather than with primary or secondary industries.[5] By tertiary industries I mean *all types of service industries*,[6] including transportation, community services, education, and recreation. In the last 150 years the proportion of our population engaged in agriculture has steadily declined while the percentage engaged in manufacturing has increased. But we have reached a stage where the manufacturing *proportion* can be expected to diminish.

Today the demand for new technology is distorted by military considerations. But the potential growth of services intensifies the need for organizational innovation and for a technology applicable to the service industries. Successful organization innovations will require the application of human relations skills of a high order. It seems not unlikely, therefore, that in America in the second half of the twentieth century innovating entrepreneurs will be drawn more from the group of men trained as social engineers than, as in the first half of the century, from those with a background in physical engineering.[7]

2. *Some Examples*

Whether or not we accept the concept of long waves of business expansion bearing some approximate relation to a forty-year cycle, the period from the depression of the 1890's to the outbreak of World War II had certain distinguishing characteristics that can be usefully analyzed. In the Western economic world, and particularly in America, this was an era dominated by the growth of the automobile industry, of electric utilities, and of the chemical industries.

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siderable attention is given to the relationship between innovative and imitative entrepreneurship and to the problem of the "cluster effect."

[5] See A. G. B. Fisher, *Clash of Progress and Security*, London, Macmillan, 1935.

[6] Considerably more work could be usefully done to clarify industrial definitions. I include newspapers, television broadcasting, hospitals, helicopter buses, ski tows, theatres, and community swimming pools as "service industries."

[7] Is it too much to hope, as a corollary, that the second half of the twentieth century will also witness the coming of age of the social sciences?

THE AUTOMOBILE INDUSTRY

The automobile industry has certainly been one of the most vigorous new industries in America. Yet by the late 1920's the innovative characteristics of the industry began to change. There can be no simple explanation of this fact. But a partial answer lies in the quality of entrepreneurial leadership, the absence of a research conception, the explosive rate of previous growth, and the success of the established oligopoly.

More than any other figure, Henry Ford typified the automobile industry in its first innovative phase. His principal contribution was in pressing aggressively for cost-price reductions to achieve a mass market. He weighed all technological changes against this objective. A skilled mechanic without professional training, Ford never included fundamental research as part of his program. Nor was it part of the program of his competitors. Therefore, while the automobile industry could perhaps have been preparing during the 1920's for basic innovations, it was in fact not doing so. Henry Ford himself was aging, though maintaining firm control of the helm. Thus he did not see the limitations of planetary transmission until a very costly shift in production was forced on him by his competitors.

More important than the character of entrepreneurial leadership was the fact that there was no strong incentive for radical technological change in the automobile industry in the late 1920's. A sound, efficient, durable car had been produced by the industry and a vast market demand had materialized. Low-cost mass assembly plants had been constructed; a smoothly working network of dealer outlets had been established throughout the country; special credit institutions had been created to finance installment purchases; and the used-car market was well organized. The automobile industry had grown up; the major *new investment* phase was over for the time being.

What was done next could be broadly described as extensions and refinements. More cars required, first and foremost, better roads; and these were provided. Greater riding comfort in the lower-priced cars was added as automobiles were used increasingly by the masses for pleasure, and higher speeds were offered for the wide new roads of the West.

During the 1930's and 1940's, advances continued to take place in mechanical and chemical engineering which made it technically possible to obtain more powerful fuel, more miles per gallon (if one wanted this, and many who turned to European cars did), and longer and more economical use of tires; and there was the possibility, not yet introduced commercially, of a jet engine.

In a dynamic society, technology does not stand still. From this point of view Usher is certainly correct in stressing the continuity of technological change. The creative urge is widespread and the demand for novelty great. But an industry may choose to stress the novelty of style changes, the recreational use of a radio in a car, the comfort of a heater, better springs, and other improvements not requiring a major new investment in capital equipment. By 1930 the American automobile was established as a satisfactory product in terms of mass demand.

Twenty-three years later one can say that there has been comparatively little innovative change. And it is interesting to speculate on what *might* have happened if Henry Ford had died, say, in 1929 and left in control a son with a strong innovative flair. I think that, if he had been as shrewd as his father, such a son would have been torn between a desire to do something fresh and important with the automobile and a realization that the more prudent and sensible policy was to consolidate past gains. Had the desire for "emergent novelty" been compelling, he might well have turned the company over to his associates and devoted his energies to some other field. As a possible parallel, after Paul Hoffman had succeeded in re-establishing the position of the Studebaker Corp. in the automobile oligopoly, he shifted his interests to the Economic Cooperation Administration and the Ford Foundation. It is perfectly possible to lose interest temporarily in the generalship of an enterprise simply because it is running well on its established momentum.

There is in fact an important difference between the life of an inventor and the life of an innovator. An inventor—like Thomas Edison or Lee De Forest or, in modern America, Edwin Land—granted some access to sources of capital, can spend his life moving from one invention to another in a broad field without a major wrench every time he shifts. Thus Land can work successively on polarized light and sun glasses, headlight glare, three-dimensional movies, and a picture-a-minute camera so long as his creative energies are concerned with the inventive rather than the innovative phases. Such activities can be financed by royalty returns on patents or by small-scale manufacturing so long as *some* of the inventions are translated into action. Inaction, as in the case of Land, who was not able to interest the automobile industry in the headlight glare problem,[8] does not need to be frustrating if the inventor feels that he has solved the basic technical problem. But the innovator must succeed through obtaining action; this is much more time-consuming, given the resistances to change which are built into our institutional environment.

I cannot see, therefore, how we can escape from stressing discontinuities when we are discussing innovations. It is not indulging in a "great-man theory" of history to conclude that innovators like Henry Ford are unusual. If the economic environment is propitious, such men may have an opportunity to participate in the creation of an important new field. But when they have done so successfully, they are not *likely* to move into another entirely new field with equal success. More probably they will exploit and develop what they have created.

When, therefore, an innovative breakthrough takes place, it tends to run its course. In modern American industry, dominant innovators do not usually perpetuate themselves.[9] In the early days of a growing enterprise, it is much more feasible to run a one-man show. The dominant figure can be in on all decisions. But with growth and success, men of lesser imagination are required to carry out the original goals. Frequently, control passes to those men who have devoted their lives to straightening things out. Such people get tired of too much change. Even if there is no such delegation of control, efficient management requires consultation, which is more time-consuming as the business becomes more complex and which inevitably slows up bold decisions. If, as occasionally happens, there is a struggle for power, the loyalties of the organization tend to group around the mediators rather than the disruptive forces. The wise innovator, therefore, frequently finds that after a certain stage of successful achievement he will get more satisfaction from devoting his major creative energies to outside activities. This is perhaps an explanation of many of the extracurricular activities taken on in middle and later life by successful businessmen. As another type of reaction, C. F. Kettering, on retiring from General Motors, became far more active in research than he had been for years.

There are thus several reasons to expect that a major innovation

[8] An example, incidentally, of the lack of research-consciousness in this industry.

[9] Nepotism is frowned upon, and there are rules against it in many large enterprises.

like the automobile will have an explosive expansionary effect that will later taper off.[10] A research conception which was not part of the intellectual scheme of the automobile industry might help to maintain the volume of new investment. If the automobile pioneers had believed in and understood research, they could have provided a more interesting innovative record since 1930. But I do not believe that research would have provided a major change in the investment pattern simply because the automobile was such a success without much real research.[11] More light, however, can be thrown on the possibilities for continuous innovation if we examine the record of an industry that sprang from science and that has placed important emphasis on research from the beginning—namely, the electrical manufacturing industry.

ELECTRICAL MANUFACTURING

The development of central power stations and the invention of the telephone have had as profound an effect on industrial development as has the automobile's rise. Partly because the technological discoveries involved were more recent and more complex than in the case of the internal combustion engine, the electrical manufacturing industry[12] and the telephone industry have maintained a much closer link to science than the automobile industry has. These ties also developed partly in response to the interest of some of the scientists who joined the first two industries. Professor Elihu Thomson founded the Thomson-Houston Electric Manufacturing Co. in Lynn, Massachusetts in the 1880's. This concern became a key element in the General Electric Co. when it was founded in

[10] Concerning Siegel's comments on this point at the conference, I do not wish to imply that there is no future for the automobile industry. A giant corporation has great power to perpetuate itself, especially if it uses creative research as a method of regeneration. But for the reasons that I have stated, I believe that a pulsating force in its investment growth is much more probable than straight-line growth.

[11] To date, the research undertaken by the automobile companies has been aimed at the engineering improvement of existing products rather than the creation of new products. For classification purposes I prefer to call this type of work "advanced engineering development," reserving the term "research" for more fundamental inquiry without such immediate, practical results in mind. There is some evidence that the automobile industry is very slowly reaching a comparable assessment of its research role and that important changes in this respect will occur in the next decade.

[12] For reasons that remain somewhat obscure, the *electric power companies* and the *telegraph companies* did not keep nearly as close to science. An exception today is the active research interest of the Detroit Edison Co. in atomic energy.

1892. Thomson remained as a consultant of the new company. In 1900 General Electric decided to establish a research laboratory at Schenectady devoted to "fundamental scientific inquiry," and Professor Willis R. Whitney was appointed the first director. It has become one of the leading scientific laboratories in the United States, with many distinctions, including that of having a Nobel prize winner—Irving Langmuir—as one of its full-time staff members.[13]

General Electric was responsible for many of the advances that took place in the electric lamp from 1900 to 1940. The patents acquired from these advances, plus outside patents which were purchased, gave the company undisputed control of the electric lamp industry. With decreasing costs through mass production and steady improvement in machine design, General Electric was able to make very comfortable profits on the incandescent lamp throughout this period. In fact, the lamp division was probably the most profitable division of the entire enterprise and carried a considerable proportion of the total research expenditures for the company.

With this background and with the acquisition for its laboratories of such notable figures as Irving Langmuir and W. A. Coolidge, one might have expected General Electric to be ready in the late 1920's with a new form of lighting which would capture the public imagination. Technically this was possible, for the fundamental work on the fluorescent lamp had already been done in Europe by that time. But the propensity to innovate was not there. General Electric was making profits in incandescent lamps; the lamp was satisfactory; the company had patent control; everyone in the industry paid royalties. This scarcely provided the environment out of which one would expect a rapid change.

Possibly, also, one might have expected General Electric or Westinghouse, both of which were supporting fundamental scientific research, to pioneer in atomic energy development. Both companies had a major stake in the manufacturing of power equipment for central stations. Might they not have invited a Niels Bohr or a Fermi to work in their laboratories on atomic power?

But the commercial prospects for atomic energy seemed and were still a long way off in the 1920's and 1930's. The leaders of General Electric, Owen Young and Gerard Swope, who had been responsible for the recent growth of the company and who were in control

[13] The only industrial laboratory in the United States sharing this honor is that of the Telephone Co.—through C. J. Davisson.

in this period, had made their mark and acquired comfortable fortunes. They were, quite properly, more concerned with the future stability of the enterprise than with taking major risks. It is probably too much to expect a highly successful business concern producing satisfactory products to jump way into the future, even though the company is research-minded. The business philosophy of General Electric and Westinghouse was to keep close enough to science to be somewhat ahead of the times.

In the words of Owen Young: "Fifteen years is about the average period of probation, and during that time the inventor, the promoter and the investor, who see a great future for the invention, generally lose their shirts. . . . This is why the wise capitalist keeps out of exploiting new inventions and comes in only when the public is ready for mass demand."[14]

Even with far-sighted management, which I believe General Electric had in the 1920's and 1930's, one has to expect a certain myopia. It is hard to take such a broad view of one's field that you anticipate its being undermined.[15] In my own institution, the Massachusetts Institute of Technology, no major contributions were made during the 1930's to the exploration of nuclear physics (apart from developments in instrumentation).

In the General Electric case a research conception did not lead to any major innovative change in lighting until the fluorescent lamp was introduced at the end of the 1930's. This was about the time when the last of the basic incandescent lamp patents were expiring. Competitively, General Electric had been in a position to take its time about such a development. Each concern in the industry operated under a quota. Patent control was so complete that it was virtually impossible for any *new* concern to enter the industry. The one lighting company which eventually broke away from General Electric and did some independent work on the fluorescent lamp (but not the original work) was Sylvania Electric, whose growth was slow and took place initially under the General Electric umbrella. By merger and by acquisition, Sylvania eventually obtained a 6 per cent quota of the total incandescent lamp business; after that there was nobody else to absorb. It was only

[14] Maclaurin, *Invention and Innovation in the Radio Industry*, as cited, p. 88.

[15] For example, Western Union, in the period when it was the most powerful communications company of its day, turned down the opportunity to purchase the telephone patents of Alexander Graham Bell.

through the assistance of the Department of Justice that Sylvania was able to break out from the quota system in about 1940.

It is, of course, impossible to *prove* that a new development such as fluorescent lighting could in fact have been introduced in the late 1920's instead of the late 1930's. But the basic technology was known, and it is my own belief that, if the entrepreneurs in control of the industry had had their full competitive vigor at the end of the 1920's, there was no reason why they could not have "ordered" a new type of lighting. We have reached such an advanced stage of engineering that, if the basic research has been done, many inventions *can* be made to order.[16] In this sense the process of invention is no longer subject to the caprices of the lone inventor. Research teams can be assembled from professional engineers, and, with some reasonable leadership and adequate budgets, a limited objective such as a change from incandescent lighting to fluorescent lighting is largely a function of management. My conviction is, therefore, that the petering out in the process of creation that was evident in the General Electric and Westinghouse laboratories beginning in the late 1920's was due partly to the fact that, as in the automobile industry, the companies had had a long run of successful years and their established leaders were more immediately concerned with the protection of past gains than with rushing into new developments.

I do not wish to imply that the petering out which I think I have observed is likely to be permanent. There are too many dynamic forces in the lower layers of a great enterprise to make this probable. Nor do I believe that a wavelike movement is *inevitable.* A full discussion of this point would lead us into the difficult realm of "succession of management"—a problem to which comparatively little articulate thought has been given. In specific terms, the question of why the quite exceptional top-management team of Young and Swope did not replace itself would have to be answered.

One other point that should be stressed here is the increasingly high cost of new technical developments in well-established fields. When technology becomes more advanced, it is normally no longer possible to make pioneering inventions with simple technical equipment and inexpensive apparatus. Edison was able to invent and develop to a commercial stage the original incandescent lamp with equipment that, by present standards, would be considered exceed-

[16] The wartime developments of radar and the atomic bomb are cases in point.

ingly crude. But the work of Coolidge and Langmuir in perfecting the lamp through improved filaments and gas-filling required much more elaborate experiments and much costlier and more precise equipment.

Moreover, in order for the fluorescent lamp to compete with an existing product that had already been perfected, it was essential that the new lamp work well. Large sums of money had to be spent on painstaking experiments with fluorescent powders, on the problem of end-blackening, on the elimination of flicker, and on instantaneous starting before the lamp was an assured commercial success. This meant that the propensity to innovate was dependent on the possession of a large research budget. What eventually happened was that Sylvania used the fluorescent lamp as a means of breaking out from under General Electric leadership, although General Electric did the major development work on the fluorescent lamp. And General Electric easily won its suit against Sylvania on fluorescent lamp patents. In fact, with the exception of Westinghouse, none of the licensees in the lamp industry had the resources or the basic technical skill to undertake a pioneering breakthrough of this type *on its own.*

Moreover, to help spread the use of higher-powered incandescent lamps and fluorescent lamps, General Electric and Westinghouse found it necessary to spend very substantial sums of money over the years in institutional advertising of the "better light—better sight" variety.

To summarize, then, from the General Electric experience, research, as one would expect, gave the company an opportunity to maintain a higher level of new investment than would otherwise have occurred. But from the standpoint of the economy as a whole, the most significant new investment opportunity lay in a fundamental change of energy source from steam power stations to atomic power. Since such a development is *not yet ready*, it is easy to see why a business enterprise would not attempt to push too far ahead of commercial prospects. The sums of money required for atomic energy development are so vast and the military applications so direct that government sponsorship is essential. In retrospect, it seems unlikely that the power companies and the electrical manufacturing concerns, even had they wanted to, could have pushed atomic energy development sufficiently to provide a continuous innovative investment stream.

On the more limited basis of providing investment in new types of lighting, there were better prospects for continuity. But the patent position of the dominant concern—General Electric—and the personal goals of the key entrepreneurs in the firm did not induce such a change until the time was fully ripe.

We are thus faced with an economic dilemma. Patents and monopolistic position are essential ingredients in providing financial support and incentives for large-scale research on and development of complex new products. A research-minded company has to work exceedingly hard during the rapid growth period of a new art to build up a watertight patent structure. Having done so, it is not consistent with normal economic motivation for it to throw away the advantages of the monopoly by forcing a very rapid rate of obsolescence.

We can see this same problem in another example taken from the electrical revolution—the radio and television industry.

RADIO AND TELEVISION

In the radio industry there were more competitors than in the electric lamp industry. But by 1927 the Radio Corporation of America had obtained undisputed patent control, and thereafter every radio-manufacturer in the United States had to obtain a license from and pay royalties to this concern. RCA conceived of itself as performing the centralized research function for the entire industry. Substantial license fees were charged, and the announced plan was to plow back these royalties into further research.

A somewhat younger industry than lighting, commercial radio in this country nonetheless dates back to the formation of the American Marconi Co. in 1899. This concern, which was controlled from England, was bought and absorbed by the newly formed Radio Corporation of America in 1919. Owen D. Young of General Electric was the principal entrepreneurial figure behind this organizational innovation, with the stock being shared originally by General Electric, Westinghouse, and the Telephone Co., all of which in effect pooled their radio patents in RCA. And since the concerns which dominated radio research were these same three, the key entrepreneurial figures were men who had made their marks by the late 1920's. There has since been a revolt against this domination, involving new men and newly made money. This revolt, however, led by such concerns as Philco, Zenith, and Admiral, has taken over twenty years to develop. During this period, from about 1930 to 1950, these newer concerns were primarily concentrating on methods of breaking down the RCA licensing procedures; their innovative activities were focused on sales and distribution methods or, as in the case of Sylvania, on low-cost manufacturing of tubes rather than on research and invention. The fact remains, however, that the history of the most important invention and innovation in the radio industry in the last twenty years, television, is primarily a history of research by the large established concerns—the Telephone Co., General Electric, Westinghouse, and RCA.

The most significant aspect of television research was the high expenditure for technical development required before *any commercial returns* were received. The Radio Corporation of America spent $10 million for research and development in the precommercial stage. The role of government regulation was very significant here. New channels had to be obtained; and regulatory commissions were gravely concerned over their responsibility to protect people from finding their sets obsolete because of later developments. The budget for television development had to come from the royalty receipts from the licensees, from the profits from broadcasting, and from the direct manufacture of sets and tubes. In such a situation, until an established product has been fully exploited—and this takes a good many years—it may very well not be to the economic interest of a concern with a dominant position to push too aggressively with a new product which renders its current products obsolete. Technologically, commercial television could have been introduced in 1930, but management was not ready then to provide the necessary support.

The paradox of technological change and investment lies in the fact that the basic shift toward service-type industries is requiring increasingly expensive preparatory work with government agencies. Second, research and development are becoming big business in themselves, and require ample budgets. Under these conditions, the concern which has not achieved a monopolistic position through a differentiated product or through patents finds it difficult to pioneer in innovations. Though many of our great modern corporations do "create what they later exploit," I have found no evidence yet of any large concern which consciously attempts to use research and innovation as a means of maintaining an *even flow of new investment.* Nor can I see any reason why new firms can be counted on to emerge at the right historical moment.

3. *The Next Long Cycle*

Though we may be straying into the realm of prophecy, it may help our analysis if we speculate about the long cycle in which we are now living in America. Let us assume for the purposes of discussion that the main upward thrust of the present cycle began about 1940 and was greatly stimulated by the rush of new technical developments emerging from the war. What industries can we expect to carry us during the 1950's and the 1960's? And what part is technology likely to play in such an upward phase?

While we could expect some continuation of technological change and new investment in the industries that carried the last major investment wave—electric power, chemicals, and the automobile—it does not seem probable that these industries will provide new outlets for investment on anything like the scale that they created in the last cycle. In a broad sense, these fields would appear to be somewhat exhausted. Although the most important new development in electric power is atomic energy, a great deal more work will have to be done in reducing costs before atomic energy is likely to displace our existing electric power stations. Servomechanisms, on the other hand, may result in an important new investment in automatic electrical machinery in our factories.

The revolution in chemistry and the rise of the chemical industries contributed to the last upsurge and can be counted on again in the next two decades. We have still not exploited all the possibilities for creating new products which represent chemical substitutes for natural processes.

More promising from the standpoint of capital consumption is the provision of better community services. Our metropolitan areas today are mostly obsolete. I should like, therefore, to discuss the investment opportunities provided by urban redevelopment in the light of modern technology. Since transportation is a key feature in such development and commuting distances are reaching the breaking point, let us also examine the prospects arising from the technology of the aviation industry. The *logical* next step in transportation from the city to the country lies in aviation rather than in the automobile, though we can certainly expect the continuation of a major investment in new construction of roads. But revolutionary change would come with the development of large-scale private flying. So let us consider the investment opportunities provided by the airplane.

THE AVIATION INDUSTRY

Is technology the principal factor inhibiting the growth of private airplane use? From what I have been able to learn from scientists and engineers in this industry, I believe not. The basic scientific work has been completed, making it possible to fly with safety under most weather conditions in either a helicopter or a helioplane requiring a short take-off space. But much work still needs to be done on reducing instrumentation costs, making piloting easier (especially in a helicopter), and providing landing fields before a mass market can develop. Without the possibility of a mass market, assembly line economies are not possible.

What, then, are the bottlenecks? The aviation industry is now controlled by men who have made their fortunes and built up substantial enterprises manufacturing large commercial transport planes and military planes. These markets have very different requirements from that for the private plane. High speed and durability are essential; original cost is comparatively unimportant. In consequence, there has been very little development work by the large, established manufacturers[17] on low-cost private planes. In fact, because of a cost-plus approach and the rapid change of military models, it is questionable whether the background of the main aviation industry provides a suitable environment for such work.

A secondary factor is that since aviation is closely regulated and since there is opposition to every new landing strip, large sums of money need eventually to be spent in building feeder airports, creating parking facilities for private planes at such airports, and getting public acceptance of the private plane in the surburban backyard.

It is my own belief, therefore, that whether or not this type of development comes in the next twenty years is primarily a function of innovative leadership. From the group of small, obscure concerns now producing helicopters or light planes, one or two entrepreneurs may emerge with the vision and energy of a Henry Ford. Such entrepreneurs could take advantage of the vast sums that have been spent on military research in aviation, but their principal difficulty will be in building an organization and overcoming resistance to change. The large capital resources of the well-established company would be a great advantage, but the new risk-taking approach probably requires new ventures.

[17] Such as Douglas, Boeing, Lockheed, and United.

If we accept the probability that new men and new money will be needed for this new type of industry, we must also recognize the fact that the launching of such ventures is more difficult today and the obstacles to be overcome are more persistent than they were forty years ago. One of the prices of SEC regulations, public safety measures, etc., is that entrepreneurial innovation becomes much more difficult. Perhaps it would be more accurate, however, to say that the process becomes different and that different skills are necessary. In complicated fields which are tinged with the public interest, organizational innovation is the most serious bottleneck in holding up the process of translating technological change into significant new products.

METROPOLITAN REDEVELOPMENT AND HOUSING

Similar obstacles beset one of the other major industries which might carry our next long wave—the housing industry.

The automobile, supplemented soon perhaps by the airplane, makes it *technologically possible* now for most Americans to live in a functional house which is well engineered for a servantless society and which provides the beauty and restfulness of the country. The basic research has all been done. The first requisite was to break down the traditional approach to housing and re-examine dwellings in the light of modern technology. Such men as Le Corbusier and Frank Lloyd Wright were among the pioneers. With the development of electric household devices, of lower-cost automatic heating facilities, of new methods of inexpensive glass production, and of new materials for interior wall construction, an inexpensive housing product became technologically possible and the general acceptance of it would represent a very significant improvement in mass living habits.

But the missing link again, I believe, is organizational innovation. Let us examine some of the obstacles that have to be overcome.

Companies specializing in prefabricated houses are not likely to solve the problem. A very small proportion of the cost of housing is in the shell; the economies obtained by mass production of panels not only are negligible but are often outweighed by the diseconomies of transportation and high overhead costs. A house, far more than most products, is very much at the mercy of local influences—local planning commissions, zoning ordinances, and building codes are key factors.

The large, established enterprises in building are component manufacturers such as Johns Manville, Revere Copper & Brass, American Radiator, and United States Gypsum. These concerns are in oligopolistic positions where they make comfortable profits, and because no one of them contributes more than a very small proportion of the total cost of the house, the demand for many of their particular products is inelastic. One cannot count on very much from such companies, therefore, in the way of radical cost-cutting improvements.

The organizational innovation that appears necessary is the emergence of some house-assembling companies of national stature who will in time develop sufficient power to control and direct community development wherever they operate. There have already emerged from the chaos of the individual-house-building industry a few enterprises of regional stature—such as Levitt & Sons. But it is very uncertain whether these essentially local concerns—successful in serving their particular communities—can grow beyond this. Most house-building concerns lack the capital resources or the management skills to serve more than a local area. American industries have characteristically grown through branch plants. But this growth has not normally had to contend with the exceptional strength of local influences in housing. Housing enterprises also have had to weather the high degree of seasonality and cyclicality of the industry, which has meant that many attempts at mass production have led to bankruptcy through high overhead costs and inadequate capital.

It may well be that the housing industry will have to grow very slowly, first through the emergence of a few local innovators with vision and then by merger of the more vigorous elements on a regional and national scale. I believe, however, that to develop the full investment potentialities of the industry, an organizational innovation not now in existence and different from that in most other industries will be required. The men who are to provide the leadership for such an innovation will have to understand the nature of the challenge. If they do so, the necessary technology for significant reduction in costs will be at their command.

4. *Conclusions*

By discussing the process of invention and innovation in particular firms and particular industries, I have tried in this paper to indicate that:

1. Invention and innovation do not go hand in hand, and there is no predictable time lag between them.

2. Basic breakthroughs in the physical sciences leading later to revolutionary changes in technology are discontinuous.

3. Due to the infrequency of the appearance of innovators, the comparative ease of imitation, and the resulting tendency for entrepreneurial activity to come in clusters, innovations are likely to peter out when the key figures associated with them have carried their original ideas to their logical conclusion.

4. The emergence of the large corporation with a research organization suggests that regeneration is more probable in corporations today than it was 100 years ago.

5. The initiation of major innovations usually requires new men and new money—a requirement which is likely to be met by new firms. I believe that the "new men" are much the most troublesome part of this sequence. In completely new fields it is relatively easy to launch a small new firm; and, while new methods are necessary to obtain capital for new ventures, the right man can usually find the money.

6. For the last 100 years at least, technology has had a tendency to outrun innovation, so that at any given point of time the possibility of applying technology to the introduction of new products has greatly outdistanced the actual performance. In fields dominated by large, established corporations the problem of succession of management is to place new men at the top with innovative vigor equal to that of their immediate predecessors.

7. As higher standards of living bring about a shift from primary and secondary manufacturing to tertiary or service-type industries, organizational innovations become increasingly important.

8. Since it is apparent that innovating entrepreneurs are rare and that needed skills are changing in character, much more careful study could profitably be given to the nature of those skills and the proper environment in which to train men in them.

COMMENT

Walter Isard, Massachusetts Institute of Technology

In discussing Usher's paper I shall dwell upon its implications (and those of his works in general) for the study of economic growth. For I believe that he points up a basic set of considerations which thus far has not received adequate attention.

Usher views the historical record as a basic interaction of technology and resources. The advance of technology, and more specifically its introduction into our society in the form of innovation, have proceeded not randomly but continuously, at times at accelerating rates, at times at decelerating rates, and even at times at rates approaching zero which have presented plateau periods.

At any given point of time an innovation occurs at one place, or at least at a relatively few places, in the world. Its spatial diffusion over wide areas of the world proceeds also at accelerating, decelerating, and even zero rates. Any advance in technology constantly revalues the resources of any particular area, so technological progress over time constantly changes both the absolute and the relative *productivity potential* of each area, since each has a unique resource endowment.

However, technological progress is a social process. It is not divorced from society; in fact, it is much influenced by the values of any given society. The direction and rate of technological progress in any given area and the rate at which innovation successful elsewhere is accepted are significantly affected by the cultural traits and patterns of the people of that area. Among those cultural traits and patterns, the economic institutions, institutions regarding property ownership, monetary and financial institutions, and practices of business organizations are of major significance in their effect on technological progress and the acceptance of progress. Hence, in studying economic growth and capital formation, we confront a complex interaction of social, political, economic, and geographical factors, as Kuznets has so neatly argued.

But no matter how much we emphasize entrepreneurial attitudes, community relations, financial institutions, saving habits, and the like, there is still a basic play of technology upon resources—resources which differ from area to area and which give these areas different productivity potentials at any given point of time.

Let us now leave this world of generalities and become more concrete. Let us consider the papers by Kuznets and Goldsmith. A wealth of interesting, though tenuous, statistical material was presented. A number of interesting hypotheses emerged. But it seems to me that before one can reach any meaningful conclusions about economic growth from these materials one must build around them a perspective in both time and space. One must put these data against a background which for key points of time in the past, given the state of technology, yields the different productivity potentials existing in the various areas of the world. One cannot escape the fact that the existence of different productivity potentials greatly influences the rate at which the areas can accumulate capital, or their ratios of capital formation to national output, and so forth. I wish we had before us a table on the resource endowment of different parts of the world—a table such as Usher has developed or such as that in A. J. Brown's pamphlet on *Industrialization and Trade*.[1] It would show very striking inequalities in the current resource positions of the various areas. No one, I am sure, would deny that these major differentials, given our existing technology, greatly influence the rates at which the various nations can accumulate and invest capital today. And so it has been at every point of time in the past. My point is that in order to utilize efficiently the data which Kuznets and Goldsmith have developed one must consider the historical impact of changing technology upon different resource potentials, along the lines that Usher suggests in his paper and in some of his other writings.

Let me proceed to another concrete illustration. The discussion earlier in this conference centered around the entrepreneur and the conditions which influence entrepreneurial action. There was much talk about comparing social conditions and organizations of various kinds and their associated national rates of growth—comparing those in France with those in Germany, or with those in England or Japan. I heartily endorse comparative analysis—but only of the right kind. Once in a while one heard at the conference the word "resources," in the general context of "mineral resources." But there did not seem to be any recognition of what I would consider a rather elementary fact, namely: *A given difference in social conditions* (assuming that we can isolate and agree upon that difference) *will yield one set of discrepancies in growth rates among nations when resource or productivity potentials are the same, and a significantly different set of discrepancies when resource or productivity potentials are significantly different.*

Let me be more specific by referring to Hoselitz's paper. He states: "Compared with Britain, France exhibits a pattern of retarded economic growth and industrialization. Yet in the history of the two countries there was a time when there were few apparent differences in the over-all productivity of the various factors, and

[1] London, Royal Institute of International Affairs, 1943.

when technological procedures and general economic organization appear to have been on a fairly even level" (page 294). After considering differences in raw materials endowment and geographical position, he concludes that to explain the differences in rates of growth between Britain and France, "the decisive factors have to be sought rather in their respective social environments" (page 295). I may be misinterpreting Hoselitz, but I feel that at least this is the viewpoint of many who have joined in the discussion at the conference.

In attempting to prove that the French resource position for the basic iron and steel industry was as good as the British, Hoselitz presents some materials on ores and imports of iron into both Britain and France. I do not wish to quibble about the data for they are not the important ones. With respect to the iron and steel industry during the eighteenth and the first half of the nineteenth century, *coal resources, not ore resources,* were the most strategic. This is obvious when one considers that from 7 to 10 tons of coal and only 2 to 3 tons of ore were required for 1 ton of malleable iron or steel. The importance of coal emerges conclusively from the actual figures on malleable iron and steel production, which show that modern iron and steel furnaces have been oriented to coal sites rather than ore sites.

To repeat, coal was the most strategic factor in iron and steel location. And with respect to coal, given the techniques of the eighteenth and nineteenth centuries, the British position was far superior to the French. One cannot deny that this was an important factor in the differences in their rates of growth.

I do not want to be misunderstood and accused of being a geographical determinist. I do not wish to minimize the entrepreneurial and general cultural factors; they are very important. If French entrepreneurship had been more vigorous, techniques of blending coal and of treating inferior coals to make them more useful could have been worked out so as to improve France's inferior coal position. Nonetheless, to some extent at least, the difference would have persisted.

To conclude, what I am urging is that we should not underestimate study of the impact of changing technology upon resource patterns, much along the lines that Usher has pioneered. If the economic historian or economic development theorist is to make headway in attacking the entrepreneurial or the capital formation problem, he must develop a framework which recognizes that the historical data for diverse regions reflect the compounding and interaction of numerous social, cultural, and institutional differences as well as differences in resources and productivity potentials.

IRVING H. SIEGEL, The Twentieth Century Fund

1. The Possibility of a Framework. In his opening paragraph Maclaurin makes a passing reference to the fact that "The most serious difficulty" in the study of factors influencing economic growth "is to provide a framework in which the various contributions can be additive." I want to comment on this important subject first.

Since the study of economic growth is in a significant sense a branch of history, a search for a unique framework (or for alternative frameworks really congruent with each other) seems destined to fail. The history of history further suggests that a word like "progressive" is preferable to "additive." Earlier contributions sufficiently prove their worth when they stimulate the formulation or facilitate the testing of new hypotheses, when they encourage or aid the reinterpretation of given data from new vantage points.

Historians have never agreed upon a unique framework, and students of growth likewise approach their own many-faceted subject with a proper variety of preconceptions and interests. Investigators of growth are bound to have different preferences concerning (1) the unit of observation or the level of inquiry, (2) the system of categories for which data should be accumulated or hypotheses formulated, and (3) the amount of data which ought to be collected prior to the design and testing of hypotheses.

As for the first point, growth obviously has its "micro" and "macro" levels. Thus a researcher may wish to concentrate on the origin and vicissitudes of a *principle, process,* or *product*; or on the life and work of a key *individual*; or on the evolution of a *firm* or *industry* as a quasi-organism; or on the changing role of *government*; or on the economic development of one or more *countries.*

As for the second point, opinions will naturally differ on the categories which are most convenient, relevant, or fruitful for the analysis of growth. Thus the selected categories may emphasize the critical role of certain human bents or modes of possible behavior—like the so-called "propensities" of Rostow[1] and Maclaurin.[2] More

[1] W. W. Rostow, *The Process of Economic Growth,* Norton, 1952.

[2] W. Rupert Maclaurin, "The Sequence from Invention to Innovation and Its Relation to Economic Growth," *Quarterly Journal of Economics,* February 1953, pp. 97-111.

often, the lists of growth determinants also explicitly include significant nonhuman categories. Abramovitz, for example, mentions five broad categories—the supplies of the factors, the attributes of the population, economic organization, legal and political institutions, and provisions for acquiring and using knowledge.[3] Many other students, like J. M. Clark, Clough, and Moulton, offer somewhat similar lists; and Spengler presents a much more detailed one.[4] Still another approach is exemplified by the mathematical-model-builders, who are satisfied with a small number of quantifiable aggregative variables for studying the behavior of the whole economy or of a major sector. A much more popular quantitative approach emphasizes trends and relationships among time series. This one is typified by the work of most government and private research organizations and of such academic students as Colin Clark.

As for the third point, the inseparability of fact and theory should be obvious, whatever the tactical emphasis at the outset of any investigation. Nevertheless, debates on this matter still erupt. The recent controversy between representatives of the Cowles Commission and the National Bureau of Economic Research on the roles of empiricism and hypothesis covered essentially the same territory as an exchange almost a generation earlier between Lowe and Mitchell.[5]

While no pseudo-Kepler is likely to arise and persuade us to adopt a unique framework, we must nevertheless recognize that modern conditions favor the revival of "laws of history," of agreeable evolutionary or dialectical "stage" theories of development, of streamlined versions of the "historicism" against which the Poppers and Hayeks still declaim. In a tense world well wired for sound, in which deliberate growth is the order of the day, more attention is likely to be paid to the originator or propagator of a bold, grand, oversimple, mathematically grounded hypothesis than to the scholar who is content to "chronicle small beer."

Maclaurin's paper, like his well-known earlier work, deals essentially with "micro" phenomena, with the role of key individuals or pivotal firms in the growth of particular industries. From studies

[3] Moses Abramovitz, "Economics of Growth," in *A Survey of Contemporary Economics*, Bernard F. Haley, editor, Irwin, 1952, pp. 132-178.

[4] Joseph J. Spengler, in *Problems in the Study of Economic Growth*, National Bureau of Economic Research, 1949.

[5] *Business Cycles: The Problem and Its Setting*, National Bureau of Economic Research, 1927, pp. 59-60.

of such cases he seeks to distill ideas concerning the determinants and the character of growth in general. His categories are non-quantitative. Elsewhere I have had occasion to endorse his literary case-study approach as a necessary element of an eclectic research program.[6]

2. *Discreteness of Innovation.* Maclaurin prefers to emphasize the discrete nature of innovation on the "micro" and "macro" levels. He observes a tendency for the innovating entrepreneur (unlike the inventor) and his vehicle, the firm, to pursue the exploitation of only one basic technological idea. He is also impressed with the appearance of innovating entrepreneurs and firms in bunches, in many industries at one time, as though marshalled by the "wave-like force" of a Kondratieff cycle.

While change may be viewed on the "micro" level in terms of unitary ideas, it is important not to overlook the familiar fact of diversification, of coexistence within a firm of many such ideas in different phases of evolution. The innovating *entrepreneur* may come, exploit his great idea, and go; but the innovating *entrepreneurship* of a firm (especially of a corporation) may still survive, finding new champions and new avenues of expression. Numerous firms may be named—especially in the chemicals industry, unfortunately not discussed by Maclaurin, but even in the industries he does consider—which are continually diversifying their processes and products, growing in new technological directions from within, through merger, or through cooperative undertaking with other companies. The overlapping of discrete pulses of innovation could form a fairly continuous whole, even as short fibers make a long thread.

I do not share Maclaurin's belief in the fruitfulness of the Schumpeterian thesis linking innovation to the long cycle of business activity. In so saying, I do not mean to deny various important points Maclaurin makes—for example, that innovation does not necessarily follow invention, that skillful innovating entrepreneurship is rare, that imitation is easier than innovation, that scientific breakthroughs occur only occasionally, that successful individuals and firms could lose their zest for novelty in the course of exploiting one idea. I mean rather that an adequate "macro" theory of innovation must explain more than the clustering of new investment opportunities, inasmuch as new unitary ideas actually are being introduced and developed continually. The long cycle, moreover, is obscure. It

[6] I. H. Siegel, "Technological Change and Long-Range Forecasting," *Journal of Business*, July 1953, pp. 155-156.

requires more concrete definition than as a "wavelike force" if it is to explain anything. Besides, such a cycle might better be regarded as only a manifestation, a consequence rather than a cause, of the phenomenon of clustering.

To explain clustering, we should look for significant historical conjunctures, for transitory circumstances of potent, pervasive influence. For example, war and preparation for war, which have no explicit part in Maclaurin's scheme (except, perhaps, as retardants of the anticipated shift in economic activity toward the tertiary industries), provide occasions and conditions for broad innovational breakthroughs. Might not the technological history of our country be convincingly rewritten in terms of the "waves" of investment in novelty "generated" by Korea–World War II, World War I, the Spanish-American War, the Civil War, etc? The result would of course be a caricature, but a likeness nevertheless. In short, Kondratieff could be dispatched with Occam's razor.

3. *Three Cases.* Maclaurin's brief sketches of the automobile, electrical manufacturing, and radio and television industries are instructive moral tales. These industries presumably took at the flood a Kondratieff wave that led on to fortune. The automobile industry achieved success without benefit of research-minded leaders or of sustained innovation in the large sense. The other industries have a strong research tradition, but innovation there has been slowed by past profitable investment, monopoly power of the dominant firms, and cost and patent restraints on would-be challengers.

Diversification is conspicuously neglected in Maclaurin's treatment of the three industries. The electrical manufacturing industry is discussed in terms of lamps—one unitary idea—only. Nothing is said of the numerous other product lines of Westinghouse or General Electric; or of their wide range of research activities.[7] The discussion of the automobile industry might have mentioned the ventures of Ford into tractors and, briefly, into aircraft; or General Motors' tie-up with Du Pont, its production of diesel-electric locomotives, its quasi-public status as a munitions contractor.

Maclaurin's sketches suggest gaps in the theory of economic decision-making which need consciously to be filled. First, the behavior of the innovating entrepreneur should be brought within the same formal calculus that includes the entrepreneur who ac-

[7] C. G. Suits, "Seventy-five Years of Research in General Electric," *Science*, October 23, 1953, pp. 451-456.

cepts the existing boundaries of economic space. Second, innovation and all other alternatives open to the entrepreneur (including research, rounding out of product lines, market expansion through advertising, and organizational innovation to save taxes) should be brought under the sway of a common principle like "marginalism" or game theory. As usual, such a treatment of decision-making would be instructive even though it would mean an idealization of actual behavior.

Finally, I wish Maclaurin had more explicitly related his conclusions of the three case studies to his anticipations of the "next long cycle." The "paradox of technological change and investment" to which he refers has often been interpreted as an omen of stagnation or as an argument for strong antitrust or other measures to foster competition. Yet in Maclaurin's treatment this paradox does not seem to pose special obstacles to the future growth of existing industries or to the expansion of a company into new fields. Presumably, the flow of nongovernment investment funds will somehow remain adequate for a vigorous, growing economy. The only real worry, it seems, is the availability of the entrepreneurial talent to, say, solve the problems of organizational innovation besetting tertiary industries like private aviation and prefabricated housing. But there are some fundamental questions here which should not be obscured by the confident (and largely plausible) proposition that "the right man can usually find the money."

4. Growing Points of the Economy. It would be idle to match prophecies concerning the "next long cycle" in view of the hazards and the melancholy history of technological forecasting.[8] I wish instead to make some remarks on the methodology of inquiries into technological prospects.[9]

The Twentieth Century Fund study of such prospects, which has been in progress for more than two years, suggests that the growing points in the economy are numberless and almost everywhere (even in existing monopolistic firms). The sites of new growth are to be found in the primary and secondary as well as in the tertiary industries. The directions of growth are shaped by the research and development activities pursued competitively or with government funds throughout the *private production sector*; by the demands of *government*, especially for security purposes; and by the

[8] See my article in the *Journal of Business*, cited above.

[9] See also I. H. Siegel, "Conditions of Technological Progress," *American Economic Review*, May 1954, pp. 161-177.

demands of the *household sector* as it seeks a higher standard of material welfare and an upgrading of the quality of "leisure." The pervasive circumstance of cold war, with its "distorted" incentives and its emphasis on the technical training of manpower, spurs the discovery and development of new principles, processes, and products which today are directed toward military ends but tomorrow will be convertible to civilian needs. (Maclaurin's private helicopters —or, more probably, "helibuses"—will be reckoned among the progeny of the Korean affair, even as peacetime atomic energy should be counted with the technological offspring of World War II. In these instances, as in so many others, government is at least the midwife if not the father.)

To investigate systematically the technological prospects of the economy, it would seem desirable to give explicit recognition to research and development in the primary and secondary as well as the tertiary industries. We may emphasize the limitations of Colin Clark's Procrustean triple classification by designating such industry-creating activity as itself a *quaternary* industry. Second, explicit attention should be given to government entrepreneurship—to leadership in organizational and institutional innovation, in the conception of and demand for new munitions prototypes, in the training of the military and civilian personnel who will eventually facilitate the peacetime use of military ideas, in setting up new industries (like atomic energy), in effecting substantial resource improvements, etc. Again, for purposes of dramatization, we may call such activities *quinary* or fifth-order. Finally, note should be taken of the increasing variety and volume of appliances, gadgets, amusements, etc., demanded for leisure-time activities. The totality of consumers may be imagined to form a *sixth-order* industry, especially insofar as they demand and use goods and services beyond the level of subsistence. (A *seventh-order* category, based on specialized foreign demand—which could become significant in the next few decades—may also be defined.)

In short, a triple classification appears too gross for the study of long-term growth. The tertiary category, especially, is too broad and heterogeneous. To assert the shift of activity in the direction of this category is not very illuminating. Besides, this sort of proposition tends, like the nonstatistical "stage" theories, to confuse what Clapham designated as logical succession and historical succession. While Maclaurin stresses organizational innovation in the "next long cycle," the importance of such activity throughout our history should not be overlooked. For example, the Constitution, the National Banking Act, the Morrill Act, and the Federal Reserve Act were triumphs of organizational innovation within the tertiary category (government) even while the primary or secondary industries were in the ascendant. The development of the commercial corporation and the growth of consumer credit (to which Maclaurin alludes in his discussion of the automobile industry) are other early organizational landmarks.

PART VI

GENERAL THEORETICAL APPROACHES