Report File
Posted

GEORGIA INSTITUTE OF TECHNOLOGY
OFFICE OF RESEARCH ADMINISTRATION

RESEARCH PROJECT INITIATION

Date: 19 June 1973

Project Title: "State of the Art Review of Understanding of the Innovation Process"

Project No: G-43-602

Principal Investigator Drs. Melvin Kranzberg & Patrick Kelly

Sponsor: National Science Foundation

Agreement Period: From June 1, 1973 Until March 31, 1974

Type Agreement: Grant No. DA-39269

Amount:

$166,600	NSF
9,172	GIT (G-43-311)
$175,772	Total

Reports Required:
Monthly Progress Report, Mid-Point Progress Report (due about September 30, 1973) and Final Technical Report (due May 31, 1974)

Sponsor Contact Person (s):

Mr. Wilbur W. Bolton, Jr.
Grants Officer
National Science Foundation
Washington, D. C. 20550

Assigned to: Social Sciences

COPIES TO:

Principal Investigator	Library
School Director	Rich Electronic Computer Center
Dean of the College	Photographic Laboratory
Director, Research Administration	Project File
Director, Financial Affairs (2)	
Security-Reports-Property Office	
Patent Coordinator	Other

RA-3 (6-71)

Pool

No Action

GEORGIA INSTITUTE OF TECHNOLOGY

OFFICE OF RESEARCH ADMINISTRATION

RESEARCH PROJECT TERMINATION

Date: April 2, 1975

Project Title State of the Art Review of Understanding of the Innovation Process

Project No: G-43-602

Principal Investigator: Dr. Melvin Kranzberg & Dr. Patrick Kelly

Sponsor: National Science Foundation

Effective Termination Date: 2/28/75(End "flexibility period").

Clearance of Accounting Charges: by 2/28/75

Grant/~~Contract~~ Closeout Actions Remaining: None

Assigned to School of ~~Social Sciences~~

COPIES TO:

- Principal Investigator
- School Director
- Dean of the College
- Director of Research Administration
- Office of Financial Affairs (2)
- Security - Reports - Property Office
- Patent and Inventions Coordinator
- Library, Technical Reports Section
- Computer Sciences
- Photographic Laboratory
- Terminated Project File No. ________
- Other ________

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
SOCIAL SCIENCES

June 19, 1973

G-45-602

MEMORANDUM

TO: Participants in Georgia Tech Innovation Project

FROM: Mel Kranzberg and Pat Kelly

SUBJECT: Report on June 13 meeting

Attendance

We are happy to report on the great progress made in getting the Georgia Tech Innovation Project formally under way at our meeting at the Georgia Tech campus on June 13. Several of our blue-ribbon consultants (Simon Kuznets, Everett Rogers, Nathan Rosenberg, Paul Strassmann) were present, along with a group from the NSF (Mary Ellen Mogee, David Roessner, Albert Bean, Eleanor Thomas, Andrew Pettifor) and representatives of related projects (Charles Douds of Northwestern; Edward Woods of Stanford Research Institute).

Industry Advisory Panel

Unfortunately, because of the short notice, none of our Industry Advisory Panel could attend. However, they are eager to be informed of the progress on our project and they promise to look over our shoulder and give us helpful guidance as the need arises. Since the names of our Industry Advisory Panel do not appear in the proposal itself, we list them here for your information:

Dr. Herman Bieber (Senior Research Associate, Esso Research and Engineering Co.)
Dr. W. Gale Cutler (Director of Corporate Research, Whirlpool Corp.)
Dr. Edward David (former Science Advisor to the President of the United States)
Dr. Jacob E. Goldman (Senior Vice President, Research and Development, Xerox Corp.)
Dr. William E. Hanford (Vice-President, Research and Development, Chemicals, Olin Corp.)
Dr. Walter R. Hibbard (Vice-President, Technical Services, Owens-Corning Fibreglas Corp.)
Dr. Roland W. Schmitt (Research and Development Manager, Physical Science and Engineering, General Electric Co.)
Dr. Julian D. Tebo (retired Director of Technical Information Services, Bell Telephone Laboratories)

Project Stages

A large project involving many complex operations requires careful organizational planning. This is particularly the case with our very short deadline which necessitates that many tasks must be carried on at the same time--and yet dovetail together into a finished product. Drs. Norman Baker and Fred Tarpley, thus applied PERT technique to the operation of this scholarly project. The results of their efforts are a flow chart (enclosure 1) which carefully delineates the steps in Phase I of the project, highlights the major areas of concern, and indicates the target dates for each part of the complex whole.

Will the PERT system work as well in a scholarly enterprise as it did, say, for Admiral Raborn in the development of the Polaris missile system? For the answer to that question, tune in next January for the windup session of the project, to be held in Washington, D. C.

Stage I: Literature Analysis

Our Innovation Project consists of two phases or stages: analysis and assessment. The first of these, has two components: (1) the classification and coding of a large body of the most recent literature on the innovation process, and (2) in-depth abstracting of the most significant subsets of this literature. Both of these processes are already under way and, indeed, test runs have indicated the viability of the computer program that has been developed.

Dr. Frederick A. Rossini, in collaboration with Tarpley and our computer systems analyst, Taylor Little, set up a coding system and computer program for the storage and retrieval (in many different combinations) of the information regarding the literature items. Dr. Morris Mitzner worked out a set of definitions for the coding of the information, and as guidance for those classifying the literature; Miss Francis Kaiser, technical research librarian, has spearheaded a literature search to seek out abstracts and journals which contain materials relevant to the innovation process. A number of graduate students and advanced undergraduates are already at work on this stage of the project.

At the June 13 meeting, the coding sheet (enclosure 2) underwent lengthy discussion. It was recognized that the information could be classified in different ways and there was general agreement that our approach was comprehensive enough to include all the significant variables. There was some dissatisfaction with the accuracy of the term "Units of Analysis" to describe items 39-50;

to meet this criticism, the term was changed to "Process Contexts". It was also decided to add two items to the computer program, one identifying the coder, and the other for the coder to give a first-evaluation of the significance of the literature item being coded.

At the suggestion of Dr. Kuznets, the definition of the first Process Phase (to be used in conjunction with the coding sheet, see enclosure 3) was changed to take explicit cognizance of the role of scientific discovery in stimulating Problem Definition and Idea Generation.

For the Descriptors of the Field of Technology (items 62-63) of the coding sheet, it was agreed that the U. S. Government Standard Industrial Classification should be employed.

Although some 4,000 pieces of literature will eventually find their way into computer storage through the literature search and classification operation, there is still the problem of determining the most significant of these for in-depth abstracting. To that end, a complementary tree-ing approach is being employed to assure completeness. The consultants and the Georgia Tech team have each furnished lists of what they consider the most important published works. These and the references they cite will be coded into the system. We plan to develop three generations of this treeing approach. Further checks will be made by means of citation counts of items occurring in the tree. It is estimated that some 400 pieces of literature will be selected for in-depth abstracting through the combination of our major classification effort and this complementary treeing approach.

Finally, it should be noted that the coding system and computer program to be employed in this project will be compatible with those being employed elsewhere (e.g., Northwestern University). Hence the data accumulated in the course of the Georgia Tech Innovation Project will be available to other interested researchers throughout the country either directly by telephone tie-ins between the Georgia Tech computer and outside computers, or by materials that we can supply by mail.

Stage II: Literature Assessment

Concomitantly with the analytic stage--the coding and abstracting described above--will be the assessment stage of the Project, which deals with the quality of our understanding of the innovation process at both the empirical and theoretical level. And, just as the analytic, data-gathering stage consists of two components, so does the assessment stage. One component is the state-of-the-art assessment of the literature to be carried on by the Georgia Tech project group; the other component consists of more narrowly-focused papers to be written by the outside consultants, complementing the general state-of-the-art assessment.

In order to provide a logical structure--and an equitable and feasible division of labor--the paper of the Georgia Tech Project Group will be broken down into five parts corresponding to the Process Phases of the Innovation process itself: (1) Problem definition and idea generation, (2) Invention, (3) R&D, (4) Innovation, and (5) Diffusion. The Georgia Tech Project Group has divided itself into five teams, each of which will be responsible for the assessment of the literature in one of the five Process Phases. Three members are assigned to each team, the assignments being made on the basis of special expertise and particular interest; every member serves on at least two teams, in order to provide the necessary feedback of knowledge among the Process Phases (and, not so incidentally, correspond with the linkages within the innovation process itself).

While the Georgia Tech Project Group works on the general assessment paper (the first draft of which is scheduled for completion at the beginning of September), the consultants will be working on their more specialized papers (scheduled for completion by November 1). Considerable time was spent at the June 13 meeting in delineating the scope and thrust of the consultants' papers; this effort was directed at taking advantage of the special expertise of the outside consultants and to make certain that the different Process Phases and methodological approaches to the innovation process would be fully covered.

Although not all the consultants could be present at the meeting, several of them had sent in abstracts of their proposed papers or, had discussed their papers by telephone with the principal investigators. The list given below is not final; indeed, we are still in the process of clarifying and more closely dividing some of the topics to be treated by the consultants. Nevertheless, the list is sufficiently complete and detailed to indicate how the consultants' papers will fit into the overall structure of the Project and contribute to its success.

Thomas Parke Hughes, in his paper entitled, "Technological Innovation: Reverse Salients, Expanding Fronts, Critical Problems, and Patterns of Patents," will focus on individual innovators, such as Elmer Sperry and Thomas A. Edison, whom he labels "inventor-entrepreneurs." The concepts which he introduces are designed to illuminate the ways in which many major inventors identified critical problems and how they went about solving them. This paper will therefore be directed primarily at the first two Process Phases (Problem definition and idea generation, and Invention), but because the subjects of Dr. Hughes' discussion of the literature are entrepreneurs as well as inventors, it will show the all-important linkages among all the phases of the innovative process.

Edwin Mansfield will focus on the economic literature which treats the innovation process at both the firm and industry levels. At the firm level, it will deal with innovation in terms of the organization of firms, the relationship of marketing to R&D, the organization and decision-making procedures within R&D, and how these are integrated. At the industry level, it will deal with market structure and its effects on innovation. In brief, Dr. Mansfield will investigate the micro-level of innovation within the individual firm and the meso-level of innovation within an industry. In order to avoid superficiality, he will not concern himself with the macro-level of the economy as a whole or with treating the role of education, government, and the like in the innovative process.

Richard S. Rosenbloom has chosen as his topic the influence of corporate strategy on R&D in innovation. To that end, he will look closely into two bodies of literature. First is the literature in the field of corporate strategy (e.g., Kenneth Andrews, The Concept of Strategy; Bruce Scott, "The Industrial State: Old Myths and New Realities," in the April 1973 issue of the Harvard Business Review). The second body of literature he will investigate is the large number of empirical studies on innovation in the private sector (exemplified by the Marquis and Myers volume). A comparison will be made between the theoretical studies of corporate strategy and the empirical studies of innovation.

Nathan Rosenberg will write on "Innovation and the Resource Environment." Viewing innovation as an adaptive process relating to various needs and endowments, he will review the literature showing how factors of production, natural resources (including energy, materials, etc.), and the like--what Dr. Kuznets termed "the explanatory variables"-link up to the innovative process. Some attention will be paid to the policy impact and implications of the innovation-resource environment interface, and how these are involved in the various phases of the innovation process.

Everett M. Rogers has chosen as his topic "Diffusion of Innovations Perspectives on National R&D Assessment." Using as his starting point his 1971 book (which has already become a classic in the field, MK), Dr. Rogers will present a short summary and synthesis of the main elements in the diffusion of innovations, including updating the diffusion literature since the completion of his book manuscript. He will then analyze the assumptions/biases in the body of diffusion research literature, indicating how these might be overcome in future research. His paper will conclude by showing the intellectual connection of the diffusion approach with related fields, such as the economics of technological change, inventive behavior, research utilization, and the transfer of technology.

James R. Bright was unable to attend the meeting or send in a preliminary abstract. However, from previous communications and messages from the other consultants, it is known that he intends to focus his paper on those elements of the innovation process which are at the "heart" of technological forecasting, which is his prime field of specialization. A more detailed statement regarding Dr. Bright's contribution to the project will be forthcoming shortly.

Albert H. Rubenstein has enlisted Charles Douds as his collaborator on a paper, "Review and Assessment of the Methodology Used to Study the Behavioral Aspects of the Innovation Process." Pointing to the wide range of methodologies employed in studying the innovation process (largely because of the diversity of specialists who have studied this problem from the standpoints of their specialized fields) and the wide variety of techniques which have been used in this connection, Drs. Rubenstein and Douds intend to explore the possible effects of the methodologies used in the characteristics of the results (such as credibility, communicability, reliability, validity, quantifiability, and generalizability). By analyzing a sample of empirical studies of the innovation process, they hope to come forth with a rough guide to the range of possible methodological approaches, some judgments on their cost/benefit aspects in terms of information yield and possible pitfalls, and some recommendations for increasing the credibility factor in future studies of the innovation process.

W. Paul Strassmann will write on what might be termed the "intangibles" of the innovative process. Pointing out that the present body of literature is biased, or skewed, because it deals almost entirely with successful innovation, he claims that we can learn much about the innovation process by studying failures and, perhaps even more important, why there has been a lack of innovation in certain fields. Dr. Strassmann further pointed out the paucity of literature dealing with innovation in the public sector (e.g., housing) and in the service trades; most of the extant literature deals with product innovation in the private sector. Utilizing his special knowledge in the field of construction--an industry where there has been a lack of innovation, where public policy has had a large impact, and where change might be less dependent upon product innovation--Dr. Strassmann will employ an approach which might be described as "reversing the field": he will attempt to add to our knowledge and understanding of the innovation process by assessing the "non-literature" in the field, or, more precisely, the literature gaps.

Simon Kuznets, as befits a pioneer in the study of economic growth, had somewhat broader concerns than our other consultants. Instead of writing a specialized paper dealing with selected aspects of the literature on the innovation process, he will write a paper--the concluding one--which will deal with matters of wider scope. He will focus on innovation as a source of economic growth and its consequences. His paper will thus provide a "wrapup" to the assessment papers of the Georgia Tech Project Group and the consultants, and will also serve as an introduction to the more far-reaching aspects of the other elements of the NSF National R&D Assessment Program. If the principal investigators might be forgiven for speaking in epistemological terms, we can place Dr. Kuznets's paper in the perspective of the entire project as follows: the coding process and computer programming will generate data about the literature on the innovation process; the abstracting will give us information; the assessment papers will provide knowledge; and Dr. Kuznets will contribute wisdom.

Dissemination

In addition to the dissemination of results mentioned in the original proposal (report to NSF, publication in Technology and Culture as a special theme issue), we plan on diffusing the information generated in the course of this project in other ways. A series of Georgia Tech Innovation Project technical reports will be issued for the information of the NSF, the consultants, the Industry Advisory Panel, and any other interested individuals and institutions. The first technical report, to be prepared by Taylor Little and Jay Norman, is a description of the computer program and coding process developed for the project. This report will be of interest to information specialists and also to scholarly researchers in innovation studies, for it will indicate how other research centers can "plug into" our data bank to assist them in their own work.

In addition, tentative arrangements have been made for a presentation of the results of the Georgia Tech Innovation Project in the context of a full-day symposium on the innovation process at the 1974 annual meeting of the American Association for the Advancement of Science. This symposium will be sponsored by Section P (Industrial Science), M (Engineering), and L (History and Philosophy of Science) of the AAAS.

Reactions and Responses

We would appreciate feedback on any item mentioned in this memorandum, or on any aspect of this Project. Communication, by etymological definition, is at least a two-way street; we want to keep all the participants informed of what the Project is doing and where we are going, but we also need your comments and reactions. Do not hesitate to write or phone Mel Kranzberg or Pat Kelly--same address (Department of Social Sciences, Georgia Tech, Atlanta, 30332) and same phone (404-894-3195).

G43-602

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
SOCIAL SCIENCES

August 6, 1973

Ms. Mary Mogee
Office of National R & D Assessment
National Science Foundation
1325 K Street, Room 700
Washington, D. C.

Dear Mary:

This letter is intended as a summary report of our work on the project during July. Topics deserving mention are:

1. Overview of work
2. Meeting with Richard Rosenbloom
3. AAAS Program
4. First draft outlines of our working papers

In line with our understanding, I will keep the remarks on each topic brief and to the point.

1. <u>Overview</u>

In addition to the anticipated down-time associated with moving the computer to its new facilities, we have lost more than two weeks because of a big, hard-to-diagnose "crash" (the computer's executive or possessor finally had to be replaced). The main effect on us was a large backlog of data which has cost us some graduate student time to catch up. We have now caught up but have had to go way over the graduate student budget to do so. This we are absorbing through state released funds.

We have done extensive cross-checks of the faculty and graduate coding three times, and now feel that the community of agreement on definitions and conventions is high. On a related matter, I am now convinced that the earlier skepticism about the ultimate value of the data bank in writing the assessment papers was ill-founded. Not only is it going to give us ready access to various cuts of the literature (which we viewed as the main benefit), but it should also provide us with some "research emphasis profiles" which should be quite helpful and difficult to get any other way.

Richard Rosenbloom made an important observation that we are going to try to follow up on. He feels that there is a fourth assessment question that should be raised, namely, "How can we come to know what we don't know?". The focus here is on the assessment of the various methodologies in terms of which the process of technological innovation is investigated. Our own work thus far has reinforced this point concerning the adequacy of our research tools. As a result, we are now planning to offer some assessments of methodology as appropriate in each of our papers. In addition Mel will pull together and summarize these comments in the introductory or overview chapter. Our plans for treating this suggestion is not reflected in the first draft outlines of our papers, but will appear in the next refinement which is scheduled for the second week in August.

As you know, we asked each of our consultants for a list of the 10 books or articles they considered the most important contributions to the field. These we have come to call "the classics". A list of the citations or references from the classics has now been compiled and the frequencies of duplication recorded. The pattern is the same as found by Price and Crane in other fields, namely a high-frequency cluster for about half the references (the invisible college) and a scatter for the other half. We are coding all of the high frequency items and will probably abstract most of them. As you might suspect Griliches article on hybrid corn in Econometrica is the champ (17 references). Mansfield has the most items cited 3 or more times.

2. Meeting with Richard Rosenbloom

We had a very productive session with Richard Rosenbloom on July 11, 1973. His paper topic as summarized in the report of our meeting of June 13 deals with the impact of corporate strategy on innovation. While the literature on corporate strategy rarely deals with technological innovation, he is going through it to see what ties are there. Then he is going to look at empirical studies of R&D within the firm for data on the role and impact of corporate strategy. He is then going to tie the two together--it should be an interesting paper, and if his day with us is any indication it will be a first-rate contribution.

3. AAAS Program

Our proposal for a one-day session on the process of technological innovation has been accepted for the 1974 meeting in San Francisco, a copy of the program is enclosed. As it indicates the session will be co-sponsored by Section M (Engineering), Section P (Industrial Science) and the Society for the History of Technology. We do not know yet whether the Section L (Philosophy of Science) will also co-sponsor (we have written Dudley Shapere but have not yet heard from him.). In any case, this session should provide an unusually fine diffusion mechanism.

4. First Draft Outlines of Our Working Papers

I have enclosed our first attempts to outline the working papers we will be sending to you and our consultants in early September. We imposed on ourselves the descipline of a very early date (July 26) for a first try at outlining our papers. We are obviously still working our way through the literature and the outlines will be restructured several times. But we felt that an early attempt would help focus our efforts. We would appreciate your reactions if you would like to offer them at this stage.

I am sure that I have omitted some items that need mentioning, but I can't think of them at the moment. In any case I need to get back to work now.

Call me if you have questions.

Sincerely,

Patrick Kelly
Department Head

PK:jdh

Enclosures

Draft Outline

The Ecology of Innovation

I. Introduction

A. Popular Stereotypes

1. The comic-strip version
2. The adage: "Necessity is the mother of invention."
3. Technology as applied science

B. The complicated nature of the innovative process -- the ecological analogy.

C. Definitions of innovation

D. The Process Phases - Definitions

II. Theories of Invention

A. Deterministic

1. Social Need
2. Natural Selection
3. Toynbee's "Challenge and Response"
4. Geographical Determinism
5. "Racial" Determinism
6. Configurations

B. Individualistic ("Great-Man") Theories

1. Heredity (Galton)
2. Freudians
3. Groups: "Races," Nations
4. Schumpeter's Entrepreneur

C. Chance Factors: Serendipity

D. Composite Theories

1. Anthropological - Dixon: Need, Genius, Opportunity
2. Gestalt - Usher

E. Inventive Creativity as Interactional Process

III. The Social Background

A. The Milieu: Sociocultural Preparedness

1. Barriers to innovation in the slave society of antiquity
2. The Christian view of man and nature
3. The Protestant Ethic -- Weber-Tawney thesis: Merton
4. Marxist views
5. Resistance to innovation

B. Economic Incentives: Profit

1. Market Forces - Demand (Schmookler)
   "Build a better mousetrap..."
2. Factor Savings

C. Political Factors

1. Military Incentives
2. Nationalism (Mercantilism, academies, "space race")
3. Government regulations: Safety, environment, etc.

D. Institutionalization of the Socio-economic Demand

1. Patent System
2. Education
3. Diffusion Mechanisms
   a. Professional Societies
   b. Publications
   c. World's Fairs and Exhibitions
   d. Transfer devices (Agricultural extension service; NASA's technology utilization program, etc.)
4. Industrial Research Laboratories for R&D: Problems of personnel, organization, structure, information flow, accounting mechanisms; measurement of effectiveness.

IV. The Technological Background

A. Technological "Readiness"

B. Technological Imbalances

C. Problem-Solving

1. The Technological Imperative: Efficiency
2. Technological "Momentum"
3. The Human Element: Homo ludens (the play factor); the aesthetic factor

D. Science-Technology Interactions

1. Hindsight
2. Traces
3. Langrish
4. Battelle - NSF Study
5. Kranzberg's "Push-Pull" Models

V. Some Brief Illustrative Case Studies

A. Watt and the Steam-Engine

B. Fulton's steamboat

C. Bell and the telephone

D. Edison and the electric light

E. The Transistor

VI. Conclusions

A. The ecological feedback among the process phases of innovation.

B. Much more to be learned - Introduction to elements of Georgia Tech project.

Problem Definition and Idea Generation

tentative outline

I. Introduction

A. Wise Sayings about Creativity
B. Types of Data
   1. Unexamined assumptions
   2. Folklore
   3. Theoretical materials
   4. Case studies - descriptive
   5. Empirical investigations
C. Boundaries of the Process Phase

II. Independent Variables

A. Macro Level
   1. Nature of the industry
      a. mature
      b. developing
   2. Nature of need/usage patterns of existing technology
   3. State of technical knowledge
B. Meso Level
   1. Economic condition of firm
   2. Organizational structure
      a. mechanistic
      b. organic
   3. Patterns of information flow
      a. from external environment
      b. internally
C. Micro Level (Lab or Project Group)
   1. Specificity and immediacy of research aims
   2. Leadership style
      a. authoritarian
      b. participatory
      c. laissez-faire
   3. Total workload

III. Intervening Variables - Problem Definition

A. Sources
   1. Need - means pattern dominant
   2. Means - need pattern secondary
      a. old technology
      b. new technology
   3. Role of customers/vendors
      a. origination
      b. oral communication
   4. Role of "top person"

B. Receptivity/Resistance
    1. Perception of organizational goals
    2. Clarity of short-term profit potential
    3. Assessment of in-house competence
C. Workload and Released Time
D. Idea Generation Groups

IV. <u>Intervening Variables - Idea Generation</u>

A. Visibility of Research Results
    1. Nature and extent of assessment
    2. Previous support of ideas
B. Perception of Corporate Objectives and Policies
C. Freedom/Control
    1. Time
    2. Search procedures
    3. Specification of solution range
D. Support/Resistance/Access
    1. Firm environment
        a. "top person"
        b. support units
    2. Lab environment
        a. "project champion"
        b. resources
            (1) physical
            (2) informational
E. Availability of Information
    1. "Gatekeepers"
    2. Consultants
        a. internal
        b. external
            (1) hired consultants
            (2) vendors
            (3) outside colleagues
    3. Information flow by mobility of researchers from firm to firm
    4. Role of oral communication
    5. Literature
        a. professional journals
        b. trade journals
        c. books, reports
    6. Technical meetings/professional societies
F. Personal Factors: Biographical
    1. Highest degree earned
    2. Time since degree
    3. Diversity of prior experience
    4. Career orientation
        a. "cosmopolitan"
        b. "local"

G. Personal Factors: Social-Psychological
   1. Positive/negative biasing sets
      a. prior experience
      b. prior knowledge
      c. "extended effort" experimentation
   2. Need for approval
      a. innovative/conformist
      b. relation to leadership style
   3. Psychological measures
      a. Guilford battery
      b. Remote Associates Test
      c. Dogmatism scale
      d. research style

H. Social Factors
   1. Role/status influences
   2. Group pressure and idea modification
   3. Cooperation/competition
   4. Group homogeneity/heterogeneity
   5. Solitary U.S. cooperative idea generation
      a. deferred judgment
      b. delphi
      c. extended effort
      d. solution development records
      e. solution pairing
   6. Anxiety and productivity

I. Incentives

V. <u>Submission and Disposition Ideas Generated</u>

A. Management Responses
   1. Project status provided
   2. Shelved
   3. Communicated to another division
   4. No response
   5. Rejected

B. Management Decision Criteria
   1. Urgency-immediacy of need
   2. Predictability - degree of certainty of applicable means
   3. Time horizon - initiation to completion timeframe

C. Subjective Evaluation of Researchers
   1. Of idea submitted
   2. Of management response

D. Factors Influencing Non-submission of Ideas
   1. Time pressures
   2. Anticipated negative evaluation from management
   3. Negative evaluation from peers
   4. Negative evaluation by group leader
   5. Previously rejected by management

PRELIMINARY OUTLINE - INVENTION PIECE [illegible]

1. Definition and Typology of Inventive Activity

   A. Definition of invention and distinction from discovery and subinvention

   B. Distinction between technical (physical) and social (human) invention

   C. Distinction between invention in private and public sectors

   D. Invention by solving the problem internally versus invention by altering the problem

   E. Linkages between invention and other process phases

   F. Scope of coverage in this paper

2. Contexts of Inventive Activity

   A. Economic - under what sorts of economic systems or organizations are various types of inventions possible/useful

   B. Organizational - individual inventor, private firm, government facility. How do these contexts influence invention and decision points within the organization? Is the proportion of inventive activity in each changing over time? (Psychological aspects left to PK)

   C. Informational - what is the role of technical, natural scientific, social scientific and general background information in the inventive process? What are the sources and uses of information and how is it diffused? — in other or related fields Technological linkage

   D. Human + Social Contexts — Great Man vs [illegible]

3. Measure of Inventive Activity

   A. Input measures into invention - consider both economic and non-economic -- capital input, R&D expenditures, technological input.

   B. Output measures from invention - consider both economic and non-economic -- profit from invention, technological output.

   C. Patent Statistics, Raw and Processed Data

   D. Criticism and evaluation of these measures

4. Rewards and Incentives for Inventive Activity

   A. Present patent and licensing [illegible] in [illegible] and other countries, government [illegible] support of inventive activity

   B. Problems with these and proposals [illegible] improvement

   C. Evaluation of rewards and incenti[illegible]

5. Conclusions and recommendations

I. Introduction

The first thing to remember when narrowly focusing on the process we call application (innovation) is that it is the fourth process phase in our five-phase schema. This means that the first three phases must be performed in a functional, if not a formal way for the possibility of (innovation) commercial application to exist. Since this whole process is in reality a continuum and the stages are merely bench marks along this continuum, the tasks relevant to this stage are a product of, and are influenced by, the variables affecting the other three previous processes, especially the R&D process. The literature stresses the connection between R&D and innovation and in most cases muddles any meaningful distinction between the two. One of the challenges which must be addressed in this essay is to treat meaningfully the strong linkage which exists between what we have designated R&D activities, while still addressing the particular problems of innovation and commercial application.

II. Market Forces--A Market Orientation

There seems to be some controversy concerning whether innovation has a demand or supply orientation. Does innovation take place because the scientific knowledge exists, or do market forces require increased and/or lower cost production, thus encouraging the firm to begin the whole innovative process and especially the decision to move from R&D to commercial application? The concensus seems to be that there is a strong market orientation. The pressure for innovation and commerical application comes from the existence of expanding markets; the need to increase production to maintain or expand market shares, the need to take advantage of potential profitable operations, etc. Additionally, a great deal of pressure seems to be exerted because of rising costs or unavailability of certain inputs. This is the classic case of factor substitution where less expensive factors are substituted for dearer factors.

The emphasis on demand orientation of the stage of the process that we call application or innovation is held by most of the economists writing in this field and, in part, is simply a result of the internal decision rules of the firm when it makes a decision to move from R&D to commercial application. Indeed, many of the writers in the field, such as Schmookler, posit that the demand orientation goes all the way back to idea generation and problem definition and applies to the whole process, not just the application phase. Utterbach in his review of the literature comments rather forcefully on this demand orientation.

The strong market orientation is reinforced by the tendency observed by several commentators for innovation to be often a process of modification and incremental improvements as opposed to great leaps forward. The tendency is toward modification of existing production processes and technique as opposed to new production processes and technology. There is a predilection for short-run as opposed to long-run projects. This in many ways is part and parcel of what we define as market orientation.

One of the areas of greatest discussion deals with the structure of the industry and/or the size of the firm. Numerous writers in the field have attempted to attack this problem in a large number of empirical investigations. Most of the empirical work arises out of attempts to support or refute the hypothesis of Schumpeter and Galbraith, which simply stated is that large firm size is necessary to carry on the complicated

R&D needed by today's technology--and, of more particular importance to the application phase, that large firms have the resources which allow them to play the probabilities. They can afford to have failures as long as they have a winning percentage. Against this position is a large body of theoretical and empirical studies which contends that high levels of concentration in an industry tend to inhibit the market pressures which argue for continued innovation and that R&D under these conditions tends to be a defense mechanism to protect established monopoly or oligopoly positions rather than providing a continuing stream of new or improved processes, products, or techniques.

Variations on this argument take place on several levels, but for the presentation of this study I think chronicling this argument at three separate levels will be sufficient. The first level is that an oligopolistic industry tends to be less innovative than an industry organized in a more competitive manner. The second level is an intra-industry argument which states that small firms tend to introduce more than their proportional share of innovation. The third level stresses the importance of the individual inventor or the small organization, including but not limited to the small, high-technology-based spin-off firm in electronics, computers, etc.

The argument about industry structure and firm size has several iterations. One level the argument is carried on is in terms of the amount of money spent on R&D and innovation; at another level it deals with the productiveness of these expenditures; at a third level it deals with an enumeration and analysis of important innovations.

Connected to the above discussion of market orientation, industry structure, and firm size is the concept of time. In many ways the concept of time is so mingled with the arguments concerning demand orientation, industry structure, and firm size, and the whole innovative process as opposed to solely the application phase that it becomes difficult to treat time in a concise and meaningful way. For example, there is a great deal of discussion in the literature concerning the time lag from one step in our process to the other--a particularly large discussion in the R&D phase and the time lag between R&D and application. Much of this argument is tied up to the argument concerning firm size, larger firms requiring a longer time period to move from R&D to application than do smaller firms. The argument is made that the time lags are greater in mature or oligopolistically organized industries as opposed to more perfectly competitive industries.

III. Internal Processes of the Firm

There is a whole other set of literature which I am just beginning to be involved with and which I feel I shall need help from Norm Baker on, dealing with how the internal processes of the firm interface with the application stage. Many of the arguments in the area are simply recasting of the "market forces arguments" outlined above, and yet the literature is often quite different and the backgrounds of the people doing the investigations tend to be in management, sociology, and management science as opposed to economics. For example, the concept of the internal organization of the firm must in part be related to firm size. Large organizations tend to need a different organizational mode from smaller firms'. The distance between the various functions performed and the policy makers at the top is a ~~function~~ determinant both of size and of the way the company is organized and the necessary levels or steps between the operational and research practitioners and top management. There is a great deal of discussion of various forms and types of management organization and strategies. For example, should new product introduction be organized on a project basis, functional basis, or some hybrid basis such as matrix?

There is also a large literature dealing with prescriptive models for making various types of decisions. Much of this is directed at what we have labeled the R&D phase, but by extension these descriptive models apply to what we have labeled as the innovation phase. These deal with balancing risks and potential pay-off with the timing dimension of alternative innovations, with the scheduling and costing of various projects, etc. As I mentioned above, I shall probably have to lean very heavily on Norm Baker in this particular area.

IV. Governmental

The policy, programs, and expenditures mechanism of the Federal government have a definite influence on the applications phase of the innovative process. In some cases this influence is direct and easily traced, and in other cases it is more indirect and often very subtle. For example, government tax policy on loss carryovers, stock options for small businesses, the treatment of expenses of individual inventories, etc. have a very definite effect on the innovative process in general and the applications phase in particular.

Government participation in developing sources of venture capital through very small business administration programs may be extremely important in terms of increasing the innovation that comes from industry inventors and small organizations. Government contracting and contract procedures often are particularly important in the innovative process. For example, many of the small electronic, computer, and aerospace firms numbered as their only or major customer in their earliest stages of existence some agency of the Federal government.

The whole concept of industry structure and firm size is directly related to antitrust policy of the United States. The policies of the Anti-trust Division of the Department of Justice and the Federal Trade Commission can and will influence at least in part the structure of various industry participation and joint venture patent polling, sharing of research results, etc.

Additionally, Federal and State government support of universities, other research organizations, and government research programs will have a very definite influence on the whole innovative process, including the application stage. Several authors have stressed the importance of interaction between the academic and industrial communities and its effect on the innovative process.

V. Methodology

Methodology in this area is of three basic types. There is a large body of theoretical work in economics which deals with various forms of market structure and how they impinge on economic efficiency, including innovation. Secondly, there is a large body of literature, basically empirical, which tests various hypotheses concerning firm size and industry structure. Third, there is a body of management or behavioral science literature which deals with internal organization of the firm and how this organization effectuates its goals. In addition, there are financial and management science models which relate to various means of dealing with risk, uncertainty, and time.

NRB 7/25/73

[illegible]tline of Paper: R&D Phase

I. Descriptive models of R&D
   A. Flow from basic - applied - development - engineering - application
   B. Idea - proposal - project - results
   C. Offensive vs. Defensive [illegible]

II. Descriptive data regarding R&D in U.S.
   A. Patterns of funding
   B. Reactions to outside factors
   C. Impact of organizational structure
   D. Nature or characteristics of research undertaken

III. Sources of ideas for R&D projects
   A. Factors influencing rate of flow of ideas and adoption
   B. Internal vs external sources
   C. Processes by which information gets into the organization
   D. Processes by which ideas flow within an organization

IV. Research project selection and resource allocation
   A. Descriptive models
   B. Data estimation problems
   C. Benefit measurement approaches
   D. Constrained optimization models
   E. Future or expected advances

V. R&D ~~performance~~
   A. External factors: industry, firm size, market, competition, obsolesence, etc.
   B. Organizational factors and individual considerations
   C. Structural factors -- stage of evolution
   D. Cost-time overruns

VI. R&D and interaction with other organization functions
   A. Marketing
   B. Transfer of innovation, information, and authority
   C. Targeting and controlling

VII. Role of long range planning and technical forecasting

OUTLINE--DIFFUSION PHASE

Morris Mitzner

1. Differing interpretations of diffusion and the ensuing confusion

2. The descriptive, non-diffuse, non-cumulative and non-comparable nature of most diffusion studies

3. The encyclopedic work of Everett M. Rogers

   a. Rogers all encompassing conceptualization of the diffusion process--the spread of a new idea from its source of invention or creation to its ultimate users or adopters--and the micro-scale constraints it places upon the study of diffusion.

   b. The "implied" linkages to the various phases of the innovative process.

   c. The key variables inherent in each of the "implied" linkages.

4. Looking at the literature, generally, what are the variables pertinent to the diffusion process? i.e.

   Economic factors, Organizational factors, Cultural norms, Communication process, Level of Education, Availability and Experience of technological know how, Invisible College, Change Agents, Opinion Leaders, Traits and Attitudes, Deliberate Policy (at various levels), Spatial Distribution.

5. What combinations of the above appear to be pertinent to each one of the linkages?

   a. diffusion--problem definition, idea generation
   b. diffusion--invention
   c. diffusion--research and development
   d. diffusion--application

6. Do the variable combinations for each of the four linkages remain constant from one diffusion context to the next? i.e.

   a. diffusion--idea generation within a social system
   b. diffusion--idea generation between social systems
   c. cross-cultural diffusion--idea generation

7. To what extent are the above items developed in

   a. empirical studies
   b. non-empirical studies

8. To what extent are there gaps in the literature?

9. What is the degree of sophistication achieved in the measurement of the spatial distribution of innovations and how is this related to the Hagestrand-Morrill-Brown macro and meso diffusion tradition.

10. What generalizations may be derived from the diffusion studies?

11. Which ones, if any, appear to approximate empirical laws?

12. What are the implications for government policy emerging from an assessment of the generalizations of the diffusion literature?

1974 Annual Meeting -- AAAS

Section/Society: AAAS Sections P (Industrial Science), M (Engineering), and L (History and Philosophy of Science); Society for the History of Technology

Title: "Technological Innovation: What We Know, Don't Know, and Should Know"

Arrangers: Melvin Kranzberg (Georgia Tech), Patrick Kelly (Georgia Tech)

Duration: One day (two 2½-hour sessions)

Synopsis: This interdisciplinary symposium will have the general purpose of investigating our current understanding of the process of technological innovation. By technological innovation is meant the full range of activities from idea generation through invention, development, application, and diffusion of new technical devices, processes, and products. The symposium will focus on indicating the present state-of-the-art of innovation studies, on both the empirical and theoretical levels, showing where gaps exist in our knowledge, and suggesting means for improving our understanding. Scholars from many different fields will be involved: economics, industrial management, industrial engineering, R&D management, history of technology, philosophy of science, sociology, science policy.

Speakers: The plans call for three speakers at each session, allowing 30 minutes for each presentation and 15 minutes for discussion after each paper. Some of the speakers will outline the results of the innovation assessment project carried on at Georgia Tech under a grant from the National Science Foundation. Other speakers will include those who have recently been developing models of the innovative process.

Morning: Chairman - Melvin Kranzberg
Frederick A. Rossini, "The Act of Invention"
Norman R. Baker, "R & D"
Michael Michaelis, "Barriers and Incentives to Innovation"

Afternoon: Chairman - Patrick Kelly
Frederick A. Tarpley, "Commercial Application"
Morris Mitzner, "Diffusion"
Aaron Gellman, "A Model for Innovation"

Audience: The material to be presented should have wide interdisciplinary interest both among research scholars concerned with technological innovation and representatives from business, industry, and the federal government. The topic has special relevance to future researchers and policymakers in both the public and private spheres, and it is also meaningful to historians and philosophers of science and technology.

G-43-602

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
SOCIAL SCIENCES

Ms. Mary Mogee
Office of National R & D Assessment
National Science Foundation
1325 K Street, Room 700
Washington, D.C. 20550

Dear Mary:

This is a progress report on our project for the month of October. All of the first draft papers are now finished except Tarpley's. I indicated in an earlier report that Tarpley was having some conceptualization difficulties with his process phase. After discussions with the other group members, I had a long session with Tarpley in which I tried to provide a sharper focus on several dimensions of this task, and also suggested that certain elements in his outline had already been covered by Baker. I suggested that he simply rework Baker's material into his paper in these cases since Baker's paper was far too long anyway.

I think these moves helped somewhat, though in the course of this session it became clear to me that Tarpley's basic problem was that he had somehow gotten himself paralized by the task at hand. I am convinced that this is not because he lacks the ability or background. Rather, I think he has just become overawed by his immediate audience (Kuznets, Mansfield, Rosenberg, Rosenbloom), i.e., a younger economist having his work examined by superstars in the field. He has just worked himself into the ground trying to turn a first draft into a highly polished version, and a masterpiece at that time. Thus he has spent 2-1/2 polishing and repolishing his work instead of getting a draft finished.

Not finding a gentle approach that would accomplish the necessary result, I simply sketched the inevitabilities of our situation as well as his. This involved imposing a November 30 deadline for a completed first draft, with the clear understanding that if it is not finished at that point, the rest of the group will either take it over and write it or decide on some other course of action in consultation with your office. I do not know yet how this hard line will fare, but we will take whatever action is necessary to meet our commitments.

Even with this "all too human" difficulty we are still convinced that with the necessary efforts during the next two months we can get back on schedule, and not have to request an extension. The funds are also adequate for the work to be done.

There are no other major difficulties except the emotional one of reminding ourselves that the first drafts of the rest of the group are just that -- first drafts. They are not finished products and all suffer from the same difficulties; overlaps, occasional incautious generalizations, and above all they are heavy on description, light on assessment. You and your office can provide a valuable service to us in pointing these and other such weaknesses out; but I trust you will also keep in mind that they are first drafts and not let these weaknesses lead you to negative pre-judgments about the quality of our final output. We have more on the line than you and will not disappoint ourselves -- or you.

Be of good cheer.

Sincerely.

Patrick Kelly, Head
Department of Social Sciences

G-93-662
MPR #6

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
SOCIAL SCIENCES

Ms. Mary Ellen Mogee
Office of National R & D Assessment
National Science Foundation
Room 700 - 1325 K Street NW
Washington, D.C.

Dear Mary:

This constitutes a report for the month of December on the Georgia Tech innovation project. Since you and Al Bean spent the day with our group on December 10, you are familiar with our progress to that point, most of which has also been recapped in the mid-project report.

Upon receiving your favorable reaction to our revised outline, we got started on the rewrite. Mel is doing the overview or introductory chapter, tentatively entitled the Ecology of Innovation Fred Rossini and Fred Tarpley are co-authors of Chapter 2. The Societal Context of Innovation which treats in one place all the exeogenous variables that were scattered throughout the working papers.

Norm Baker and I will be writing Chapter 3. The Organizational and Individual Contexts, Chapter 4, The Utilization Context, which includes the transfor from R & D and diffusion is being prepared by Fred Tarpley and Morris Mitzner, with some help from Norm Baker on the former topic.

Mel was able to confer with Nate Rosenberg while at the Society for the History of Technology meeting in San Francisco the week after Christmas. He has promised to have comments on our first drafts ready as soon as possible.

That's all I really have to report now.

Sincerely,

Patrick Kelly, Head
Department of Social Sciences

Xc: Jerry McPherson, ORA ✓

Monthly Progress # 7

G-43-602

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
SOCIAL SCIENCES

Ms. Mary Ellen Mogee
Office of National R & D Assessment
National Science Foundation
Room 700 - 1325 K Street NW
Washington, D.C.

Dear Mary:

I wish to report the activities and developments on our innovation project for the month of January, 1974. The most important item to be reported is that Morris Mitzner is in the hospital with a pinched nerve in his neck. He is undergoing traction therapy at this point (he has been in about 10 days) and the prognosis is unknown. For the time being I have taken over the work on the diffusion section of Chapter IV, and am trying to develop a structure that will at least allow for treatment of the range of variables involved.

Work on the other chapters is coming along rather well, though slower than we had anticipated. It is now clear that at least a large portion of Chapter I will have to await completion of work on the other chapters, since it is going to have to anticipate them. Baker is developing some very interesting stuff on the influence of organizational structure on innovation, especially as concerns the distinction between incremental and discontinuous or breakthrough innovations. I think this is going to be a very important contribution.

It now may well turn out that we are going to have to request an extension (with no additional funds) if Morris' physical problems persist much longer. We certainly will have to if his condition requires surgery. I will keep you posted, and if such a request becomes necessary I will make it well ahead of the deadline.

Sincerely,

Patrick Kelly, Head
Department of Social Sciences

Xc: Jerry McPherson, ORA ✓

Monthly Progress #8

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
SOCIAL SCIENCES

March 1, 1974

Ms. Mary Ellen Mogee
Office of National R & D Assessment
National Science Foundation
Room 700
1325 K Street NW
Washington, D.C.

Dear Mary:

This is a project progress report for the month of February. You have of course received my letter requesting the 90 day extension on our project and the permission to shift $5,000 from the "Other Direct Costs" category to personal services. You also have a much better idea of how things are going on the project, since you were with us for the full day's presentation at the AAAS. We were fairly pleased with the session.

I don't remember if I told you while in San Francisco, but Morris Mitzner has now been home from the hospital for about a week. He seems to be making a satisfactory recovery, though I expect it to be April 1 before he is able to return to work. I will be happy to have him back on the project, so I can let him have the diffusion section back.

Fred Rossini has learned that there is a group at NASA-Ames that has been working on the diffusion process for some time. He went down and talked to them while on the west coast, and they are going to send along some materials. We found perhaps more interest in diffusion than any of the other phases. I ended up having long, and very fruitful, conversations with representatives from several groups that are active in the area.

We will get the material from our data bank along to Len as soon as possible. I also hope to be sending you drafts of chapters i - 4 within a couple of weeks.

That is all for this report.

Sincerely,

Patrick Kelly, Head
Department of Social Sciences

Xc: Jerry McPherson, ORA ✓

March Progress Report
G-43-602
Due 4-3-74

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
SOCIAL SCIENCES

April 2, 1974

Ms. Mary Ellen Mogee
Office of National R & D Assessment
National Science Foundation
Room 700
1325 K Street NW
Washington, D.C. 20550

Dear Mary:

This is to report project activities for the month of March.

We have now received the commissioned papers from all of our consultants except Rubenstein and Rosenbloom. Rubenstein has sent us the first half of his paper and the rest should be finished shortly. Rosenbloom called last week to say that his paper was now finished and is being typed, and that we should receive it in a week to ten days.

We have received a number of requests for copies of the papers which we presented at the AAAS. I am enclosing one such request which we found particularly interesting. There seems to be quite a substantial interest developing in the results of our project. This reflects well on the RDA office in selecting a timely topic, and on Mel Kranzberg's reputation in the scholarly community.

An additional spinoff from this project should be noted. Dr. Robert Whelan and I have had a paper accepted for presentation at the May, 1974 meeting of the American Society for Public Administration. This paper "Patterns of Information Flow in the Management of Technical Innovation: A Comparison of the Public and Private Sectors", is based largely on project research, and appropriate credit will be given.

We have sent to you the most recent (semi final) drafts of Chapters II and III. Chapter IV is composed of two major sections: "Transfer from R & D" and "Diffusion". Norm Baker and Fred Tarpley are working on the former now, and Morris Mitzner, with some input from me, is writing us the latter.

Sincerely,

Patrick Kelly, Head
Department of Social Sciences

Encl.

April Progress Report
G-43-602
Due 5-3-74

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
OCIAL SCIENCES

May 1, 1974

Ms. Mary Ellen Mogee
Office of National R&D Assessment
National Science Foundation
Room 700
1325 K Street NW
Washington, D.C. 20550

Dear Mary:

Enclosed are three computer runs on our project data bank. The first gives the author, the journal in which the entry appeared (or the title if it is a book) and the date, page number, volume, and number (if issues are numbered during year).

The second printout is of the bibliography deck (i.e. the information called for under the heading "bibliography card" on the enclosed coding sheet. To take entry one on this run, the first four digits (0002) give the number the article was assigned when first examined. These members do not correspond to the consecutive numbers in the left margin for two reasons. First, the order here is alphabetical, and secondly, far more articles were examined (and thus assigned a number) than were coded. The next four bits (EM07) is the volume (volume 7 of Engineering Management) the next two digits (01) means that this was in issue 1 of EM07. 0020 refers to Page 20., 60 is 1960, and Avery, R is the author.

The third run gives the coding for each item. The first four digits again gives the article number (0002), 0360 means March, 1960 The coding is as indicated. I have transferred the coding on the first article back on to the coding sheet enclosed.

I apologize for the form that this is all in. I am afraid that it will be of little help to you. We have not needed a run of the sort you wanted and thus have not developed a sub-program to give us the mass of data in a more convenient form. I couldn't really ask the student who has been running the computer end of the project for us to develop this sub-routine since we are out of money and he is no longer getting paid. We "under bid" the project rather badly in terms of both time and money and are now limping to its completion by absorbing substantial costs in the Departmental budget and just creating more time at the expense of other things. The faculty is more than willing to operate on this basis since we feel that our final report is going to represent a substantial scholarly contribution, but I can't ask students to do so.

The kind of sorting and labeling mode that our computerized procedure forced us into was a very helpful and systematic way to work through the innovation literature in the early stages of our project. In fact, I don't know how else we could have done it in an orderly manner, but the demands of making an assessment forced us beyond this mechanical procedure. It helped us to access the literature, but to assess it required substantial application of the usual skills of reading, thinking, discussing, writing, rethinking, etc. The data work was of some help in this latter process, but not as much as we initially, and naively thought. This has been especially true since mid-December when we revised the whole structure of our report. In fact, in a sense our first drafts can be viewed as suffering from an over-reliance on the mechanical, linear mode that our computerized procedure brought with it.

We will of course provide all the help we can in making our data bank useful to you and others. But not until after our report is finished, please. We think that we are going to make an importent contribution with this report and need to give it all the time we have.

Sincerely,

Patrick Kelly, Head
Department of Social Sciences

May Report
G-43-602

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
SOCIAL SCIENCES

June 3, 1974

Ms. Mary Ellen Mogee
Office of National R&D Assessment
National Science Foundation
Room 700
1325 K Street NW
Washington, D.C. 20550

Dear Mary:

This is a project progress report for the month of May, 1974.

As I discussed with you on the phone, we have experienced a setback on the completion of Ch. 4 "Diffusion of Innovations." I am now picking up this portion of the work, and anticipate that a first draft should be complete in three weeks to a month. Assuming two rounds of revisions beyond that, the chapter should be finalized by late July.

We have not yet received the official word from the Contracts Office about the add-on grant of $6,500. Work has begun, however, on the additional computer runs you requested. I will be back in touch with you shortly about the format for the cross-classification volume.

The other project elements are going well and should be finalized by late July.

Sincerely,

Patrick Kelly, Head
Department of Social Sciences

June Report
G43-602

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
SOCIAL SCIENCES

July 1, 1974

Ms. Mary Mogee
Office of National R & D Assessment
National Science Foundation
Room 700
1325 K Street NW
Washington, D.C. 30550

Dear Mrs. Mogee:

This is a brief report on our Project (#DA-39269) for the month of June 1974.

The Diffusion Chapter is developing as I projected last month. A first draft is now complete and has been critiqued by the project group. The second draft should be finished in a week or so -- I will send you a copy.

The computer program has been developed for cross-classifying our bibliography. The format will be as we agreed on the phone. I will send you a working copy before the final run.

I will also be sending you shortly final copies of Chapters 1, 2, and 3. Mel has now completed the first draft of Chapter 5, Summary and assessment, which was developed by our problems with the Diffusion Chapter.

You should have copies of all 9 consultants' papers now. This volume is almost ready for the printer.

Sincerely,

Patrick Kelly, Head
Department of Social Sciences

July Progress Report - G-43-602 Due 8-3-74

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

DEPARTMENT OF
SOCIAL SCIENCES

August 3, 1974

Ms. Mary Mogee
Office of National R & D Assessment
National Science Foundation
Room 700
1325 K Street NW
Washington, D.C. 30550

Dear Mary:

Enclosed are Chapters 1-4 of our final report. The work remaining on them before printing is as follows:

Ch. 1 - Only the final typing remains; this is to begin Monday.

Ch. 2 - Final typing complete; requires one more proof-reading.

Ch. 3 - Final typing complete; has not been proof-read.

Ch. 4 - This is the second draft which was critiqued by the group on 8-2-74. Third draft should be ready for review by 8-8-74. Should be ready for final typing by 8-15-74.

In addition, we have critiqued Mel's first draft of Ch. 5 (Summary and Conclusions). Will probably require two more drafts, the first of which should be complete by 8-9-74. Both this chapter and the executive summary have been slowed by the difficulties we experienced with Ch. 4 (diffusion).

Volume II, which contains the nine consultant papers is nearing completion. All the chapters have been typed and proof-read, and five of them are now printed. The printing should be complete by 8-9-74, if we experience no equipment failure (whihc we have had a lot of in the past two weeks). Collating and binding should take about a week beyond that.

I am not sure when the whole thing will be complete, but the sibstantive, scholarly work is almost finished. It is largely a matter of detailed, time-consuming busy-work now. It has been a most difficult assignment for which we badly understimated the time required. I think, however, that we will all be quite pleased with the final results.

Sincerely,

Patrick Kelly, Head
Department of Social Sciences

Enclosures (4)

G-43-602

MID PROJECT REPORT

STATE OF THE ART REVIEW OF UNDERSTANDING OF THE INNOVATION PROCESS

Prepared for

NATIONAL R & D ASSESSMENT PROGRAM
NATIONAL SCIENCE FOUNDATION

By
Innovation Project Group
Department of Social Sciences
Georgia Tech

Co-principal investigators
Melvin Kranzberg
Patrick Kelly

December 1973

Mid-Project Report on State of the Art Review of
Understanding of the Innovation Process

I Introduction

This project is aimed at determining and critically assessing the present knowledge and understanding of the process of technological innovation. By technological innovation is meant the full range of activities from problem definition and idea generation through invention, development, application and diffusion of new technical devices, processes and products.

The project has an analytic and an assessment phase, each of which has two components. The analytic phase involves both the classification and coding of a large body of the most recent research literature on the innovation process, and an in-depth abstracting of the most significant subset of this literature. The assessment phase is concerned with the quality of our understanding of the process, an both the theoretical and empirical levels. A state of the art assessment paper is being prepared by the Georgia Tech Project Group. In addition, more narrowly focused complementary assessments are also being prepared by nine outside consultants who are outstanding research scholars in the field.

This mid-project report covers the first six months of the study, which began in mid-June 1973. Its submission was delayed somewhat beyond the actual mid-point so as to reflect certain conceptual changes which have been under consideration for several weeks, and which have only now been fully developed and received the concurrence of the National R&D Assessment Office. The nature of these changes and the reasons for them will be discussed below.

Co-Principal Investigators:

Melvin Kranzberg, Calloway Professor of the History of Technology
Patrick Kelly, Chairman, Department of Social Sciences

Faculty Associates:

Norman R. Baker, Professor, Industrial and Systems Engineering
Morris Mitzner, Professor, Department of Social Sciences
Frederick A. Rossini, Assistant Professor, Department of Social Sciences
Fred Tarpley, Professor, Industrial Management
James E. Brittain, Associate Professor, Department of Social Sciences
Robert K. Whelan, Assistant Professor, Department of Social Sciences
Ms. Frances Kaiser, Technical Information Specialist, Georgia Tech Library

Consultants:

James R. Bright, University of Texas
Thomas P. Hughes, University of Pennsylvania
Simon Kuznets, Harvard University
Edwin Mansfield, University of Pennsylvania
Everett Rogers, University of Michigan
Nathan Rosenberg, University of Wisconsin
Richard Rosenbloom, Harvard Business School
W. Paul Strassmann, Michigan State University
Albert Rubenstein, Northwestern University

Graduate Research Assistants:

N. J. Norman
R. David Howell
Russell A. Zimmerman
Taylor E. Little
Lu Ann Sims
Mary C. Martin
M. S. Clark

Farah Eslami
R. D. Green
Mary J. Nelson
Carlos E. Seminario
L. O. Cox
Duncan Wood

II Project Outputs to Date

At this point in the project the outputs are as follows:

1. Six working papers prepared by the Georgia Tech Innovation Group.

2. Five of the nine papers being prepared by our consultants.

3. Critiques of working papers by several of the consultants and by the RDA Office.

4. A revised outline or structure for the state of the art assessment prepared by the Tech group.

5. Approximately 2000 fully coded literature items in our data bank.

6. Approximately 125 detailed abstracts of selected literature items.

Outputs 1-4 above are enclosed with this report. The computer printout for output 5 is not enclosed since it is not fully useable by the reader without the bibligraphical index. This index exists at the moment only in the form of 5X5 cards. The final form of this bibliography will be alphabetical and has not been prepared since items are still being added to the system. A detailed explanation of the literature search and coding procedures, and a sample coding sheet are provided, however in appendix I. Finally, samples of the abstracts are also enclosed. Each of these outputs are discussed below.

1. Georgia Tech Working Papers

As indicated in our original proposal, for the analytical purposes of our working papers the process of technological innovation was treated as linear and broken down into five phases:

1. Problem Definition and Idea Generations
2. Invention
3. R & D
4. First Application
5. Diffusion

In addition to a working paper on each of these process phases, there was also an introduction essay. It was recognized that this linear model was subject to at least the following weaknesses:

1. There are non-linearities involved in the process, both in terms of feedback loops and phase overlap.

2. That the same or very similar exogenous influences impinge on and modify several or all of these process phases.

3. That other models or ways of conceptualizing the process exist.

Nevertheless, the linear model had the advantage of providing a simple starting points for "mapping" the whole process in terms of the topology revealed in the literature. In addition the manner in which the literature items were coded was sufficiently rich and flexible to permit modification of this simple linear structure in subsequent drafts. Thus this straight-forward vehicle was employed in the working papers.

The critiques of these working papers, as provided by the RDA Office and our consultants (see Appendix II) pointed up all three of the above noted weaknesses. As a consequence of the first two the papers were overly long and taken together, highly redundant. They overlapped one another since the phases are not clearly distinct, and since each author attempted to treat the feedback characteristics of the process. They also tended to cover the same ground in the sense that many of the same kinds of exogenous considerations were treated in each phase. It was also pointed out by our readers that other models or approaches to the process needed to be considered.

In addition to these criticisms which were the fairly direct result of our preliminary assumption of linearity, there was the criticism, especially from the RDA offices that our working papers were quite weak on assessment, this was certainly not intended, but reflects our preoccupation at that stage with "mapping the terrain" and being sure that we had given the literature a fair and balanced treatment. It is certainly a weakness, however, and one which we shall try to correct in subsequent drafts.

Perhaps the most telling weakness in our linear approach was revealed by the difficulty we had in treating two of the phases we had identified, "Invention" and "First Application." These cuts turned out, by and large, to be too fine to be supported by the literature when taken in conjunction with the other phases to which they were linked. What could reasonably be called "invention" tended to be eroded in our structure by "problem definition and idea generation" on the one side and by "R&D" on the other. As a result this working paper tended to be largely concerned with factors exogenous to invention itself. It dealt with the various contexts: human and social organizational, informational, technological and economic. It also treated the various measures of inventive activity that have been proposed. And finally it dealt with the incentives and controls of invention activity, such as the U. S. patent system and proposals for its improvement. These are all important considerations, but they needed treatment in another context.

The consequences of our process phase distinctions for "First Application" were quite similar, but in some ways more severe. The movement of an innovation towards the marketplace involves not a single decision or even a clearly defined set of them. Rather, it is an incremental decision process that is in fact deeply imbedded in the prior phase that we had called R&D. And on the other side, the subsequent life of an innovation we had placed in the diffusion phase. The conceptual difficulties inherent in dual erosion of this phase led to real difficulties in the writing of the working paper, and it was in fact the last one finished. It too focused almost entirely on "environmental" consideration, i.e., firm size, market structure, patent and anti-trust policy, etc.

The working paper on the R&D phase, as one would expect from the above, was the longest (80 pages single spaced) and even at that length it was very tightly written. A quarter of it was devoted to the same exogenous considerations everyone else felt compelled by our structure to treat. In addition it discussed idea flow in R&D, project selection and resource allocation, and motivation and performance variables.

The problem definition idea generation paper also focused on the patterns of information flow, as concerns information about both market needs and technical capabilities. This material overlapped somewhat material that had been included in the R&D paper. The flow patterns were treated as modified in their effect by social psychological variables having to do with supervisory authority patterns, primary social groups, and individual mind-sets.

At the other end of our linear process was the diffusion phase. This paper grappled with the complexities that arise in the literature as a result of the specialized interests of diffusion researchers. Among other approaches, it examined diffusion in terms of the sociocultural resistance it often encounters; in spacial terms, in terms of economic and technological factors; and in terms of the role of social contact networks. The basic reaction of our critics to this paper was that it failed to synthesize the material it treated, and was short on an assessment of the state of the art. An attempt was made, however, to establish the existence of feedback loops to the other process phases, especially the first.

To round out our brief summaries of these first draft papers let us turn to the introductory essay which was entitled "The Ecology of Innovation". This was a rather broad brush historical overview of the process of technological innovations, the macro-theories, cultural and institutional determinants and modifiers, science-technology interactions, and several brief illustration case studies. Our reviewers aften remarked on the eloquence and erudition of this paper, but they also found it overly long, and probably more loaded with historical examples than is required for our purposes.

In summary, the working papers which constituted a major part of the preliminary output of this project were in fact guilty of the criticisms mentioned earlier. But on the other hand these were working papers and as such served their purpose quite well. They did enable the project group to analyze the literature relative to the whole process. That this was done in terms of an overly simple model and with the resulting redundancies and overlap does not constitute a lead tradeoff. The material was largely in place to be refined and reworked on a more adequate manner. The assessment deficiences, while quite real, could now be remedied.

But the needed reworking called for a modificaion of the linear model on which we had relied. This need had already become clear to us, but was reinforced by the criticisms supplied by the RDA Office. The Project Group spent a great deal of time in November and early December developing a new structure. This structure has now emerged and has been recently approved, with some modifications by the RDA Office. The suggested changes have just been received and the Project Group has not had time to consider and respond to them. Except for these relatively minor modifications, however, the restructured state of the art assessment will take the following form:

Chapter I - Introduction

THE ECOLOGY OF INNOVATION

I. Problems of Definition

A. Popular Notions

B. Scholarly Distinctions
Invention, Discovery, Innovation.

C. Typology of Innovation
Technological, Social, Adaptations, Alternatives
Contextual Types: Independent, industrial R&D; public sector; mixed public-private

D. Types of Definitions
1. Technical, economic, geographical social, etc.
Dependence upon investigator's interests, and points in process where applied

2. Phase Models and Idea-Flow Models

E. Descriptors of the Innovation Process
1. Idea-flow: technology as knowledge (know-how)
2. Novelty and creativity
3. Interaction between ideas and socio-politico-economic environment (and institutions)
4. Interaction between individual and contextual environment
5. Change

II. Theories of the Innovation Process

A. Historical theories
1. Great-man theories
2. Deterministic
3. Composit - Usher, Schumpeter

B. Defects in traditional theories
1. Dependence on investigator's special interests
2. Theory versus empirical data
3. Inapplicability of a linear-sequential analysis to a complex and dynamic ecological system

III. Innovation as an Ecological Process

A. Complex interactive relationships among the different phases
  1. Theory
  2. Practice -- Empirical case studies

B. Diffuse nature of the decision-making process in innovations

C. An Ecological Model of the Innovation Process

IV. Scope of Our Study

A. Focus on ecological elements, contextual characteristics, and decision points

B. Design of the project

C. Specific questions to which the project addresses itself

CHAPTER II

DETERMINANTS AND MODIFERS: THE SOCIETAL CONTEXT OF INNOVATION

I. Scientific and Technical Knowledge
   A. Science-Technology Interaction: relation between scientific knowledge and technological innovation
   B. Technological Readiness
   C. Technological Imbalances

II. Social and Human Factors
   A. Individual Creativity and its Role: the great man theory
   B. Social Determinants
      1. Social Needs, e.g. security, health, welfare
      2. Social perception of innovation and reaction to innovation
   C. Resistance/Acceptance of Change: social and individual levels

III. Economic Factors
   A. Economic Endowments: labor, capital, resources
   B. Demand and Induced Innovation
      1. Schmookler's demand thesis
      2. Factor saving in induced innovation
   C. Firm Characteristics
      1. Size
      2. Structure
      3. Market signals
      4. Factor saving
   D. Market and Industry Structure including cross Industry comparisons
   E. International Considerations

IV. Public Policies and Institutions
   A. The Patent System
      1. History and Purpose
      2. Problems and Proposals for improvement
   B. Anti-Trust Policy
      1. History and Purpose
      2. Interaction with Patent System
      3. Role in Innovation
   C. Federal R&D Expenditures
      1. Purpose, Trends and Areas of Interest
      2. Impact on the innovation process
   D. Political Priorities Affecting Innovation
      1. Policies: e.g. environmental, safety, health, welfare
      2. Policy Conflicts: e.g. energy and ecology

V. Conclusions and Recommendations

CHAPTER III

THE PROCESS OF INNOVATION: THE INDIVIDUAL AND ORGANIZATIONAL CONTEXT

I. Introduction

A. Reference to Theories of Innovation

B. Development of Institutionalized Innovation

1. Increase in Importance of Institutionalized R & D

2. Different Patterns of Development

a. Industrial Pattern Within the Firm
b. Agricultural Model
c. Contract R & D Firms
d. Bureaucratic R & D: Control of In-house and Contract R & D

3. Influence of the Patent System

C. Organizational Setting: Differences at the Laboratory Level

1. To whom report ---- organizational location
2. Corporate vs. Divisional
3. Functions Performed in R & D
4. "Leader" vs. "Follower" R & D
5. "Offensive vs. "Defensive" R & D

II. The Individual Inventor/Entrepreneur

A. Inventor, not Entrepreneur

1. Psychological Characteristics, Traits
2. Socio-economic impingments
3. Illustrative cases: Watt, Shockley, Carlson

B. Entrepreneur, not Inventor

1. Psychological Characteristics, Traits
2. Socio-Economic Impingment
3. Illustrative cases: Bolton, J. D. Rockefeller

C. The Inventer/Entrepreneur

1. Illustrative cases: Edison, Land, Sperry
2. The spin-off phenomenon: New technologically based firms

III. Organized R & D: Within the Laboratory

A. Approaches, Definitions

1. Distinctions based on degree of uncertainty and extent to which R & D effort is focused: Phase Model.
2. Distinctions based on Behavior of R & D Personnel: Information Flow, Model.
3. Integration of Phase and Information Flow Models —— Idea Flow takes place in all phases
4. Innovation in the Firm, but not in R & D (Hollander)

B. Problem Definition and Idea Generation

1. Integration of Phase and Innovation Flow Models with Social-Psychological Constructs

    a. Differential Impacts of Mechanistic and Organic Systems
    b. Compliance Patterns and Alienation
    c. Supervisory Authority Patterns
    d. The Primary Social Group
    e. Individual Traits and Biasing Sets

2. <u>The Flow of "Need" Information</u>

    a. Current Econ and Social Utilization
    b. Market Needs
    c. Market Information Gatekeepers
    d. Impact of Organizational Needs, Aims, Strategies
    e. Subjective Perceptions of Needs

3. <u>The Flow of Technical Information</u>

    a. Current State of Technical Knowledge
    b. Technical Means
    c. Technical Information Gatekeepers
    d. Range of Technical Options Considered

4. <u>Post-Idea Generation Filters and Linkages</u>

    a. Proposal Submission Decision
    b. Proposal Disposition Decision
    c. Project Status

C. R & D Project Selection and Resource Allocation

1. Descriptive Literature: The R & D Project Selection Problem
2. Estimation Problems: Uncertainty, Cost, Time to Completion, Trade-offs
3. Normative Models

    a. Review of Reviews
    b. Benefit estimation methods
        (1) overview
        (2) recent advances
    c. Decision Models: recent structural considerations
    d. Summary and Discussion

4. Research Opportunities

D. Motivation and Performance in R & D

1. The Variables
2. Performance of Scientists in Organizations
3. The Role of Supervisory and Management Behaviors
4. Research Opportunities

E. Utilization of R & D Output Within the Firm

1. Transfer of Output to Other Functional Activities
2. Project Control and Scheduling

F. Assessment of Research Gaps and Needs

Chapter IV

APPLICATION AND DIFFUSION: THE UTILIZATION CONTEXT

I. Transfer of R & D Results to Application

   A. Organizational Variables and Boundary Problems

      1. Restraints
      2. Modifications

   B. Other Considerations

      1. Resource Variables
      2. Market Variables
      3. Production
      4. Learning and Redesign From Early Experience

II. Diffusion

   A. Scope and Definitions

   B. Specialized Approaches

      1. Socio Cultural
      2. Spatial (Geographic)
      3. Economic
      4. Informational
      5. Social Networks

   C. Special Problems

      1. Multinational Corporations
      2. Public and Private Sector Transfers
      3. Unions, Trade and Professional Associations

   D. Synthesis of Specialized Approaches

   E. Research Needs and Opportunities

III. Problems of Measurement

A. Technological Change and the Growth of the Economy

1. "Residual" models
2. Problems associated with this approach

B. Technological Change and Economic Growth in Industries

1. Results
2. Problems of Industry Studies

C. R & D Expenditures

1. Input Data

a. Definitional Problems
b. Choice of Input Variables
c. Weakness of Data

D. Output Measures and Estimates

1. Kuznet's measure of technical and economic potential
2. Technology assessments

E. Potent Statistics

1. Results
2. Problems associated with the use of potent statistics

In essence this revised outline is designed to accomplish the following:

1. It gets us away from our heavy reliance in the working papers on a linear model of the innovation process. It is now blended with an information flow model in a way that permits more direct treatment of feedback patterns.

2. It takes consideration of exogenous factors which were scattered throughout the working papers and treats them more systematically and concisely in a single chapter (II).

3. It represents a considerable shortening of the introductory chapter.

4. It eliminates the badly eroded process phase "Invention" by blending most of the material covered in that working paper into Chapter II.

5. It thus permits the combining of the "Problem Definition and Idea Generation" material with the material on "R&D". This better reflects the treatment of these topics in the literature.

6. It separates material covered in "First Application", with source going to form a part of Chapter II, and the remainder being combined with the "Diffusion" material. This latter move creates a much more natural conceptual unit.

It should be noted at this point that the above outline will come to be supplemented by a fifth chapter in which the assessments made throughout will be brought together and summarized. In addition, this final chapter will also pickup and highlight the recommendations for further research that have been made in each chapter. This chapter will, of course, be written last. The timetable for its development is presented in a later section.

2. Summaries of Consultants Papers Received to Date

At this point we have received papers from five of our nine consultants; Kuznets, Rogers, Mansfield, Rosenberg, and Strassmann. Copies are enclosed. We have been in touch with the remaining consultants and should be receiving their papers within the month.

Let us indicate briefly the focus of the papers we have received.

Edwin Mansfield: "The Economics of Industrial Innovation: Major Questions, State of the Art, and Needed Research".

Professor Mansfield focuses on "...the innovation process in the individual business firm, particular attention being devoted to the effects of the organization and decision-making procedures and forecasting techniques of the firm on the innovation process. Also, attention will be devoted to the effects of industrial organizations on the rate of innovation, and to the extent to which industrial innovations can be forecasted." He also discuses and evaluates the kinds of research conducted by economists and others on these topics, and describes the sorts of research that he feels is needed in the future.

Nathan Rosenberg: "Technological Innovation and Natural Resources: The Niggardliness of Nature Reconsidered."

Professor Rosenberg addresses the basic economic question of, "how the resource endowment constrains the production of goods and services." He traces the pervasive influence of classical economic tradition developing from the Malthusian-Ricardan position and concludes that it has resulted in, "a most exaggeration of the importance of natural resources and an overstatement of the constraints which they imposed on an economy's development possibilities." This tradition has failed to "recognize how profoundly technological changes required a redefinition of the economies meaning of the natural environment." It has also, "ignored a whole range of additional adaptations which are a mixture of pure technological change, redesigning and substitution."

Professor Rosenberg's conclusion, "is not that population growth, pollution, and increasing scarcity of key natural resources are unimportant problems, or that technology may be confidently relied on to provide cheap and painless solutions." Rather it is that the Malthucian models, "define the problems incorrectly, and that they divert attention from more "modest" but genuine questions." A sampling of such questions is offered.

W. Paul Strassmann: "Assessing the Knowledge of Innovations in Neglected Sectors: The Case of Residential Construction."

Professor Strassmann argues that research on technological innovation has neglected certain sectors of the economy. These are not necessarily the sectors that have had the fewest technological advances, but have often been those where changes has been difficult to measure and thus difficult to analyze at an abstract level. The true output from industry should not be seen as a flow of objects, but as the flow of services from the accumulating stock of such objects. The problem with studying technological change in the neglected sectors, e.g. medical care, education, postal services, garbage collection, etc., is, "that they are poorly understood in their non-technological aspects."

As an example of such a neglected sector, Professor Strassmann chooses to examine residential housing. In this as in other "neglected sectors" technological change is not just a question of having or not having "barriers."

It is a question of solving technological problems and social organizational problems in terms of one another.

Everett M. Rogers: "Diffusion of Innovations Perspectives on National R&D Assessment: Communication and Innovation in Organizations."

Professor Rogers argues that the classical diffusion model has severe limitations resulting from several of its implicit assumptions, and must be successfully applied in broader contexts than originally intended. One of the most important of these modifications is required by the fact that technological innovations are often diffused to and within organizations. In this paper Professor Rogers attempts a synthesis of organizational behavior research, especially as it relates to "innovativeness", and research results gained by those concerned with the diffusion of innovation. He is especially concerned with the impact of structural characteristics of organization an diffusion.

Simon Kuznets: "Technological Innovations and Economic Growth."

This paper deals with the relations between technological innovations and modern economic growth on the macro level. We shall depart from the above pattern of brief summaries and simply reproduce Professor Kuznet's concluding list of his major observations.

> (a) Technological innovation clearly played a key role in accounting for the rise of product and productivity in modern economic growth, and also induced a major transformation of conditions of work and life.
>
> (b) These transformations were required to channel new technology effectively by organizational changes in the earlier institutions that governed production; and the resulting changes in conditions of work for the active participants were a major element in changing conditions

of life. Thus, technological innovations required social innovations, on the part of would be participants. They also required adjustment to the result by displacement of resources in earlier, and obsolete, uses.

(c) For reasons partly indicated below, the focus of technological innovations shifted over time from one sector of the economy to another, or created new sectors. Their current impact was always unequal on the various sectors, and hence on groups in the economy; and such inequality of impact was itself a social and economic problem that required adjustments.

(d) Because of the combination of conventional economic inputs with required changes in conditions of work and life, and because of the combination of conventional economic outputs with possible non-conventional externalities of technologically-induced economic growth and adequate quantiative gauge of the net contribution of technological innovations to economic growth is still to be secured. There is a question whether such a net measure would be of much value, considering the variety of elements, in both inputs and outputs, that give meaning to the comparison. Yet one may argue that the social valuation of technologically facilitated modern economic growth is high and positive, with the critical reactions reflecting temporary lags in adjustment.

(e) Technologically-induced economic growth, once taken place, may be seen to have effects that stimulate further technological innovation. This occurs largely through the learning by society of past benefits and hence of the effort to allocate more resources and provide more favorable institutional conditions for further innovation; learning by entrepreneurs and inventors of better ways of stimulating successful technological innovations; and, particularly important, the learning, through mass application of recent new technology, yielding new data, new tools, new insights and puzzles to natural science, and helping to widen the base provided by the latter to further technological breakthroughs and innovations.

(f) Economic growth also leads to attaining of maturity in the older fields, through the slowing down of final demand for the products; and may affect the conditions for responsive innovative entrepreneurship in the already established and modernized fields because of the large scale of the firm, and the possible dominance of a few in an oligopolistic an monopolistic situation. There is also the rise in the share of the public sector, which in its non-military (or defense non-related) may be characterized by lesser responsiveness to technological innovation. It is the slowing down of the older sectors, once modernized through technological innovation, that helps to shift the focus innovation to new sectors. It is these shifts to new sources of power, new materials, new types of producer equipment, and, in an important way, new types of consumer goods, that help maintain a high or increasing pace of technological innovation; and a high or increasing pace of economic growth.

(g) The clustering of even major technological innovations into groups of related changes (stemming from a relevance to the same source of power, or to the characteristics of the new material and the like), combined with the interplay between the innovations and the social and institutional adjustments to them, means that we are dealing here with long and complex processes, with a sequence of distinct phases in each. This bears clearly both upon policy considerations and the tasks of prognostication and forecasting the trends.

These summaries have certainly not done justice to the contributions made by these papers, but perhaps they do convey the basic focus.

3. Critiques of Working Papers

The critiques provided by the RDA Office, and our consultants have proved invaluable in pointing up gaps and weaknesses in our working papers and in helping us to think through the needed modifications in our structure. Samples of these critiques are provided in Appendix II.

4. Revised Structure for State of Art Assessment

This has been provided on pp. 9-16 above.

5. Data bank of Coded Literature Items

We have indicated above that while a full printout of the approximately 2000 fully coded items can be generated, it would not be completely informative to the reader without the bibliographical index, which at this point exists only in the form of several drawers full of 3x5 cards. The missing element on the printout is the title of the article or book. The title was not built in to the system for computer storage reasons: we were were already storing 76 bits of information on each literature item. (See coding sheet on page 11 of Appendix I) A full bibliography, arranged alphabetically, will of course be provided at the completion of the project.

To provide an indication, however, of the capabilities of the system and the kind of use we are making of it, below is a copy of a recent printout. In this particular case we requested a listing of all articles that discussed both problem definitions/idea generations and diffusions. It turns out that there are presently 33 such items as follows: (See pages 24-25-26)

For each of these articles relating problem definition/idea generation and diffusion this run provided the name of the publication (journal, book, etc.) in which the item appeared, the author, date of publication, volume and page. Our card file would have to be consulted for the title of the item. Subsequent runs could provide further refinement on this set as needed, using any of the other 70 odd variables desired.

6. Abstracts of Selected Literature Items.

Samples of these abstracts are provided in Appendix III.

```
EXECUTION TERMINATED BY AN ATTEMPT TO READ PAST AN END-OF-FILE.

FORTRAN V ERROR TERMINATION:
  I/O  CALLED  AT  SEQUENCE  NUMBER 000356  OF  MAIN PROGRAM
END    37116 MLSEC
DATA IGNORED - IN CONTROL MODE
```

Run 3

```
@XQT NSF.500A
 TYPE THE NUMBER OF VARIABLES WISH TO SORT ON
 PLEASE ENTER YOUR INFORMATION AS FOLLOWS
 PUT IN INDEX NUMBER , DESCRIPTION NUMBER
 REPEATING UNTIL YOU HAVE REACHED THE NUMBER
 OF VARIABLES THAT YOU WISH TO SORT ON
 NOTE HOWEVER THAT ONLY TWO SETS OF NUMBERS CAN
 APPEAR ON A LINE : ENTER INFORMATION NOW
 THERE ARE   33 ARTICLES THAT MATCH YOUR NEEDS
 TYPE IN THE NUMBER OF ATICLES THAT YOU WISH TO HAVE LISTED
                                                DATE  PAGE  VOLUME  #
 RESEARCH MANAGEMENT
                                                 1 73    29  0016    0
     BY  BRYAN G L

 RAND REPORTS
                                                 8 72     0  1002    0
     BY  BIGELOW J

 RAND REPORTS
                                                12 71     0  0656    0
     BY  BLACK C

 RAND PAPERS
                                                 8 72     0  4893    0
     BY  BREWER G

 RAND REPORTS
                                                10 70     0  0525    0
     BY  OSTRANDER N

 OPERATIONS RESEARCH SOCIETY OF AMERICA, NAT MEETING, 41ST, APRIL 1972
                                                 4 72     0  0000    0
     BY  BAKER N R

 INSTITUTE OF MANAGEMENT SCIENCE MEETING, 11TH, OCT 1970 (LOS ANGELES)
                                                10 70     0  0000    0
     BY  ALBOOSTA C A

 THE ENGINEERING ECONOMIST
                                                 1 64     1  0009    0
     BY  CRAMER R H

 CASE INSTITUTE OF TECHNOLOGY - OPERATIONS RESEARCH GROUP REPORTS
                                                 9 66     0  0000   65
     BY  DEAN B V
```

	4	64	524	0010	0
BY DEVRIES M G					
IRE TRANSACTIONS ON ENGINEERING MANAGEMENT	12	62	170	0009	0
BY HESS S W					
IEEE TRANSACTIONS ON ENGINEERING MANAGEMENT	9	65	103	0000	0
BY NUTT A B					
SCIENTIFIC AMERICAN	2	70	13	0222	2
BY BROOKS H					
GOVERNMENT REPORTS INDEX, JANUARY 72 - MARCH 73 (ON MICROFICHE)	34	7	2	N71M	0
BY					
GOVERNMENT REPORTS INDEX, JANUARY 72 - MARCH 73 (ON MICROFICHE)	20	75	3	PB0M	0
BY					
THE CHANGING ECONOMIC ORDER:READINS IN AMERICAN BUSINESS AND ECONOMIC HISTO	0	68	380	0000	0
BY HAWORTH L T					
JOURNAL OF ECONOMIC LITERATURE	12	70	1137	0008	4
BY NADIRI M I					
JOURNAL OF ECONOMIC ISSUES	12	71	63	0005	4
BY DAVIDSON M J					
SCIENCE	1	72	31	0175	7
BY ETZIONI A					
ADMINISTRATIVE MANAGEMENT	8	69	30	0030	0
BY SHAININ D					
BELL TELEPHONE MAGAZINE	9	69	26	0048	0
BY CLARKE A C					
HARVARD BUSINESS REVIEW	5	69	68	0000	0
BY NORTH H Q					
GOVERNMENT REPORTS ANNOUNCEMENTS, 1971 (ON MICROFICHE)	10	54	1	N71M	0
BY					
R & D MANAGEMENT	10	70	10	0001	1

BY GIBBONS M

NEW SCIENTIST 7 66 154 0031 21

BY COOMBE R A

SPECIAL LIBRARIES 5 63 271 0054 5

BY KNOERR A W

POTENTIAL CIVILIAN MARKETS FOR THE MILITARY-ELECTRONICS INDUSTRY 0 70 89 0000 0

BY WACKERMAN L

POTENTIAL CIVILIAN MARKETS FOR THE MILITARY-ELECTRONICS INDUSTRY 0 70 315 0000 0

BY ULLMANN J

INDUSTRIALIZATION AND SOCIETY (ED HOSELITZ B F & MOORE W E) 9 66 259 0000 0

BY ANDERSON C A

ACCELERATING INNOVATION 0 70 11 0000 0

BY HARRIES T W

SCIENCE 8 73 415 0181 3

BY KNOX W

TECHNOLOGY AND CULTURE 16 59 48 0001 1

BY ALLEN F R

TECHNOLOGY AND CULTURE 14 63 277 0004 4

BY DRUCKER P F

III. Time Schedule for Project Phases

The scheduling of the phases of this project, as contained in the original proposal, was changed by agreement between the R&D Assessment Office and the Georgia Tech Innovation Group before the proposal was officially funded. This change resulted from a decision to hold the Washington Symposium in late January or early February 1974 instead of mid-December 1973. Thus the period of the grant, as awarded, was June 1, 1973 to March 31, 1974.

In retrospect, this was a fortuitous decision on grounds other than those on which it was made. As it turned out, the work scheduled for the initial phase of the project required more time than anticipated. A flow chart of these phase I activities, as originally planned, is provided in figure 1. As indicated, it was anticipated that the 2000 or so literature items could be coded by mid-August. Due to considerable computer "down-time" over the summer and the press of concurrent activities, this task was not completed on time. It is now complete, however, and the delay has not had any undesirable carryover into our current activities. A detailed account of the literature search procedure and coding process is provided in Appendix I.

Our efforts to get critiques of our research design from our consultants during this initial phase was quite successful. Four of the consultants (Kuznets, Rosenberg, Rogers, and Strassmann) attended a meeting in Atlanta in mid-June for this purpose. This meeting was also attended by representatives from the RDA Office as well as by Charles Douds and Edward Wood who were involved in concurrent RDA-funded projects at Northwestern and SRI. Richard Rosenbloom and Tom Hughes met with the Tech Innovation Group later in the summer.

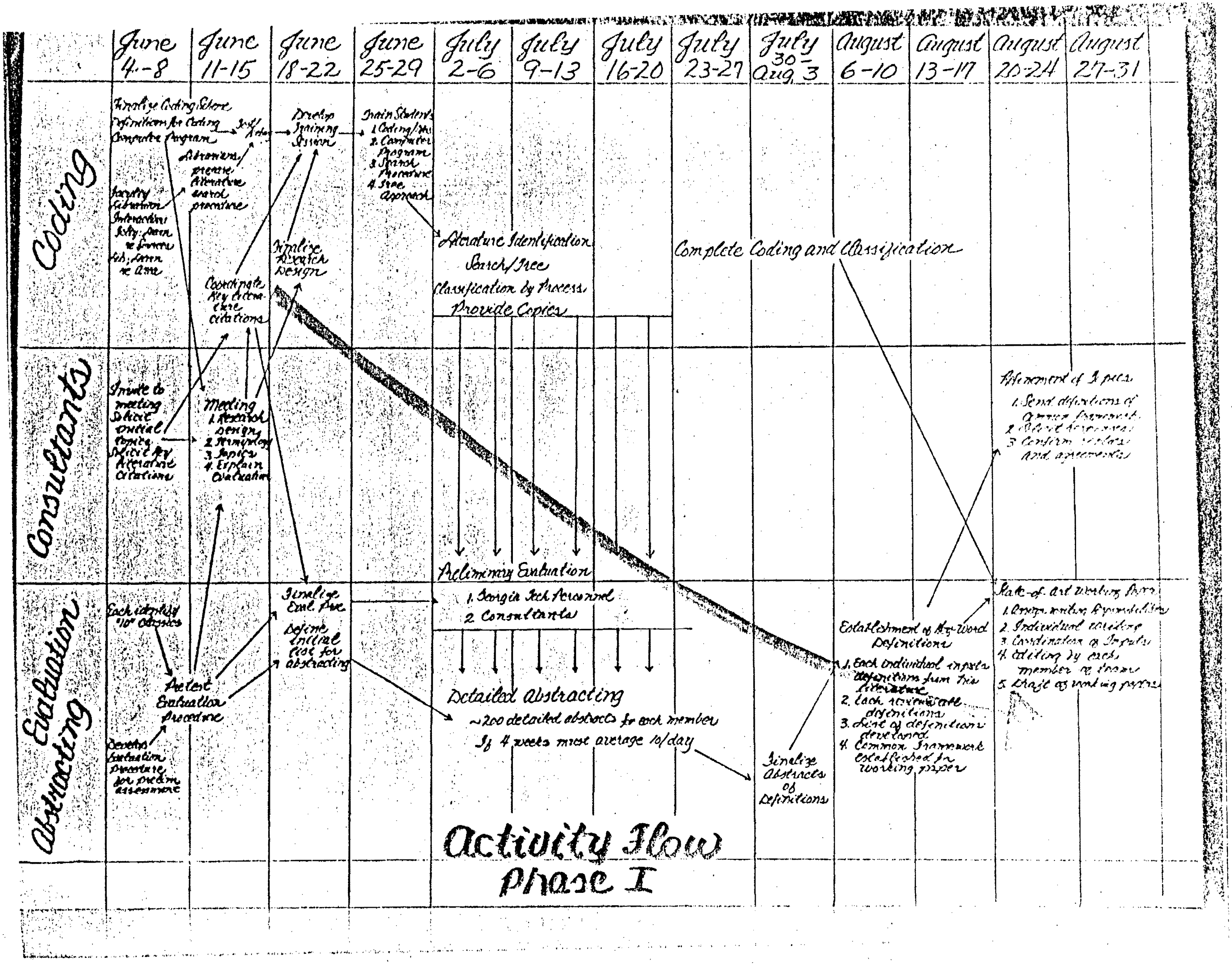

June 4-8
June 11-15
June 18-22
June 25-29
July 2-6
July 9-13
July 16-20
July 23-27
July 30 - Aug. 3
August 6-10
August 13-17
August 20-24
August 27-31
Coding
Consultants
Evaluation Abstracting
Literature Identification Search/Tree Classification by Process. Provide Copies
Complete Coding and Classification
Preliminary Evaluation
1. Georgia Tech Personnel
2. Consultants
Detailed Abstracting
~200 detailed abstracts for each member
If 4 weeks must average 10/day
Finalize Abstracts of Definitions
Activity Flow
Phase I

Figure 1.

The remaining consultants provided their critiques by mail or orally on the occasion of other meetings.

The abstracting of selected literature items proceeded fairly well in the early part of this first phase, but then dropped off sharply as the faculty became involved in the writing of their working papers. This is the only agenda item that we still have not been able to catch up on. We have discussed this problem with representatives of the RDA Office, and are currently working on a solution. We do not view this as a major problem, and it should not effect either the quality of our state of the art assessment or our attainment of a March 31, 1974 completion date.

The main point at which our original schedule for Phase I activities was proven unrealistic is in the projection of September 1 for the completion of our working papers. We felt from the beginning that this deadline was probably too optimistic in light of the diversity and complexity of the literature to be synthesized and assessed. Two of the six papers were completed by that date, however, and a third shortly thereafter. Two more were finished in mid October. The sixth paper suffered badly from conceptual problems inherent in the process moded with which we began, and was not complete until late November.

The difficulties posed by not meeting fully the deadline we imposed was more specious than real. This is the case for two reasons. First the deadline was unrealistic both in terms of the work required, and the extra time that was built in before the project began. Secondly, it worked out just as well to send the working papers to our consultants for critique a few at a time rather than all at once, since that is the way they would obviously be reading them.

At this point in the project the slippages in the Phase I schedule have now all been overcome with the exception of the completion of the abstracts of the most significant literature items. The remaining project events and the deadlines that have been established for them are as follows:

January 10:	Completion of rewrite of papers by Georgia Tech Group under revised organizational structure (paper to be submitted to RDA Office for quick review)
January 17:	Response as needed to consultant's papers.
February 1:	Completion of first draft of Chapter 5 (Summary of State of the Art Assessment and Research Recommendations)
February 10:	Final drafts of papers by Tech Project Group reviewed internally.
February 15:	Rewrite of Chapter 5 due.
March 1:	Final copy of Tech papers and consultant's papers due
March 31:	Final Report to NSF

This time schedule is realistic in light of the acceptance by the RDA Office of the revised structure described above, which enables the incorporation of 75 to 80% of the working paper materials. It will be met, with the possible exception of the abstracts, the completion of which may require a portion of the allowable 60 day report period following the expiration of the grant.

G-43-602

MEMORANDUM
October 9, 1973

TO: Mary Ellen Mogee and our Consultants

FROM: Mel Kranzberg and Pat Kelly (and the rest of the Ga. Tech Innovation Group)

RE: Explanation of Enclosed First Drafts

As you doubtless assumed by this time, we are running somewhat behind schedule in getting the first drafts of our state-of-the-art innovation study to you. Rather than keep you waiting until all the drafts are completed, we are enclosing the three which are available; the rest should be ready in a week and will be sent to you as soon as they can be typed and duplicated.

The three rough drafts enclosed - for your information, comments and criticisms - are Kranzberg's introductory essay (loftily entitled "The Ecology of Innovation"), Rossini's piece on the invention phase, and Baker's study of the R&D phase. Later you will be getting Kelly on problem definition and idea generation, Tarpley on first commercial application, and Mitzner on diffusion.

We apologize for the rough state of these manuscripts. We were eager to set our thoughts down as quickly as possible in order to get the benefit of your critique before "freezing the design." And if Rossini's paper seems unusually rough, perhaps resembling chop suey in places, it is because it was typed by a bright little Chinese girl who has been in this country only a couple of years and to whom English is a second language (but not yet second nature).

Not only are these rough drafts, they are also over-written and overlapping. These would be grave faults if allowed to persist in the final papers; in these preliminary drafts they can be regarded as virtues. Because we feared that some major points might "fall between the stools" and thus remain uncovered in our review and assessment, we decided that each author should put down everything (or almost everything) involving the particular topic he was assigned, without worrying if he was repeating material covered by others. Partly such repetition owes also to the nature of the subject matter: the phases of the process spill over into one another.

Once we have the other preliminary drafts completed and have the benefit of your criticisms, we will cut down on the redundancies and repetitions - and also make some hard decisions about the placement of various elements and the amount of importance to be attached to various points.

Hence we badly need your advice on these preliminary drafts. We want your opinion on structure and style, of course, but we also want to know what you think we have omitted, which items should be stressed more, and which items we have stressed too much and hence can be cut out or summarized more briefly. Indeed, feel free to argue with us in sending us your reactions to these drafts. All your criticisms and suggestions will be given our most serious consideration.

Because of our tardiness in getting these papers to you, we have postponed the due date on the consultant papers from November 1 to December 1, and the meeting in Washington for the final presentation from January or February to March or April.

September 1973
Melvin Kranzberg

The Ecology of Innovation

Popular mythology presents some simplistic notions of the innovative process. Though contradicting one another, each myth focuses on separate aspects of individual and social behavior, of scientific and technical knowledge, and of economic factors. Taken in their entirety, however, the myths comprising the folklore of innovation manifest clearly the complex nature of that process.

We get only a half-truth, albeit a very important one, from the comic-strip version of invention. There the inventor -- who is considered a somewhat eccentric fellow (after all, he walks around with a symbolic electric bulb suspended over his head) -- receives a flash of inspiration (the electric bulb suddenly lights up), and lo and behold, an invention has been born. True, because such cartoons depict the importance of individual imagination and ingenuity in innovation. But not entirely true, because the act of inventive insight is only one in a series of developments which are necessary to produce a successful innovation.

"Necessity is the mother of invention" is another such half-truth. Indeed, the metaphor embodied in that old adage is the clue to its inner contradiction: Except in cases of parthogenesis (undocumented except in ancient mythology), mothers are never <u>solely</u> responsible for begetting children. Besides, necessity by itself can never produce an invention; the "necessity" explanation falls down when we recognize how many felt needs have not yet mothered inventions to meet them, and when we think of many innovations which arose not from any necessity but from other causes. Because in many cases innovations require additional inventions in order to make them work properly, one could perhaps turn the adage around to state: "Invention is the mother of necessity," or, more aptly, "Invention is the mother of the necessity of invention."

Yet there is an important truth in the "necessity" explanation, for it forces us to consider those social needs and human wants which help to formulate those problems toward which inventors direct their attention and to provide the guidelines along which innovative activity is directed.

Another old saw frequently alluded to in superficial studies of innovation is the saying attributed to Ralph Waldo Emerson, "If a man can write a better book, preach a better sermon, or build a better mousetrap than his neighbor, though he builds his house in the woods, the world will make a beaten path to his door." There are many reasons why a better mousetrap might never achieve recognition or successful implementation. Emerson's reputed statement is important because its departure from the facts focuses attention upon several elements in the innovative process: the necessity to link together social needs with inventive activity; the role of the entrepreneur in bringing together ideas, men, money, and techniques to produce a profitable innovation; and the importance of the diffusion of information so that the potential users are fully aware of the existence of an innovation and its potential usefulness to them.

The proverb-makers have not yet framed a quotable phrase for the fourth of our popular stereotypes: the notion that technological innovations derive directly from basic scientific discoveries. If this generalization can be said to have any validity at all, it would hold only for relatively modern times--and even then, as we will see, the situation is simply too complicated to admit of such simplistic views. Nevertheless, the concept that technical innovation drives from scientific discovery exerts great power, underlying government support of basic research on the grounds that such research would ultimately achieve utility.

Now, it is no great intellectual feat to destroy old adages and popular myths. The very qualities which give them their strength and popularity -- their pithiness and their simplistic imagery -- are also their major shortcomings, for they fail to take into accountcounter cases or to show the complex interrelations which lie at the heart of most human and social endeavors. Taken separately, these four items of the folklore of innovation are meaningless; together, they reveal the complicated interplay of many different factors in the innovative process.

If we must have imagery to help us understand the nature and process of innovation, the nearest metaphor is perhaps to be found in ecology. The innovation process is indeed analogous to the complex interrelations of biological organisms with one another and with their physical environment. Technological innovation proceeds in a social, political, economic, and cultural environment where the parts interact with one another, and where change in one element produces a chain reaction of chanes in the other components. Like biological organisms, technological innovation changes and evolves to meet modifications in its environment. Indeed, innovation itself might be considered as a unique social means devised by man in the overall process of natural selection and adaptation to his environment -- a social and physical environment which previous innovations have themselves helped to create!

The limnologist -- the scientist who studies fresh-water organisms in lakes, ponds, and streams -- is aware that a slight change in water temperature or oxygen or food supply can promote the growth of some species while causing others to disappear or to adapt to the changes. Similarly, by adopting an ecological approach, the student of innovation can see how changes in the knowledge base, in social needs or public taste, in the availability of capital or natural resources or labor, in the structure of industrial research laboratories, in government funding, or in any other of the multitude of vaiables affecting the environment of innovation can bring about changes in the innovative process itself.

Recognition of its ecological nature provides us with a powerful intellectual construct for analyzing the process of innovation, but it complicates rather than simplifies the task. It makes us aware that small perturbations in one element of the ecological system can have mighty repercussions elsewhere. This is no small matter in a society where complex questions of social priorities arise in connection with problems of limited natural resources, desire for economic growth, demands for improvement in the "quality" of life, fears of environmental pollution, requirements for energy, and many other competing "goods." Such considerations force us to ask what kinds of innovation are necessary, and how are we to obtain them. The problem of stimulating innovation cannot be considered by itself but must be related to changing national goals and social priorities.

Throughout history, society has consistently underestimated the impact of technological innovations on the ways in which people live, work, learn, and play. Just as research and development (R&D) attempts to reduce the element of risk in applying new technology, so should a study of innovation reduce the element of surprise in our calculations for the future by indicating where and when government or business might intervene in the innovative process, and what form that intervention should take in order to prove most fruitful.

* * *

An ecological approach to the study of innovation magnifies the difficulties of encompassing its many complex interrelationships.

Lest the complexity get out of hand, we must necessarily delimit the range of our investigations. Strictly speaking, an ecological approach to innovation would include social innovations, those modes of social adaptation, accommodation, or advance in order to reach certain goals or to respond to changes. Perhaps the study of innovation upon which we are embarking here will be the prelude to similar investigations into such social innovations. Here, however, we limit ourselves to technological innovations.

What is a technological innovation? In the simplest term, it is the introduction of a new product, tool, device, or technique. But such an inelegant definition would not correspond to the sophisticated approaches of the different disciplines concerned with innovation. To some economists, for example, an innovation is the introduction of something technologically new which has an impact upon the "production function" of society.[1] Economists would naturally think of innovation in terms of its economic function and impact. To the economist, therefore, innovation requires the introduction of a product or process into the economy; otherwise, it would be economically irrelevant.[2]

The individual business firm is narrower in its view of innovation; it is scarcely concerned with the effect of innovation upon the general economy but rather the effect upon its own balance sheet and its position in the industry. In the case of new food products,[3] for example, these might affect the food costs to the consumer, the profits of farmers, processors, distributors, and retailers, the public health through nutritional changes, the international balance of trade, and the like. But the individual businessman is thinking primarily in terms of profits for his firm.

In some industries, particularly the "high technology industries" such as electronics, competition is so keen that innovation becomes a matter of survival, let alone profits. Changes occur so rapidly in such industries that "innovation is an accepted way of industrial life."[4] Innovation becomes a ding an sich, a thing in itself, an obsession similar to Captain Ahab's pursuit of Moby Dick, pursued without any rational constraints.

Other thinkers regard innovation in technological terms of social goals. Morris Tanenbaum states, "Technological innovation is the novel application of physical knowledge and technique to make premeditated changes in the physical aspects of the environment."[5] This same purposive element is stressed by Peter Drucker who regards innovation as a conscious attempt to bring about, through technology, a change in the way man lives.[6] Such emphasis upon social purposes might explain the end result of innovation, but it scarcely explains the motivations of most innovators or the ways in which the process actually occurs.

There are other complications in defining innovation. Although the word carries the root for the words "novel" and "new," many innovations simply involve a combination of old elements -- which in combination create something new. Sometimes, the novelty resides in the context. For example, M. T. Hodgen considers a technical innovation "as having taken place when a tool, a device, a skill or a technique, however unknown or well-known elsewhere, is adapted by an individual in a particular community and is regarded as new by the members of that community."[7] This contextual definition is important in studying the diffusion of innovation; it is fundamental for cultural anthropologists and developmental sociologists, yet it scarcely defines technological innovation in terms of our broader interests.

If we leave aside questions of motivation, impact, and context, and the different views taken by different disciplines, we can employ a very useful simple functional definition: By technological innovation we mean the full range of activities from idea generation through invention, development, application, and diffusion of new technical devices, processes, and products.[8]

This definition embodies a congeries of activities, some of which are sometimes identified as the innovation process itself. For example, the idea of invention is sometimes used to describe the entire innovative process. Joseph Schumpeter distinguished between the two: invention he regarded as the initial event, the discovery of a new tool or technique; innovation is the final event, when the new tool or technique is generally implemented.[9] Inventions might be technologically new, but it they are not applied, they would not be classified as innovations. For example, Leonardo da Vinci designed a flying machine, but it never flew -- and it could not have flown had it been built; the real invention of the airplane came with the Wright brothers at the beginning of the 20th century, which was both an invention and innovation.

One other distinction might be useful in order to clarify our definition, and that is the distinction between invention and discovery. For example, R. J. Forbes claimed that scientific discovery usually recognizes or observes some new natural object or phenomenon, while an invention is the creation of something technologically new which had not existed before in nature.[10] For example, man discovered fire but invented devices to start fire and use it for lighting and heating.

But such a distinction between discovery and invention is surely superficial. Frequently what we call inventions could also be classified as discoveries. For example, Charles Goodyear discovered that rubber when sprinkled with sulfur and heated would not melt and would retain its resiliency. Although this was an accidental discovery, it was also the invention of the vulcanization process. Frequently invention and discovery are so closely intertwined that it is difficult to draw any clear-cut distinction between them; for example, Edison's "invention" of the electric lamp was based partially on his "discovery" that a carbon filament possessed physical properties which would enable it to incandesce in a bulb from which the air had been removed -- and this itself was a "discovery" obtained by trial-and-error of many different possible filament materials. Discovery in this sense would be merely one of the broad range of activities comprising the innovative process.

* * *

Our functional definition of innovation as a wide range of activities dictates the analytical framework to be employed in studying the process. Sorting out the activities in a logical -- and sometimes chronological -- continuum, we arrive at five phases in the innovation process: (1) problem definition and idea generation; (2) invention; (3) research and development (R&D); (4) application; and (5) diffusion.

Although each process phases is complex in itself, we have, for analytical purposes, provided them with simple definitions. Our first phase -- problem definition and idea generation -- is in a sense a "knowledge" problem. A need (social, economic, or simply technical) is perceived or an opportunity (perhaps some new scientific discovery or another technical innovation) presents itself -- and the inventor is able to link together these needs and opportunities in an idea for some technical improvement. Invention -- the second phase -- is the creation of a new technical device, product, or process which attempts to link together the needs and opportunities. Since the new device, process, or product might not be able to be directly employed -- the invention might be only a working model, a patent drawing, or the like -- further work of various kinds must be done on it. This involves research and development -- our third phase -- until the invention is ready for application. Application involves decisions regarding risk-taking, market evaluations, and a whole host of other considerations. When it is finally applied, the innovative process is completed when the technological invention is diffused into different firms throughout industry, into other industries, and into other countries.

The five phases might seem clear and distinct in definition, but they are anything but that in practice. Indeed, because the innovation process is an ecological phenemenon, there are feedbacks among all the elements of the process phases. For example, during the course of research and development, it might be discovered that certain technical problems present themselves, and sometimes these in turn involve the acquisition of more basic scientific knowledge. In other words, new problems are defined, and ideas must be generated in order to create new sub-inventions, which in turn are subjected to further research and development. Similarly, in the course of application or in diffusion, new problems arise which also feed back into the system. Because all these elements interact with one another throughout the innovative process, there is much overlap among the individual process phases. For analytical convenience, we separate the phases, but we must always remember that they all form part of the ecological system comprising the many activities and developments which we define as the innovative process.

II. Theories of Invention and Innovation

The major theories of invention -- early theorists made no distinction between the act of invention and the entire process of innovation -- polarize around two positions; deterministic and individualistic ("heroic" or "great-man") theories.

The determinist explanation of the innovative process holds that the innovation occurs when the conditions are "right," whereas the "heroic" theory stresses the role of the individual. To those who think that technology runs its own race, unbridled by any external pressures, the individualistic approach appeals; others who regard technology as bounded and created by social forces would tend toward the deterministic school.

Within these two major theses, there are many variations, and the history of technology suggests that both schools of thought have some validity and are not necessarily mutually exclusive. More sophisticated theories developed later; these are composite theories, embodying elements of both earlier schools of thought.

The position of the deterministic school is embodied in the adage "Necessity is the mother of invention." That necessity can be social or economic need, military demand, and the like.

If need alone sufficed to bring forth inventions, we would have had many innovations long before they actually occurred. Long before the telegraph, telephone, or radio appeared on the scene there was a need for immediate communication over great distances. Similarly, the demand for fast transportation of bulky commodities would have brought about the invention of the railroad and steamboat long before the 19th century. It is obvious that many things besides necessity determine if an innovation occurs, when and where it occurs, by whom it is invented, and the nature of the innovation itself.

But merely to deny that necessity is the sole factor is not to deny its great importance in innovation. Indeed, it is a crucial item, for there is little point in inventing something which nobody wants, needs, or uses. When that occurs, the process is sometimes aborted before the invention is actually applied and diffused. Many "inventions" went through the problem definition and idea generation and well into the research and development phase before they came to a halt. The patent office files are filled with inventions which never saw the light of day; that is, they were patented but they were unable to satisfy some social requirement, such as profitability, and never "made it."

True,some innovations, such as the polaroid camera and cellophane may be said to have been invented before specific user-need was articulated; nevertheless, in the broader sense there has always been the human desire for immediate depiction of a scene and for a clear packaging material. There are many different social needs and human wants to which innovations have responded.

The two most important social stimuli to innovation throughout human history have been military requirements and economic profit. For example, the demand for cannon stimulated discovery and innovation in metallurgy and chemistry, and cannon were themselves a response to military needs. In more recent times the growth of aeronautics has owed much to military requirements.

Wartime developments, more than any other, exemplify the kernel of truth in the adage about necessity begetting invention. Radar was developed in Britain to meet the German bombing threat in World War II. Similarly, artificial harbors, degaussing of ships, blood plasma, the proximity fuse, and many other innovations were direct responses to military needs and demands.[11] Indeed, only the desperate needs of wartime force hard-crusted military chiefs and conservative bureaucracies to try new items and ideas. Wartime necessity often results in "crash" programs which accelerate the pace of innovation.

Economic factors certainly rank with military ones in stimulating and determining the nature of much innovative activity. For example, the development of the water wheel and windmill in medieval Europe has been attributed to the labor shortage caused by the decline of slavery with the downfall of the Roman Empire and the rise of Christianity; these made it necessary to utilize sources of power other than human muscles. The shortage of charcoal for metallurgical purposes led to the innovation of "coke" in the metallurgical process. Similarly, the scarcity and high cost of labor is said to have stimulated mechanization in 19th century America.

Economic requirements can express themselves in different ways in response to different resource factors. Take the requirement for greater agricultural production in the 19th century. In land-short Europe, the emphasis was on developing new crop rotations and applying chemistry to agriculture in order to increase the yield per acre, while Americans, possessing an adundance of land but a shortage of workers, developed machines such as the reaper and combine-harvester in order to increase the yield per man.

Besides looking at obvious manifestations of social need, such as military demand and economic requirements, we can view the deterministic interpretation of technological innovation in more cosmic terms, as part of a Social Darwinian schema of natural selection, adaptation, and survial. From the viewpoint of those anthropologists who regard tool-making and tool-using (to be more precise, tool-dependency) as the determining characteristic of our species, technical innovation is part of a natural selection process which has enabled man to adapt to his environment and to survive. Such a cosmic view of socio-biological needs for survival might explain much technological innovation during "the descent of man" and prehistoric times, but it scarcely accounts for man's later inventiveness. It does little to explain the great surge of innovative activities in, say, the last three centuries, when man's survival on earth is no longer threatened by a hostile nature or by other animal species -- unless we assume a Jungian innateness to innovation in the human psyche.

Akin to the deterministic interpretation of innovation as derived from principles of biological evolution and natural selection would be an argument for innovation in terms of the "challenge-and-response" thesis of Arnold Toynbee. In his multi-volume Study of History, Toynbee considers the rise and fall of civilizations as a function of their ability to respond to challenges.[13] Thus, the challenge of the natural environment sometimes elicits technical responses.

Other external factors -- such as geography and climate -- can lead to a deterministic interpretation of the innovation process. L. Don Lambert claims that the tropical climate in Africa where the earliest civilizations arose was conducive to the growth of internal parasites; those areas eventually stagnated, unable to innovate sufficiently to stem the debilitating effects of parasitic infections. The "coldward course of progress" contined until "a latitude of permanent climate control of parasites was reached."[14] Climate is not the only geographical determinant. The lack of presence or certain natural resources can propel innovation in certain directions. For example, we would not expect the cotton gin to be invented in a colder clime where cotton does not grow!

A deterministic approach underlies some interpretations of innovative capabilities in national or ethnic terms, although such views might derive from the application of pre-existing prejudices to a limited body of data. For example, national pride led various peoples to assert special credit for inventions -- as witness the American boast of a special "Yankee ingenuity"[15] and the post-World War II Soviet claims of Russian priorities to a number of inventions. S. Collum Gilfillan, who pioneered in early studies of the sociology of invention, has recently claimed that various "races" are unequal in inventive ability; he finds that the central Eskimos are very good at invention, while Negroes are the opposite.[16]

There is also a tendency to think of inventiveness as a monopoly of the Western world, which is contrasted with the "backwardness" of Asian and African societies. The fact is that Western man is not unique in technological innovation; at various times in the past Asian and African societies have led the way, while the West has borrowed much from them. It is in Africa and Asia, for example, that the earliest remains of man's tool-making and tool-using prehistoric ancestors are to be found. Indeed, until the beginnings of modern science and industry, East Asia was far ahead of the West in many elements. Chinese priority in the development of gunpowder, paper, block-printing, moveable-type printing, porcelain manufacture, and the magnetic compass is well known, but China was also advanced in other technical matters. Some fifteen centuries before Europe was able to cast iron, the Chinese had mastered the technique; mechanical clockwork began not in early Renaissance Europe but in Thang China; the Chinese were the first to build iron-chain suspension bridges, and the first of all segmental arch structures is Li Chun's bridge of 1610 A.D.; and it is claimed that the Chinese developed the crank for converting rotary to longitudinal motion.[17]

India, Malaya, and Tibet were also important sources for the diffusion of technology to the Western world.[18]

Within the Western world, inventions have come from all different nationalities. Indeed, a look at the backgrounds of "American" inventors would indicate the many different ethnic groups which have contributed to America reputation for innovation. For a long time it was thought that the Russians had lagged behind Western Europe in invention; when Soviet historians after World War II put forth claims for the priority of many Russian inventors, Western scholars scoffed at first. Now, however, most of these claims have stood up in the light of serious historical inquiry. It is now obvious that no European nation has been without its outstanding innovators. Inventive creativity would not seem to be a monopoly of any nation or group of peoples.

Nevertheless, the fact that some nations forged ahead of others gives further evidence of the importance of the socio-cultural millieu -- the social deterministic factors, if you will -- underlying innovation. If Asia was so advanced in science and so efficient in technology between the second century B.C. and the 15th century A.D., why is it that modern science began its meteoric rise in the West at the time of the Scientific Revolution, and why is it industrialization began in Britain in the 18th century? If Russian inventors were so fecund in ideas and devices during the 19th century, why is it that their works never took root and why did Russia lag behind Western Europe in industrialization? The answers to these questions would suggest that, while inventive creativity can be found among men at all times and at all places, the sociocultural factors are of major significance in the complex process of innovation.

The importance of the social element in determining the nature and course of innovation is given added emphasis by the historical fact of multiple inventions. Given the widespread diffusion of technological knowledge in the modern world, the similarity of technical problems, and the apparently universial potentialities of the human mind, it is not surprising that many inventors hit upon the same or similar solutions of technological problems. Indeed, the great amount of patent litigation is proof of the fact that all the factors are "right" for the solution of a problem at that particular time. Robert K. Merton and Elinor Barber have studied intensively some 264 multiple discoveries in science, and have found 179 to be doublets, 51 triplets, and so on, up to two discoveries each of which was made independently nine times.[19] We know of no similar studies of inventions, but we have no doubt that the results would be comparable. Indeed, the number of patent applications which are turned down on the basis of lack of novelty would indicate how often the human mind arrives at solutions to technical problems which others have already thought of -- and patented.

Even given the importance of the social factor, we cannot do without the human element in innovation. We can say in a figurative sense that necessity is the mother of invention or that one innovation spawns another, but in the literal sense, need by itself nor machines by themselves do not bring forth innovations. Human beings have the ideas, define the problems, perform the creative act of producing a device, do the research and development, and finally decide upon the application and the diffusion of the innovation.

The importance of the human element has led some thinkers to postulate a heroic, or "great-man," theory, whereby an individual is given complete or virtually complete credit for a specific innovation, as having come almost entirely from his brain alone.

We like to interpret all history in human terms. History is eponymous, and we seek for heroes to whom we can ascribe ideas and developments.[20] The history of technology has not been immune from this hero worship. One of the earliest studies of inventors -- the great innovators of the Industrial Revolution -- was Samuel Smiles, Lives of the Engineers,. This multi-volume collection of biographies, similar to Plutarch's Lives in its didactic purposes, first began appearing in 1862 and passed through many editions. In his effort to inculcate in his readers the virtues of self-discipline, self-help, devotion to duty, integrity and dogged perseverance, Smiles depicted his heroes somewhat larger than life. Smiles ascribed the inventiveness of his heroes to the standard Victorian virtues of his time.

Although the heroic theory of innovation does not deny the stimulus of economic needs and the influence of sociocultural conditions, it emphasizes the role of the individual "hero" in bringing about innovation. For example, scholars of the deterministic school would argue that 18th century Britain was "ready" for the steam engine, both in terms of economic need and the level of technology, so that if James Watt had not invented the steam engine, someone else would have. They also recognize that it would not have been the same engine and that it would not have come into use under precisely the same conditions.[21] Another example, involving functional equivalents, derives from the field of color television. Here both mechanical and electronic systems were devised to achieve the same end result, namely, the transmission of a colored image which could also be received on existing black-and-white sets. The different means employed in transmitting and receiving a color picture depended upon the individual inventor, but the selection of the electronic over the mechanical system resulted from a number of external factors.

Emphasis upon the individualistic interpretation of innovation activity raises as many complications as does the deterministic approach. For one thing, it involves investigation of the act of creativity, a subject of great complexity, many theories, and little agreement. There is, however, a wide body of opinion which holds that the creative act is basically the same in every field of endeavor.[22] Thus, the creative activity of technologists is similar, say, to the creative work of humanists and artists; some people express themselves in steel, concrete, electric circuits, and other artifacts, while others express themselves in poetry, drama, music, scientific theories -- and even theories of the innovative process! But such generalizations do not tell us what technological creativity is. But what is it?

Methodologically, creativity has been studied from two major viewpoints, which correspond, interestingly enough, to the social-deterministic and individualistic approaches employed for interpreting the innovation process: sociological and psychological.

The psychological approach to creativity tends to focus on forces within the individual, concentrating on such factors as intelligence, personality, and attitudes. The sociological approach, while not denying the importance of those elements, claims that these derive from various types of social background and conditioning. In other words, this is the old "nature versus nurture" argument applied to innovative creativity.

One of the earliest scholars who sought to explain creative genius was Francis Galton, who found heredity a primary determinant of eminence.[23] Other pioneer psychologists also found the explanation for creativity in "native genius."[24]

More recent studies have relegated heredity to a minor role, although not discounting it completely. Ann Roe (1953) showed other factors to be of major importance, such as the intellectual atmosphere of the home, childhood interests, and position in the birth order.[25] Not until 1955 was a conference on the identification of creative scientific talent held, and then scholars placed differing emphasis on various demographic, cultural, religious, and personality attributes.[26] Within a few years, however, it was evident that "profiles" of eminent scientists did not necessarily shed much light on the creative process itself.[27]

While Samuel Smiles had made out his great inventors to be the most reasonable and virtuous of men, some iconoclastic thinkers of the late Victorian and Edwardian eras were endeavoring to show that creativity resided primarily in those choleric and splenetic individuals who refused to adjust to the world about them and who did not adopt its values. Not surprisingly, the psychoanalysts, with their emphasis upon the neurotic and irrational elements in the human mind and behavior, came forth with theories relating creativity to emotional disturbances.[28] More recent investigators, however, have abandoned the popular cliché of linking creative genius with a light touch of madness; they now tend to view creativity and psychological health not only as compatible, but as mutually supportive.[29]

The most recent literature distinguishes among different types of creativity and links these to different kinds of activities and goals. Some creative individuals, especially composers, expressionist painters, sculptors, and writers, are simply expressing their inner states; others direct their creativity to meet externally defined needs and goals; while a third type cuts across both of the first two.[30] In addition, studies are being made of the environment in which the creative individuals work. This approach is of importance in modern technology, where the industrial research laboratory now provides the setting for much innovation.

The individualistic theories of innovation which we have dealt with heretofore have been concerned primarily with the inventive creator of the process, device, tool, or technique. There is another individual whose contribution to the innovative process frequently equals or exceeds that of the inventor; the entrepreneur.[31]

It is the energetic entrepreneur who is willing to take risks, who amasses the capital to finance the invention, who sees the idea through to actual production and introduction to the market place. He is the man who links together the social need, frequently expressed in terms of profit potentialities, with the creative ideas of the inventor; he couples the marketplace with the invention.

Sometimes the inventor and the entrepreneur are combined in the same person, as in the case of Edison[32] or Elmer Sperry,[33] but more often they are different individuals. For example, James Watt, as we will see, possessed remarkable technical ability, but he lacked capital and business accumen; these were supplied by Matthew Boulton, who became the driving force making for the successful introduction of the Watt steam engine. Indeed, many innovations would have been stillborn had no capital been found to make their application effective and had not the entrepreneur brought together the need and demand with the creative ability of the inventor.

In contemporary business organizations, the entrepreneur sometimes takes the form of the manager of R&D,[34] who brings together scientific knowledge, technical expertise, knowledge of the marketplace and of economic constraints, in an effort to produce profitable innovations. Sometimes he is the "product champion", the man who sees the potentialities of a novel idea and who possesses the determination and persistence -- and the salesmanship -- to push an innovative concept through to completion. Not everyone possesses the unique qualities of the entrepreneur, for not everyone has the imagination to foresee the practical consequences of inventions.

For example, Chester Carlson, inventor of the xeroxing process, seemed alone in his belief of the practicality and demand for his process and he took his xerox invention to over 20 companies before he was able to find one which "bought" the idea and proceeded to develop it into a major industrial company and a world-wide communications phenomenon.

Successful entrepreneurs need not have very much in the way of technical background, but they must have a keen vision of the opportunities afforded by an innovation. Robert Fulton, for example, possessed little technical background, but we hail him as the inventor of the steamboat because he was the first to see it across the threshold of profit, learning his technology as he went along. Thomas J. Watson was above all a superb salesman; it was his vision of the marketplace and his willingness to make critical decisions involving risks, rather than any special technical expertise, which moved IBM into its position of technical and commercial dominance.[35]

Whether or not the inventor and the entrepreneur are the same man or different men, it is the entrepreneur who marshalls the resources -- ideas, men, technology, money -- and directs them toward the goal of innovation.

In stressing the importance of both individuals and the social environment in the innovative process, one should not forget the role which happenstance plays in history. In the case of technical innovations, the intervention of chance is called "serendipity" (derived from the name of the Persian god of chance), which means happily accidental discoveries. Two famous historical incidents illustrate serendipity at work in the field of innov tion: Charles Goodyear's discovery of the vulcanization process, and W. H. Perkin's discovery of aniline dye.

These and other cases of accidental discoveries or inventions are not quite so "accidental" as they might seem. If anything, they provide proof that innovation does not occur in haphazard fashion. In virtually every case of serendipitous invention, we find that the inventors were aware of the needs and problems, that they had already conducted persistent and careful searches for what they wanted, and that they were acute and perceptive enough to recognize it when a happy accident gave them their answer. In other words, they could appreciate the significance of a chance occurrence and utilize it for practical purposes. Most innovative advances come as a cumulative result of answers to a series of closely directed questions;chance or accidental observations come as a bonus to the perceptive scientist who has already done his "homework". As Louis Pasteur observed, "Chance favors the prepared mind."

* * *

From what has been said above, it is obvious that none of the monistic theories -- social-deterministic, individualistic-heroic, or even blind chance -- can serve by itself to explain the complexities of the innovative process. It is not surprising, therefore, that composite theories have been enunciated which attempt to take into account the many different factors, such as individual capabilities and the socioeconomic environment, involved in innovation.

Almost half a century ago, the anthropologist R. B. Dixon postulated a triad of factors in the background of every new cultural trait: opportunity, need, and genius.[36] An historical study of the conditions fostering successful R&D, carried on (1965) by the Arthur D. Little Co. for the Department of Defense, found a similar triad of factors behind innovatory weapon systems: a clearly understood need; relevant ideas, information, insight, and experience; and the men and money to push through the job. In brief, these were Dixon's anthropological factors translated into the context of the modern R&D laboratory.

One of the most influential expositions of a theory of innovation which comprises both the individualistic and sociological theories was presented by Abbott Payson Usher.[37] Usher's theory, drawn from Gestalt psychology, regards innovation as a social process consisting of acts of insight of different degrees of importance and at many levels of perception and thought. These acts converge into a mass of syntheses, which Usher analyzed as a genetic sequence of four steps: (1) the perception of a problem, meaning the recognition of a social need and of the problems involved in its fulfillment; (2) the setting of the stage, involving the existence of a body of technical knowledge and of technological and financial capabilities; (3) the act of insight by which the essential solution of the problem is found; and (4) critical revision, in which the newly perceived relations are thoroughly mastered and effectively worked into the entire context of which they are a part.

Usher's theory deals only with the first three process phases of our functional division of the innovative process. It helps account for problem definition and idea generation, and the act of creative invention; and his "critical revision" would now be comprehended in the "development" part of modern "research and development" (R&D). However, Usher's theory neglects the risky economic decisions involved in the application of inventions and their diffusion; yet any complete theory of innovation must take those into account. Usher was primarily concerned with the "act of insight" in the inventive process, probably because of the emphasis which Gestalt psychologists placed on the "Eureka" or "Aha!" phenomenon. He helps tell us how and why an invention takes place, but not how that invention is translated into a true innovation.

Although no generally accepted model of the innovation process exists, it is pretty clear that when one emerges it will view innovation as an interactional process between the individual and the environment, or as a social process in which the individual participates. It is the interaction between the inventive individual and social forces (economic pressures, scientific knowledge, and technical expertise) which is responsible for inventions, and it is the further interaction among these factors, including the complexities of risk-taking and decision-making for the application of the invention, which explain the process of innovation as a whole. Any generalized theory of innovation must take note of all these elements.

This notion of the interactional nature of the innovative process bears out our contention that the process must be viewed ecologically, that is, as a complex network of mutually dependent variables, with changes in one element bringing about changes in the others and feeding back to effect changes in the original mover.

III. The Social Ecology

A. The Sociocultural Environment

We have already posed the question of why China, which did so brilliantly in science and technology between the 2nd century B.C. and the 15th century A.D., failed to develop industrially, and why Russia, with a brilliant array of inventors in the 18th and 19th centuries, lagged behind Western European states in industrialization. We might also ask why Britain took industrial leadership during the 18th century, at a time when France held intellectual and cultural predominance over the European world. The answers to such questions would suggest that while inventive imagination can be found among men at all time and all places, differing sociocultural conditions affect the opportunity for employing this creative capacity for technological innovation.

Human creative genius is not sufficient by itself to produce inventions which will take root in a given society unless a number of other conditions are fulfilled. As Homer Barnett has pointed out, there is a great deal of resistance to change in all cultures;[38] nevertheless, change does occur. Innovations take place, and their ability to take root depends in large measure upon the sociocultural environment and its receptivity to technological innovation.

It is not surprising that the threshold of receptivity to technological innovation has varied from time to time and place to place during the course of history. There have been times when technological change was not regarded as possible, necessary, or desirable; instead, greater importance was attached to, say, religions, literary, or other non-technical pursuits. In such societies, creative minds would not be likely to apply their efforts to technical endeavors. The question then arises: Which types of social environment stimulate technological innovation and, conversely, which inhibit innovation?

Without attempting to survey the entire course of human history, it can be pointed out that the social and economic system of antiquity discouraged innovation. This has usually been attributed to the institution of slavery.[37] Because there was little incentive to improve the lot of slaves, there was little pressure to improve mechanisms in order to save slave labor. When technical problems requiring the application of more power arose, the solution was simply to employ more slaves rather than to develop labor-saving mechanisms. The finest minds of antiquity regarded technical problems as unworthy of their attention. The scholarly tradition thus existed completely apart from the technical tradition in classical antiquity; the man who worked with his hands was regarded as inferior to the ivory-towered philosopher.

The great technical advance of the waterwheel provides an instructive case. Although invented during Roman times, the Romans made little use of it. Rather than harnessing the power of moving water to perform work, the Romans simply used the muscle power of slaves to do the job. However, with the disintegration of the Roman Empire, the supply of slaves declined. Mechanical means were necessary to obtain the power formerly provided by human muscles, and the result was a great growth in the application of water wheels during the Middle Ages.[40] Cause and effect are mixed here: when slaves were supplanted by water power and other mechanical devices, slavery disappeared from Western Europe.

Although, as Lynn White has pointed out, the Christian tradition elevated man to dominion over the earth and all the creatures thereon[41] -- unlike, say, the Hindu tradition of man's subservience to, or at least inclusion in, the natural environment -- this theological justification did not provide sufficient practical impetus to widespread invention. Instead, the medieval social structure, based on hereditary classes, and the emphasis on salvation in the life to come rather than material happiness in this world militated against innovation. The roots of our modern proclivity for innovation go deep into our Western medieval heritage,[42] but the flowering had to await a more favorable sociocultural climate and soil.

During the Renaissance elements of a cultural climate favorable to innovation were strengthened. In addition to the strong currents of Christian renewal, which are frequently overlooked in Renaissance scholarship, there was a tendency toward secularism and an opening of the economic system which allowed enterprising financial and commercial individuals to rise in the social hierarchy. And while much of Renaissance "humanism" looked back to classical antiquity, there was also a strong trend for acceptance of novelty. Innovation is morelikely to flourish in an atmosphere where there is anticipation of change and acceptance of novelty. The Renaissance spirit also helped break the bonds of reliance upon ancient authorities. At the close of the Renaissance, the "battle of the Ancients and Moderns" was ultimately decided in favor of the Moderns.

The transformation in religious institutions and theological approaches, sparked by the Renaissance and manifested in the Protestant Reformation, provided a new foundation for technical advance during the 17th century. Perhaps the most influential study of the sociocultural elements underlying scientific and technological change is Robert K. Merton's investigation of the relations of Puritanism to the Scientific Revolution in 17th century England.[43] According to Merton, the Puritan ethic required the scientific study of nature for the glorification of God and His works, and it designated social welfare, the good of the many, as a desirable goal. Puritanism thus nurtured science as leading to the domination of nature by technological invention. Although some scholars deny that the modern scientific spirit is the offspring of Protestant asceticism, they do not deny the role of sociocultural factors in the origins of modern science but merely substitute a different set of factors for Merton's emphasis on the Puritan ethic. Thus Lewis S. Feuer claims that the scientific movement developed out of a hedonistic-libertarian ethic which was not confined to 17th century England but could also be found in Catholic Venice, Confucian China, and Mesopotamia.[44]

Perhaps even more important is emphasizing the importance of the sociocultural environment for innovation were the thoughts of

Max Weber, which R. H. Tawney applied to economic history.[47] In a classic expression of the ties between religious views and economic development, Tawney argued that the Protestant ethic -- with its emphasis on hard-work, sobriety, and the relentless pursuit of one's calling -- provided religious justification for the amassing of wealth through business activities. Although scholars might disagree as to the role of Calvinism in engendering a capitalist frame of mind, there can be little doubt that capitalist economic theory and institutions were extremely favorable for the growth of a spirit of innovation.

During the 18th century Enlightenment, sociocultural environment for innovation improved still further. The idea of progress[48] began to capture man's imagination; man's enlightened progress was to be seen not only in the fine arts, literature, and religion, but in the technical arts. Indeed, the great Encyclopédie, under the editorship of Denis Diderot, was dedicated to the diffusion of advanced thought and contained large portions describing technical processes and progress. During the 19th century, under the impetus of great industrial growth, human progress and technological advance became indissolubly linked in the public mind.

During the 19th century, except for romantic lovers of the medieval past and some Utopians,both capitalist and socialistic thinkers advocated further technological innovation in order to achieve their respective goals. Despite his castigation of capitalism and the exploitation of workers in the factory system, Karl Marx praised the technological achievements which bourgeois capitalist society had brought about. Indeed, to Marx, the alterations in the means of production -- the great technological innovations of the Industrial Revolution -- provided the indispensable groundwork for the emergence of the proletarian society and ultimately the classless society which he hoped to bring about.

By the time the 20th century rolled around, the identity of technological progress and human advance was established in the popular mind. International expositions emphasized the role which technological innovation had played in bettering man's life. At the opening of the 20th century, when newspapers and magazines were filled with prognostications for the future, much was made of the "fact" that man's technical progress in the future was assured -- based on the assumption that man was moving toward a new Utopia brought about by science and technology.

Mid-20th century American society manifested the epitome of this equation of technological innovation with human progress. Unlike previous cultures, Americans embraced the concept of novelty: advertising campaigns drummed home the idea of "new and (hence) improved." While some social critics might be skeptical of whether the new was ipso facto better than the old, public opinion rarely admitted such doubts.

So great was the identification of novelty with progress that sometimes innovation was sought for its own sake. The introduction of the annual model change in automobiles, instituted by Alfred P. Sloan as head of General Motors, added impetus to this notion. Even though much of the novelty was spurious, because it was new it was considered better. Only in the past few years have numerous critical voices begun to ask "innovation for what?" and to question the impact of technology upon the quality of life. This outcry against "technology" -- an outcry which is as irrelevant as inveighing against, say, history or biology -- must give us pause. Nevertheless, while many contemporary American problems are undoubtedly generated by our previous heedless application of technological innovations on a grand-scale, it is clear that we can only resolve or palliate some of them by the application of more and better technology. While the legitimate concerns of the social critics should cause us to take a closer look at the social and human implications of any new technology which is introduced -- and hence the rise of the movement for Technology Assessment-- the social needs and demands of the people of the United States and the entire globe will require further innovations. Hence the current desire to simulate technical creativity and to develop innovations to meet changing and growing social demands and human wants. Indeed, this present project to review the state of our knowledge concerning innovation manifests the public need for technological advance.

* * *

The widespread acceptance of technological change in contemporary American society does not mean that there are no obstacles to innovation. Sociocultural resistance to innovations has been common throughout history, and still exists.

In the case of biological and medical innovations, objections have been raised on moral or religious grounds. Anti-vivisectionists continue to declaim against the use of animals in medical research, and the application of fluorides to water supplies has provoked more resistance on religious grounds and political grounds of personal liberty than on grounds of health.

More serious resistance to technological innovation occurs when these are perceived as jeopardizing someone's livelihood. For example, when the medieval gilds felt threatened by the introduction of machines which would allow unskilled workers to compete with skilled gild craftsmen, they proceeded to outlaw certain kinds of cloth and machines, although earlier in the Middle Ages the same gilds had welcomed new tools and devices which would enhance the profits of their members.

Not surprisingly, then, a good deal of the resistance to innovation comes from workers who fear that new machines might throw them out of work. An often cited, but misinterpreted, example is the destruction of textile machines by the Luddites in 19th century England.[47] The grievances of the Luddites were not so much against the machines as against a whole series of changes in their living and working patterns; they were not opposed to the machines as such, but the machines were the most convenient and conrete symbols of their distress, which is why they wreaked their frustration against them. In other cases, however, the workers have seen the machines themselves as dispossessing them from their jobs. River bargemen, for example, tried to break the paddle-wheels of Fulton's steamboat because it posed a threat to their livelihood.

When direct action against machines proved unavailing, the workers attempted through labor unions and legislative pressure to ensure their jobs. They have sometimes been successful, as witness the "featherbedding" regulations which require firemen on railroad engines although the passing of the steam locomotive has long since made their job technically obsolete.

During the Great Depression of the 1930s there were frequent complaints that innovations in production machinery were responsible for mass unemployment. World War II and the post-war boom temporarily quieted such complaints, but the advent of automation in the 1950s, accompanied by predictions of wide-scale unemployment, again aroused fears.[48] Nevertheless, except in a few instances, workers' resistance has rarely proved a major obstacle to innovation; the major hindrances to the application and diffusion of inventions arise from factors within the structure of modern industrial enterprise.

Businesses as well as workers can feel threatened by innovations. For example, canal companies in England attempted to prevent Parliament from granting construction rights to railroads which might jeopardize their own transport monopoly and hence their profits. Almost as a corollary, a monopoly position might prevent corporations themselves from introducing innovations. Sumner Myers uses the railroads as a case in point.[49] Because of their monopoly position, they were protected from the prod of competition; as their market deteriorated very slowly, they did not perceive the crisis confronting them and there seemed no compelling reasons to force them to overcome their natural inertia. When the truckers began to take over their business, so that their economic decline accelerated, their inertial habits and their anti-innovative attitudes prevented them from taking the innovative actions which would have provided a proper response.

Sometimes the hierarchical structure of a corporation or an orgainzation forms the major obstacle to innovation. Elting

Morison has highlighted the difficulties of Lt. William S. Sims to convince his superior officers of the advantages of a continuous-aim gunfire control system in the United States Navy.[50] Encrusted bureaucratic attitudes have frequently prevented inventions -- even those whose advantages were easily discernible -- from being applied. Military annals are filled with stories of officcialdom turning its back on such technical items as the tank, air bombers, and the like because of the disruption which these might cause in the existing organization and traditional attitudes.

Resistance to innovation might also come from organizational communication problems. Albert Rubenstein has pointed to evidence of delays in the utilization of R&D efforts because of inadequate understanding on the part of the decision makers who would be responsible for facilitating the applications of such innovations.[51]

Sometimes, big organizations suffer from the "dinosaur" effect -- too large to react quickly and effectively to technological opportunities. Yet their resistance to innovation can also be considered as a self-defense mechanism. Indeed, it has been suggested that in certain industries dominated by only a few firms, they fear innovation which might disrupt their domination. Hence they are deterred from creating new technology or accepting its use.[52] After all, the great corporations have built up expensive production facilities, armies of trained service technicians, large inventories of spare parts, and thus have an enormous investment in preserving the status quo. Naturally enough, the suggestion that all of this be modified or discarded because of innovative changes meets with resistance.

When the corporations within a given industry are successful, they become complacent and resistant to change, and this helps explain why technological innovation sometimes comes from "invaders," outside industries who innovate in the field of the existing group.[53] Thus, many major innovations in textiles within the past half century have come, not from within the textile industry itself, but from the chemical companies who introduced synthetic fibers; the oxygen furnace in steel-making came, not from the American industrial giants but from smaller European concerns, and so on.

Finally, there is the argument advanced by critics of capitalism that the structure of capitalist society gives industrialists a positive interest in suppressing inventions. This argument was fashionable among the socialist critics in the 1920s and 1930s, and they adduced as evidence the fact that only one or two percent of all patents were ever used. Their underlying assumption was that anything new would necessarily be economical and practical, and that if industry were truly interested in providing the consumer with better and cheaper products a higher proportion of patents would be utilized. Whatever the truth of this argument -- and its advocates could never prove that the unused patents would actually have been an improvement or more economical, and there are also other data showing a higher percentage of patents achieving application -- the whole argument became outdated in the period following World War II when competitive capitalistic industry actively sought to stimulate innovations.

To some social critics, any study of the resistance to innovation seems irrelevant. What they deplore is the seemingly unthinking acceptance of all innovation on the part of industry and the public without heed to considerations other than those of the marketplace. They claim -- and deplore -- that there is no truly effective resistance to innovation.

Other writers who approach the subject from quite a different point of view would also agree about the ineffectiveness of barriers to innovation. To them, no innovation which effectively meets social demands or human want can long be detained. Their motto is "There is nothing so powerful as an idea whose time has come." But one can think of many ideas whose time has long since passed, but which have never been applied. The notion of the inevitable acceptance of innovation simply bogs down when tested against the historical facts.

More important is the fact that innovation proceeds in a complex ecological system. Even before we reach the diffusion phase where society can decide to accept or reject an innovation, there are many points where decisions might be taken which would inhibit the progress of innovation. Given the general openness to innovation in contemporary American society, there are still major obstacles to innovation. These are to be found in the decisions made on the basis of economic considerations and in the organizational mechanisms which society has developed for innovative purposes.

The sociocultural environment provides a backdrop upon which the drama of innovation played out. In certain cases, it is a crucial element in determining the nature and course of innovative activity. But now we must go from the macrocosmic to the microcosmic, to look at the specific economic and political elements which form part of the broader sociocultural environment and trace their role in the innovation process.

B. Economic Incentives

Within relatively recent times economists have begun the serious study of economic growth -- and even more recently, the "Limits to Growth" -- and their concern has extended to the role of technological innovation in that growth. By and large they focus on three primary factors affecting economic growth: the quantity of labor and capital; the quality of the labor input; and of particular interest to those studying the innovation process, the growth in our storehouse of technical knowledge.

Except for analytical purposes, it is difficult to separate out the above three elements. After all, the quantity of labor and capital input is itself affected by innovative activities, and the quality of labor is, partly at least, an embodiment of new technical knowledge. The professional segment of the labor force is rapidly increasing its proportion in the total work force, and its growth is a clue to the importance of the knowledge element; it is also the segment which is responsible for most of the innovative activity.

However, an even more fundamental question is involved: What is the role of innovation -- indeed, of technology itself -- in the economic system?

The importance of technological advance in economic growth was first brought to the fore some two decades ago by the quantitative studies of M. Abramovitz[54] and Robert M. Solow.[55] Their researches showed that classical economic theory, which had defined output on the basis of labor force and material resources (capital goods and natural resources), did not adequately explain the growth in American output during the 20th century. A new factor would have to be taken into consideration, namely, the efficiency with which capital and labor were used in production. This focused attention upon the role of technological innovation in raising the quality, and hence efficiency, of the labor and capital inputs, for Solow's findings suggested that some 90% of the per capita increase in output was attributable to technological change.

Since then, many economists have sought to investigate the factors leading to that technological change which affects output so greatly. Of these factors, the one which has dominated their thinking about innovation has been economic demand.

The classical statement by an economist of the view that innovative activity derives primarily from the marketplace is Jacob Schmookler's Invention and Economic Growth.[56] By assuming that the number of patents provides a reasonably good indication of innovative activity, Schmookler was able to quantify the technological process so that it could be compared to other economic variables by statistical methods. His conclusion, which draws on patent statistics from many industries over a long span of time and compares them to various statistical indicators of other economic elements (for example, price, investment) in the same fields, is that inventive activity varies as a function of changing economic incentive or demand.

By treatment of some 900 specific inventions in different fields, Schmookler was able to show that the pattern of inventive effort shifted in response to changing demand elements. While not denying that changes in cost and capabilities of inventing different product fields had some influence on the number of patents, and while admitting that in some cases the increased supply of innovations could have been dominant in affecting the market, he believed that these exercised only minor influence compared to the power exerted by marketplace demand. Innovation, according to Schmookler, arises primarily, if not solely, from marketplace demand.

Granting Schmookler's argument that most inventive activity is demand-induced, we are still left with many questions. Schmookler was indeed a trailblazer in linking together inventive activity (as measured by patents) with the economic variable of demand, but that variable itself contains many variables within it. What induced the market demands -- was it the desire to save labor, save on natural resources, save on capital, etc.? A finer-grained analysis of the relation of economic factors to innovations is thus necessary.

Natural resources -- the abundance or lack thereof -- have long been recognized as affecting innovation. A century ago, W. Stanley Jevons attributed British inventions to "our command of coal."[57] In early American history, the presence of wood resources helps account for innovations in woodworking machines which helped later to develop the machine-tool industry.[58]

The reduction or cutting off of an accustomed source of supply of materials can stimulate the search for innovations to replace the missing ingredients. For example, when war comes, some supplies of raw materials are cut off. Substitutes are sought, and frequently superior products emerge. For example, the LeBlanc process was an outgrowth of the Napoleonic wars, and during the American Civil War, technological changes in the British textile industry were stimulated by the cutback in cotton exports from the United States. In World War II, the innovation of Buna rubber, to replace natural rubber, was extended. At present, when certain natural resources are or will be in short supply, we are attempting to stimulate innovation which will provide us with functional equivalents or substitutes.

Natural resources and raw materials are not the sole economic factors in which savings would be a stimulant to innovation. John R. Hicks emphasized labor saving as the chief stimulus to invention.[57] But the complex nature of the innovative ecological system and the number of different ways in which problems might be defined would indicate that no single factor can generally account for the process.

Hence, later economic theorists do not credit any single factor-saving bias as the source of inventions. Looking at individual businesses where the innovative process is actually carried on, Salter[60] and Fellner[61] find that firms are not wedded to any particular factor-saving bias; instead, they seek for anything which will lower their costs, no matter what factors are involved. In some cases, they might seek to save labor costs, in others to cheapen raw material costs, still others to save on fuel, etc.

Attention paid to different economic factors in inducing innovation is necessary because, while Schmookler was correct in pointing out the role of the marketplace in stimulating innovation, the ordinary messages coming from the marketplace are general, not specific. For example, they do not specify which economic factors should be changed by innovations. Raw materials, labor supply, and other factors provide focusing devices which force the industry to seek innovations along specific lines. The common denominator of all these innovations -- no matter to which factor they are directed -- is the expectation of profit on the part of the entrepreneurs. They see before them the happy prospect of higher profits by an innovation, or sometimes they innovate because they are frightened by the unbearable prospect of being put out of business or at least the threat of losing much of their profits -- by strikes, by cutting off of raw materials, and the like.

Evidence can thus be found for innovations deriving from a whole host of factor-saving induced incentives. Karl Marx, in his Poverty of Philosophy, was certain that the threat of strikes provided the chief stimulus for the industrial capitalists to introduce labor-saving inventions. Andrew Ure, an apologist for the early factory system and thus at the other end of the ideological spectrum from Marx, did not disagree, for he pointed out that the self-acting mule was invented in 1835 by Roberts as a result of a strike of mule-spinners. In 1851 the long engineers' strike in England stimulated James Naismith to a frenzy of innovative activity in order to cut down on labor.

Not all innovations involving the labor factor have been labor-saving devices; some were to reduce labor costs not by saving on the number of workers but to make up for the lack of skill on the part of the labor force. The example of the New England machine-tool builders in the 19th century in innovating devices which "built the skill into the machine," in part as a result of the shortage of skilled workers, springs immediately to mind.[62]

Because so many major technological innovations have involved the labor factor, we should not forget other economic factors which induce innovation. Nathan Rosenberg has reminded us of the importance of the producers of capital goods in the innovation process. According to Rosenberg, the producers of capital goods have financial incentive to utilize various means (marketing, demonstration) of persuading firms to adopt an innovation which they produce. The influence of Schumpeter still makes us look upon the transformation of an "invention" into an "innovation" as the work of the entrepreneur, but Rosenberg claims that, from a technological perspective, this transformation is really owing to the capital goods industry. This is particularly the case where the invention is a cost-reducing process, not a product, and does not have to be marketed to final consumers.[63]

We should also recognize that the economic factors can vary at different stages in the life of an innovation. The idea and working model for power steering for automobiles came rather early in the game, but the introduction of power steering waited for almost a quarter of a century until the industry was sufficiently strong enough to support the high developmental cost of this innovation. Nordhaus points out that in the early stages of the development of a product it tends to be labor intensive, because it is often easier to use labor's flexibility than build a machine to do the task, and it is cheaper to use human labor at this early point when the product's market future is still doubtful.[64] Once the product's future is assured and a large market exists, it becomes economically feasible to design a machine to replace the human worker.

There is also a bias toward economic savings through growth in scale. In certain technologies large-scale devices can increase efficiency and hence make for economy. This is evident in electric generating plants -- which require innovations in order to achieve the economies of scale -- and in petroleum refining. Yet there are some dangers in "giantism";[65] there might also be economies of small-scale, and in some cases innovation would attempt to reduce the optimum size.

Finally, it should be stated that the economic incentive to innovation can come from economic leadership itself. Joseph Schumpeter pointed out that some entrepreneurs sought the advantage of lead time -- that is, producing new items before their competitors could catch up with them -- for once the competitors followed their lead, their profits tended to disappear.[66] Staying ahead of the competition is thus an incentive to innovation for many firms, especially those engaged in "high", or sophisticated technology; they fear that their profits would erode if they lost technological leadership. Corporate pride and prestige may be a non-economic factor, but it has economic roots. In our socio-ecological system, innovation can provide a mechanism for adaptation and survival.

C. Non-Economic Factors in the Ecology of Innovation

Non-economic elements also stimulate and direct -- and sometimes hinder -- innovation. In some cases, as in the case of warfare, the path of innovation winds along much the same trail as some of the factor-induced innovations mentioned in our discussion of the economic environment (for example, the cutoff of regular supplies of raw materials leading to the inventing of substitutes). It is not surprising, therefore, that many of the non-economic elements should overlap with and affect the operation of the economic factors. Not only is this to be expected in any description of an ecological system, but the non-economic factors frequently manifest their influence on innovation indirectly, through stimulation of certain economic factors.

Next in importance to the economic incentives to innovation must certainly be the military stimulus. Throughout history, military needs and requirements have been an extremely active factor in inducing technological change. Warfare -- or "national security" -- seems always to have had a priority upon man's innovative capacities.

In classical antiquity, more ingenuity and engineering skill were applied to the instruments of warfare than to those of peace. Indeed, the term "engineer" as used throughout most of history referred to someone who produced military weapons, devices, and fortifications. Not until the end of the 18th century did John Smeaton introduce the term "civil engineer" to indicate someone who was engaged in civilian rather than military works.

The military stimulus to innovation is so well-known that it is scarcely necessary to expound further upon it. Early warfare of the spear-and-shield type resulted in metallurgical improvements -- a change from stone to bronze to iron weapons -- and the development of mechanical devices (ballista, trebuchet) for siege warfare. Building technology responded to the need for fortification and defense. The switch to cavalry warfare, induced by the introduction of the stirrup into Western Europe,[67] led to metallurgical innovations as the armoured knight became the prime military agent during the Middle Ages. But it was the introduction of gunpowder and the development of cannon which had their greatest impact upon technological innovation in at least two fields of endeavor: metallurgy and chemistry.

Many elements of our modern technology derive from innovations which were originally intended to meet military needs. For example, John Wilkinson's boring mill, designed to bore cannon, was an important factor in making James Watt's steam engine a practicable device. Interchangeable parts, a necessary element in today's mass production manufacturing, arose from the work of Hall, North, and Whitney, in providing small arms for the American Army. The great burst of inventive effort during World War II laid the groundwork for technical developments which have entered into peacetime life: nuclear energy, radar guidance for airplanes, blood plasma, DDT (discovered much earlier in another connection, but not applied as a pesticide until World War II), jet-aircraft, and the like.

It can be seen that the military stimulus to innovation extends beyond weaponry. The development of food canning, for example, was a direct outgrowth of military needs, but it had nothing to do with military tactics. Similarly, the demand of the military for standardized uniforms and vast amounts of equipment encouraged innovation in mass-production methods in many industries. In other words, military demand stimulated process as well as product innovation.

To describe the importance of the military stimulus to technological innovation tells us only about incentives for innovation, but it does not tell us much about the innovative process itself. It is not clear,for example, if the process of military innovation differs from that of innovation for civilian uses. Yet there is some reason to believe that it might. The military market is homogeneous and unified; it would tend toward standardization of processes and products rather than encouraging alternative innovations. In certain highly sophisticated electronic and aerospace fields, the military might proved the sole customer, and this would affect the role which the marketplace normally plays in inducing innovation. Also, in the development of military innovations, cost factors which might be decisive in the civilian economy become secondary to performance characteristics. Insofar as the demand factor affects the nature, course, and rate of innovative activity, the distinction between military requirements and the civilian marketplace might indicate broad differences in the civilian and military processes of innovation. If we pursue our ecological metaphor, the environment of the military system differs from the civilian environment -- hence the parameters and variables entering into the innovative process might differ between the two. Here indeed would be a fertile field for fine-grained investigations.

Closely related to both military and economic factors are those elements of national pride and prestige which have stimulated governments to foster technological innovation. Even before the rise of modern nationalism, princes and prelates encouraged innovation for self-glorification. At the very dawn of civilization, the god-kings of Ancient Egypt, the Pharaohs, erected great pyramids as monuments to themselves and as protection for their bodies in the afterlife; construction of the pyramids required innovatory management techniques in order to marshall the men and resources required to build them. Throughout history, the "edifice complexes" of rulers have helped to launch innovations, just as the competition among medieval bishops to erect bigger and more daring cathedrals gave rise to innovations in construction techniques, in stained glass, and in other religious artifacts. Their motivations were religious too, as in the case of those monastic orders, which equated humble work with reverence for God ("To work is to pray," Laborare est orare), and who pioneered in agricultural and cheese and wine-making techniques in the Middle ages.

The fact that religious motivations do not loom large in the background of today's innovations is a measure of the secularization of modern society. But today's secular religion is manifested in our contemporary skyscrapers, the "cathedrals" of the 20th century. Just as medieval bishops vied for something extraordinary in building monumental houses of worship, so today's corporate moguls -- as well as speculative real estate developers -- sponsor innovatory design and construction to build up the corporate "image." The mania for the tallest building, the longest bridge, the superlative of anything, cannot be explained solely in terms of economic determinism; human pride -- Hubris, if you will -- represents a powerful motivational factor which frequently results in stretching technology to its limits.

Just as individuals seldom do things for only one reason, but usually for a combination of reasons, so have governments and corporations sponsored technological developments for mixed reasons of economic power and public esteem. This became apparent during the mercantilist period of the 17th and 18th centuries, when the emerging national states of Europe sought to ensure their wealth and power by intervening in the economic process. For those states which did not always possess supplies of bullion or access to it (as the Spanish did in the mines of the New World), the road to power lay through the encouragement of commerce and domestic manufacturing. To free themselves from reliance upon foreign goods, governments encouraged the creation of industries within their own countries, sought to develop new techiques employing their own natural resources, lured foreign workers -- with their skills and tools -- to their own lands, and, in gneral, served both to diffuse technology and to innovate new techniques and products.[68]

In the 17th century, under the influence of Francis Bacon's formulation that scientific knowledge would prove useful for mankind, the European rulers founded scientific academies. England and France were first, and the other European states soon followed suit. These were intellectual adornments to any ruler's prestige, but it was also expected that their scientific researches would result in useful knowledge -- and indeed, in their origins and charters the emphasis was upon the technical advances which were presumed to arise out of the investigations and meetings of the scientific academies.[69]

If we doubt the impact on technological innovation of nationalistic competition for pride and power, we need but look at its modern counterpart: the space race.[70] Here, on a grand scale modern nations have emulated the dynastic rivalries of early modern times. They have expended vast sums of money; they have, at least in the case of the United States, renovated and redirected the educational system to provide trained manpower for scientific-technical advance; and the project itself has required and evoked much technical innovation in order to achieve its goals. NASA can point to developments in computer miniaturization, integrated circuitry, telemetry, materials design, and a host of other innovations deriving from or stimulated by the space effort.[71]

In order to justify the expenditure of billions of dollars on the space program, much has been made of the "spillover" or "spinoff" of space technology to civilian uses. Attempts at cost/benefit analysis differ in their evaluations of the economic payoffs of our space efforts. But history is not cost accounting; the space program was undertaken primarily for political reasons[72] and with some scientific and technical rationale. Its immediate payoff has not been economic but in terms of national pride and honor; its ultimate impact upon history cannot even be gauged from this close historical perspective. Yet it demonstrates how non-economic factors, utilizing the economic "carrot" of government funds, can stimulate technical innovations in many different fields.

Not all governmental action involving innovation stems from concern for economic growth, political power, or national pride. Governments perform many other functions to meet fundamental social demands for security and justice and , increasingly in recent times, to preserve the country's resources and natural environment for posterity. Because of the interrelated character of modern society, these non-economic decisions are bound to have an economic impact. In terms of our present study, they influence innovation both directly and indirectly.

One of the first instances of governmental intervention for safety reasons in the United States occurred in the case of bursting boilers on steamboats.[73] In order to eliminate the danger to human lives by frequent boiler explosions, Congress subsidized tests and eventually passed legislation regarding boiler design, materials, and operations. The result was to focus attention upon innovations which would meet the new requirements. Similarly, subsequent legislation to protect the public's safety -- fire codes, building codes, and the like -- have stimulated the search for substitutes or improvements in existing products or processes.

Currently much attention is focused upon innovations to protect the citizens against air and water pollution. Such efforts have a long history, stretching back to ancient regulations regarding water supplies and sewage disposal in order to protect public health. Such measures frequently force innovations. Many of today's experiences might echo that of the Alkali Act of 1863, passed by the British Parliament to do away with the poisoning of the air by massive discharges of hydrogen chloride gas from British alkali works. The Alkali Act required a 90% reduction in HCl emissions, and, in a manner reminiscent of today's conflict, experts had testified to the Parliamentary Commission, first, that HCl gas was not dangerous; second, that the attempt to legislate its removal would result in economic disaster for the industry; and third, that in any case, there was no way of removing it from the wastes spewed forth by the alkali plants. Nevertheless, within a few years after the passage of the Act, sophisticated gas scrubbing towers appeared, which exceeded the act's requirements.

At first, much of the hydrochloric acid produced by the gas-scrubbing towers was taken to sea and dumped, but soon profitable markets appeared for this acid. Thus, "the Alkali Act appears to have stimulated invention."[74]

Today we can see "forced" innovation at work in the automobile industry where frenzied efforts are being made to change fuel requirements and engine design in order to meet government emission standards by a target date. Similarly, innovations in nuclear reactors, pesticides, drugs, food processing, textile fireproofing, plastic containers, and the like are being carried on under pressure of governmental regulations, or the threat of such regulations.

Attempts to do away with urban traffic strangulation and automobile-induced smogs are leading to innovation in mass-transit systems, exemplified by the sophisticated hardware and computer programming employed in the BART system in the San Francisco area. The "urban space race," financed by federal, state, and local governments and regional authorities, is providing a powerful stimulus to engineering innovators everywhere. In a similar fashion, problems of the energy crisis, waste material disposal, and depletion of natural resources have become the object of public concern, which has led to the injection of both private and governmental funds into areas previously neglected by inventors and entrepreneurs.

In such cases, the demand function is both economically and socially induced. Although non-economic factors play an important role in the decision to "do something" about such problems, it seems probable that the major inducement to innovation comes when these non-economic factors are translated into economic incentives. Government action can enable social demand to be effectively felt in the marketplace.

We can view this matter in bookkeeping categories of "internalized" and "externalized" costs. Governmental regulation in the field of pollution, for example, is forcing industry to "internalize" costs which had formerly been paid by the public, either in terms of municipal sewage systems or in terms of a declining quality of the environment (which could not be satisfactorily stated in quantitative monetary terms).[75] When such charges are internalized, the factor-saving bias of industrial corporations should provide a powerful stimulus for innovations.

What about those industries -- public utilities, air transportation, railroads, telecommunications, and the like -- which are directly regulated by the government? The commonly accepted view is that government regulation hinders innovation and decreases efficiency. This negative view of the impact of regulation arises primarily from the failure of the railroads to innovate greatly under the domination of the Interstate Commerce Commission -- it might be right in the case of railroads, although other factors of organizational rigidity and lack of foresight might also account for the shortcomings of that industry.

In the other industries, however, government regulation can and has fostered innovation. For example, some regulatory agencies specify a "fair rate of return" on capital investment, and fix utility rates on that basis. Where profits depend directly on the size of investment, the industry is encouraged to innovate, especially when the new technology requires additional capital plant and equipment. There have been major innovations in air transport and telecommunications -- in spite of, sometimes because of, governmental pressures. William M. Capron and Roger G. Nowl conclude that while regulated industries seem to fall short of reasonable goals in terms of innovation, this fact has not yet been clearly established by scholarly research. Hence they recommend more study on the operation of the regulatory agencies, their impact upon the industries, and the connection between the performance of the agencies and their organizational structure.[76]

The mixture of economic and non-economic factors in the environment of innovation that we have just described is proof of its ecological nature. There is a complex interplay between the elements within an organism and the organism's relation to the outside environment. Since we are dealing with a problem in social ecology, we must do more than relate the individual to the environment; we must relate the individual to institutions which in turn interact with both the individual, other institutions, and the rest of the social environment.

D. Institutionalization of the Socio-Economic Demand

We are concerned with the institutional mechanisms employed by government and business corporations with the direct purpose of fostering innovation: the patent system and the industrial research laboratory (whether private of governmental). Although both of these institutions have as their avowed purpose the promotion of innovation -- in the case of the patent system, for the public weal, in the case of the corporative R&D institution, for private profit -- there is the basic question of how well they do that job, and the further question of how might they do it better. Indeed, it is for the purpose of assessing the contribution of R&D to innovation that this entire project has been undertaken. We turn first, however, to the patent system.

1. The Patent System

Throughout most of history, the process of invention was either ignored, taken for granted, or assumed to be the work of mysterious or divine forces. Thus, classical writers credited inventions to demigods and other mythical persons; Pliny the elder, for example, said that carpentry was invented by Daedalus and the flute by Pan. We do not know the names of the inventors of such basic innovations as the wheel, stirrup, waterwheel, horseshoe, gunpowder, and the mariner's compass, even though some of these occurred during historical times.

Not until the sociocultural climate had changed, beginning with the Renaissance, did the idea gain currency that a technological innovation might be a valuable contribution to society and that its inventor should be rewarded by receiving an exclusive right to the fruits of his achievement. The first known patent was issued by the Florentine Republic in 1421 to Filippo Brunelleschi for a cargo boat, and in 1474 the Venetian Republic enacted the first formal patent law. Soon the idea spread throughout Europe.

The first known English patent for a technical innovation was issued in 1559 to the Italian Giacopo Acontio, who persuaded Queen Elizabeth I to issue a patent for his furnaces and "wheel machines" as protection against copiers and as a reward for his ingenuity. Until that time, the rulers of England had granted letters of patent for monopolies or as other privileges to favorites.[77]

In order to prevent the "give-away" to royal favorites, the British Parliament passed the Statute of Monopolies (1623-24) which was to become the basis for subsequent British -- and then American -- patent legislation. The purpose of this act was to prevent the British crown from awarding lucrative monopolies to royal favorites by restricting the issue of patents to the first inventor of a new technique. In the American colonies there was at first no general patent laws; each patent was obtained by special petition to the governing bodies of the individual colonies. The first one was granted by the Massachusetts General Court in 1641 to one Samuel Winslow for a novel method of making salt; in 1646 the same court granted the first American patent on machinery: a mill for making scythes, devised by Joseph Jenkes. In 1691 South Carolina passed the first general patent law in the colonies.

The sociocultural climate of the 18th-century Enlightment so favored the encouragement of science and technology that the bases for the American patent system were embodied in the United States Constitution, Article I, Section 8 of that document gives Congress the power "to promote the progress of science and useful arts by securing for limited times to authors and inventors the exclusive right to their respective writings and discoveries." This section is based upon two philosophical premises peculiar to Western civilization and which developed in the 17th and 18th centuries, namely, the idea of human progress through science and technology, and natural rights political theories which held that each man (the inventor)had the right to his own property (his invention).

President George Washington signed the first Federal Patent Act in 1790; the responsibility for granting patents was entrusted to a board headed by the Secretary of State, at that time, Thomas Jefferson, himself an inventor. In the first year, only three patents were granted, but by 1802 the number had increased to the point where it was obvious that the Secretary of State could not review all patent applications himself with the aid of a small board, so Congress established a separate Patent Office. By 1838 only a total of 11,098 patents had been granted, and these were given without any examination into the merits or novelty of the invention.

In 1836 the patent system was reformed. Patent examiners were appointed to compare the applications with the prior art to determine novelty and usefulness. Also, patents began to be numbered; No. 1 went to Senator John Ruggles of Maine for a locomotive "designed... to prevent the evil of the sliding of the wheels."

Although there is some question about whether the number of patents bears a direct relationship to the amount of inventive activity and to the number of innovations actually coming into use,[78] it is perhaps significant that the United States Patent Office over the years has issued over 3½ million patents and continues to turn them out at the rate of about 1,250 a week.

A patent can be obtained from the United States Patent Office by filing an application, paying the requisite fees, and then waiting an average of 2½ years before it has gone through the process of examination and approval. Some 1,150 patent examiners review the application, using the criteria of novelty, invention, and utility. The invention must be new and useful, and it must represent something more than a trivial improvement. It is difficult to define "invention" in this context, but it usually means "a contribution over and above the exercise of mechanical skill," and in 1880 the Supreme Court used the expression "a flash of thought" to describe an essential attribute of invention. The search for novelty by the patent examiners is difficult, because a search must be made through all the patents which have ever existed; in order to expedite the search, the patents are filed by subject in some 65,000 subclasses.[79]

It should be recognized that the patent system includes more than the patent itself and the Patent Office. S. C.

Gilfillan defines it as follows: "The patent system includes all the patent laws, customary breaches of them, the folkways, patent lawyers and other personnel, the activity of inventors and executives spent on patents, the litigations and all else that goes with patents as an effective means of acquiring and preserving a private, commercial ownership of the invention (or more accurately, the legal right to exclude others from them by infringement suits), for commercial purposes transferable for some such period as 17 years." [80]

A patent right might be regarded as a negative right, giving the patentee the right to prevent others from practicing or using his invention without his permission. He could achieve the same effect by keeping his invention a secret, but the patent allows the inventor to enforce his rights by law. Although the public sometimes regards the patent as a monopoly which elimates competition, William Callyhan Robinson, in his classic three-volume work, <u>The Law of Invention Patents</u> (1890), emphasizes its contractual nature: The patent is a contract between the inventor and the American people obligating the inventor to communicate to the public the character and uses of his invention by publishing an accurate description of it, while the public is then obliged to protect him for a prescribed period in the exclusive use of his invention.

First it must be noted that not all inventions are patented, nor are all invention patentable. Some manufacturers prefer to rely upon secrecy for protection from competition, the primary example of an unpatented but successful product being Coca-Cola.

Innovations dealing with military weapons are classified; since patenting involves publication, which would destroy the secret nature of these weapons innovations, they are not patented. Inventions dealing with atomic energy, for example, involving government subsidization of the research and development work, are also not protected. These already belong to the people of the United States, who have paid for them through their taxes. Similarly, the publication of research done with federal funds cannot be protected, for it is already, by virtue of its public funding, the possession of the public.

Does the patent system, as briefly described above, serve the purpose of advancing innovation, as the framers of the Constitution hoped it would?

First, there are some difficulties inherent within the system itself. One internal problem is the long period between filing an application and issuance of the patent. If 2½ years is the average time, then many applications must take much longer to process -- and there is one famous case, the Seimer case, where because of interference proceedings, it took some 27 years before issuance of the patent.

As the number of patents increase, the review procedure is made more difficult because there is a growing patent literature which must be checked for novelty and infringement. Furthermore, the increasing sophistication and specialization of knowledge complicates further the problem of deterimimg patentibility as well as determining validity.

Even when a patent is issued, there is no guarantee of its validity. It can be challenged by other claimants, and then the issue must be determined in the courts, usually through long and costly litigation.

Some of the problems affecting the role of patents as a spur to innovation lie outside the patent system and its legal framework. Indeed, two developments in the structure of 20th century innovation present complicating factors. One is the increasing number of inventors who work for industry rather than for themselves, and the other is the growing amount of research financed by the federal government.

Since the inventor working for a private corporation assigns his patent to the corporation, there are questions about the incentives offered him and the rewards which he might obtain. Is he adequately rewarded for his efforts, or does the corporation get the gravy? What system of rewards is the most equitable, and which will most encourage innovation?[81]

Although the legal principle might seem clear enough, government-financed research also presents problems regarding the ownership of patents, an especially acute problem when the work is done not in a government laboratory but by a contractor. Because the work was done through government funding, it can be freely used by anyone, which means that there is no protection for a private contractor who has spent some of his own money in helping develop the innovation. Yet if a contractor is given title to a patent for government-funded work, he will enhance his position in the marketplace, and the patent system will be charged with aiding monopoly. The issue of work done under government contract becomes further complicated by the fact that different federal agencies employ different policies in such matters.

Finally, it should be noted that evaluations of the patent system have varied at times. The Temporary National Economic Committee created by Congress in 1939 "for a thorough study of the concentration of economic power and its injurious effect on the American system of free enterprise" included much discussion of the patent system in the 31 separate volumes and six supplements which it published.[52] Because the hearings of the TNEC occurred during the last years of the Depression, when there was great public concern with the concentration of economic power and the problem of unemployment, its recommendations were for limiting the rights of the patentee in order to ensure more widespread use of the patent by others.

By 1943 economic conditions had changed, and the problem appeared somewhat different. President Roosevelt asked the Attorney General to make an investigation of Government Practices and Policies, which resulted in a report and 19 separate monographs published in three volumes. Under wartime conditions government-financed research began becoming large scale, and the major question considered was the title to patents arising from such research. The thrust of the recommendations was to ensure public use and ownership of innovations derived from government-financed work.

In 1961, the National Academy of Sciences-National

Research Council held a symposium on The Role of Patents in Research.[83] This symposium, reported in two volumes, concluded generally that the patent system provides adequate incentive for attracting funds and stimulating individuals, but some qualifications were expressed.

On the occasion of the 175th anniversary of the United States Patent System in 1965, the Patent Office Society held a major symposium attended by patent attorneys,inventors, research directors, and various other specialists. But there were few suggestions for radical changes in the patent system and no final recommendations or general conclusions were reached.[84]

Nevertheless, some complaints continue to be heard about the patent system. These come largely from impecunious private inventors who cannot afford the expenses of filing and then perhaps defending their applications and patents. A superficial evaluation would say that the laws are based upon the rights of the individual inventor, while the court judgments favor the corporate inventors.

So many different sectors of the economy are concerned with patent matters that any basic overhaul of the system is bound to cause anguished protests to arise from one or another interest group. Hence the major changes in the patent system during the past half dozen years have been to improve its mechanical operation. The Patent Office has moved into a new building with a larger library, and increasingly utilizes computers in classifying information and literature searches. However, there has been no attempt to match the patent system with the changes in the technological, social and economic environment. In brief, it would seem that we are left with a 19th-century patent system to cope with major transformations which have occurred in the socio-economic environment in which innovation actually takes place.

In this connection, it is perhaps worthy of note that the major attempt to "open up" patents for freer use in today's world comes not through patent litigation, but from efforts by the government or by large corporations to force other corporate giants to divulge, or at least share, their patented innovations. Since these corporations protect themselves by an almost impregnable and unassailable fortress of patents around their basic innovation, attempts to break up patent monopolies no longer attack the patents themselves; instead, this is done through vague legislation or lawsuits involving "conspiracy," "restraint of trade," "anti-trust," and the like. Thus, the protection afforded by patents is circumvented. And thus the question of whether or not patents serve to stimulate innovation becomes superseded by other questions involving giant business organizations and their commercial practices -- and few studies of the implications of these latter for innovation have yet appeared.

Indeed, to return to patents themselves, there would seem to be no answer to the question which we posed regarding the patent system as an incentive for innovation. As Fritz Machlup has stated: "None of the empirical evidence at our disposal and none of the theoretical arguments presented either confirms or confutes the believe that the patent system has promoted the progress of the technical arts and the productivity of the system."[85]

Finally, it should be noted that the government can attempt to reward inventors and hence stimulate innovation by means other than patents. On March 16, 1972, President Nixon sent a message to Congress calling for strong efforts to marshall science and technology to strenghten the economy and improve the quality of life. "To foster useful innovation," the President said, "I also plan to establish a new program of research and development prizes. These prizes will be awared by the President for outstanding achievements by individuals and institutions and will be used especially to encourage needed innovation in key areas of public concern. I believe these prizes will be an important symbol of the nation's concern for our scientific and technological challenges."

Some 18 months following the announcement of this step to enhance the climate of innovation, no prizewinners have yet been announced. Indeed, there are still no published set of criteria regarding who would be eligible for such awards nor the measures by which they would be judged. If the awards are ever given out, it is highly likely that they will be presented to those who have already achieved some measure of recognition for their innovations. Nevertheless, to the extent these awards focus attention upon the importance of innovation to American society and thus might have a general stimulative effect, they are harmless enough. At least, they won't stifle innovation -- even if we might not be able to prove their efficacy in stimulating it!

It might also be pointed out that President Nixon's suggestions are by no means new in American history. In 1849, the commissioner of Patents, Thomas Ewbank, proposed that Congress sponsor an "Olympics of Science" to encourage science and invention, with cash awards and medals to scientists and inventors who made outstanding contributions. Commissioner Ewbank even went so far as to list inventions which might meet the pressing needs of his time -- just as President Nixon listed new sources of energy and safe, fast transportation as pressing needs for our own time. Among Ewbank's suggestions were an economical mechanical plow which would work the land without horses (a $10,000 prize); a ship that would cross the Atlantic at an average speed of 20 miles an hour ($20,000 prize); and a grand award of $100,000 to the designer of an electric motor or engine that would operate through atmospheric pressure, or an explosive to drive a cheap, light engine. All of these inventions have come about without the aid of Commissioner Ewbank's prizes.[86] Any objective assessment of this historical evidence would indicate that while public recognition might be very satisfying to innovators, it would have but little effect upon stimulating the innovation process itself.

2. <u>Industrial Research Laboratories for R&D</u>

The limnologists tell us that a "population explosion" in some microscopic fresh-water organisms can change the entire ecology of the pond or lake which they inhabit, and this in turn creates changes within the micro-organisms themselves. This same ecological phenomenon has manifested itself in technology during the 20th century. The almost exponential growth of technology during our times is both cause and resultant of many changes within the innovation process, and these have in turn affected the social environment in which innovation is carried on, the nature and structure of technological work, and the entire field of knowledge comprising the technological endeavor.

For example, the increasingly scientific character of the technological enterprise itself and the growing together of science and technology along a whole spectrum of activities have affected the educational background of the technologist, the information base which he uses, and forced him to collaborate with other individuals possessing specialized knowledge and techniques. The growing specialization and fragmentation of scientific and technical fields makes imperative cooperative enterprise, as does the increasing complexity of technological devices and processes and products. The institutional, or organizational, response to these interrelated phenomena has been the industrial research laboratory, which is responsible for the R&D (research and development) phase of innovation.

Industrial research laboratories originated in the German chemical industry in the late 19th century.[87] The first one in the United States was established by Thomas Edison in Menlo Park, New Jersey, but although this was organized for the purpose of making technological innovations, it was scarcely a team effort. Edison's genius dominated it, and he simply hired specialized assistants to carry his ideas into practice; it was not at all the cooperative venture represented by the modern R&D laboratory. At the beginning of the 20th century, other companies -- General Electric, Eastman Kodak, Bell Telephone, and others -- established industrial research laboratories which have become the prototype for today's.[35]

Although much work of R&D laboratories is in routine testing, we are here primarily concerned with their function as innovators. Teams of specialized scientists and engineers, organized into project groups, bring together the requisite scientific knowledge and technical expertise. Their interaction with one another is not only part of the developmental phase of the innovation process, but also produce creative sparks which help to define problems, generate ideas and practical inventions, and bridge the gap between inventive ideas and models to commercial application and diffusion.

Some solo work in innovation is still done, and from time to time claims are made that the individual inventor still maintains a major role in the innovation process. In the 1960s this view gained support from the study of 50 important inventions of the 20th century by John Jewkes, D. Sawers, and R. Stillerman, which showed that of the inventions selected for analysis over half stemmed from independent inventors or small companies.[89]

From there the argument moved to a quantitative basis with the counting of patents -- or a pseudo-quantitative basis which allowed for differing interpretations.[90] The proponents of the individual inventor argued against those who claimed that group inventive effort produces new innovations by pointing to the fact that the total annual issue of U.S. patents in the 1960s was no greater than 30 or 50 years previously, and that in terms of patents per unit of population, the number was less in 1960 than in 1870, despite the ten to twenty percent annual rise in R&D expenditures in the half century from 1910 to 1960. They also pointed out that the number of patents had not grown in proportion to the increased number of scientists; in other words, the research force was growing far faster than the number of patents produced by that force.

Their opponents -- the believers in the efficacy of R&D laboratories in producing innovation -- first discounted the number of patents as a true index of the nature, amount, and quality of innovative efforts. Furthermore, they claimed, current patents, though fewer in proportion to the population, are individually longer and more technical, and a larger percentage of them were being worked than had formerly been. In addition, an increasing proportion of inventions were being made by government employees or were in the filed of weaponry, and in either case would be unlikely to be patented. They also stated that it was becoming more difficult to make patentable inventions as time goes on, because there was a tendency for the proportion of basic inventions to shrink while that of minor, unpatentable improvements grew.

The high point of the argument in behalf of the independent inventor was reached in the Charpie Report of 1967. The report was the product of a panel of private citizens convened by the Secretary of Commerce; the official title of the report is Technological Innovation: Its Environment and Management,[91] but it is usually referred to by the name of its chairman, Robert A. Charpie, then president of Union Carbide Electronics. The thrust of the report was that the government, primarily through tax concessions, must ease the way for the backyard or garret inventor and for the small company. Its major recommendation -- a White House conference on "Understanding and Improving the Environment for Technological Innovation"--was never held, and few of its other recommendations ever took hold. Perhaps just as well, because the Charpie Report was based upon a static and hence an unhistorical data base.

History is a dynamic process. While individual inventors in 1959 still accounted for 40% of the new mechanical patents, 35% in electricity and electronics, and 30% of new chemical patents, the percentage of patents in those fields to individual inventors was far less than in previous years. The Jewkes, Sawyers, and Stillerman study, constantly referred to by the Charpie panel, dealt with inventions back to the turn of the century, when the structure and nature of technological innovation were far different from what they had become. Furthermore, while the original ideas for many of the inventions cited in that study might have come from individual inventors, their actual development had gone nowhere until they were put into the hands of larger corporations possessing industrial research laboratories which could develop them into commercially feasible and saleable innovations.

By focusing on a limited number of innovations, by looking upon the innovatory environment as static rather than dynamic, and by failing to distinguish among the different process_phases of innovation, the Charpie panel had diagnosed -- and prescribed for -- a situation which was at least a quarter of a century out of date. At the very time the Charpie panel was carrying on its deliberations, the percentage of significant inventions made by independent inventors, even measured by their beloved patent count, was dropping markedly. The lone inventor was giving away to the group worker in the organized research laboratory. The cost of development of a complex innovation were too great to be borne by any individual, and even by some large corporations, who had to turn to the government for support of the most advanced and sophisticated technology.

The fact is that modern"high" technology has become too specialized and complicated for one man to grasp in its entirety. The industrial research laboratory brings together men from a wide area of scientific and engineering disciplines, each contributing his specialized knowledge. Such a laboratory gives an individual access to skills and facilities which greatly increase his capacities. By means of a research team, several lifetimes of skill and knowledge could be brought to bear upon a single problem.

In addition to bringing expertise and skills together in their industrial research laboratories, large corporations can command the funds necessary for carrying on the development of high-priced technology. The Charpie Report correctly noted the advantages which large firms had in the innovative process because of their command of resources; the Charpie Panel's recommendations for tax and other incentives were an attempt to provide the individual inventor and the small firm with the necessary resources for developing his invention, making it easier for him to support the risks involved. That Panel perhaps instinctively realized -- although there is no exposition, either implicit or explicit in its report -- that the major purpose of R&D is to reduce risks in the application of an innovation by reducing uncertainty.

Studies of the decision-making process involved in applying an innovation have been made. Bela Gold points to weaknesses in current models of decision-forming and decision-implementing processes.[92] Gold points out the changing nature of the factors entering into managerial decision-making and stresses the need for new analytical perspectives. More data are necessary on specific case studies involving many different fields, and there must also be studies of laggard companies and industries as well as those which lead in innovation.

If we accept the paramount role of the R&D laboratory in today's innovation process, we still want to know how it ticks, and what can make it tick better in terms of stimulating the flow of innovations. A whole host of questions present themselves: the optimum mix of personnel in R&D laboratories, their organization and structure, the flow of information within the laboratories, the accounting mechanisms employed, and perhaps most important, the measurement of their effectiveness in producing innovations. Studies of R&D laboratories have been carried on by economists, sociologists, and psychologist, each of whom focused on separate elements corresponding to their own professional interests

The economists have sought to measure the effectiveness of R&D by input-output studies, carried on at the microeconomic level of the firm, at the higher industry level, up to the macroeconomics of the national level. They have run into trouble because of the fuzziness of the data base in measuring input and the indeterminate, perhaps non-quantitative, ways for measuring outputs. In the case of inputs, for example, it was difficult to determine just what proportion of a firm's costs attributed to R&D were actually a part of the innovative process; the accounting systems of many firms included routine testing, customer services, pilot plant operation, and test marketing as part of R&D as well as research and development for new innovations.[93] In measuring output, they ran into the ubiquitous problem of patent counting as an effective measurement, and the difficult task of determining the amount of new "information" through citation counts of professional papers as an index of inventions.

The most important and fine-grained analyses of the contributions of R&D to innovation and to economic growth have been those made by Edwin Mansfield of the Wharton School.[94] Mansfield has carefully delineated the fundamental problems involved in measuring the effectiveness of R&D. In evaluating existing productivity studies on R&D, Mansfield has pointed out that too little is known about the activities which firms comprehend under the term "research and development," and that even if the figures were exact, there might be spurious correlations, because the productivity increase attributed to R&D might rather be owing to forward-looking management. In other words, both R&D and the increased productivity might be due to the quality of management in all segments of the firm. In addition, productivity studies run into difficulty, according to Mansfield, because a large percentage of R&D carried out by some industries is directed at productivity increase in other industries; we have too little information on inter-industry or inter-firm flows of technology which underlie these relationships. And if R&D is treated as investment in new knowledge, how can one use standard depreciation rates in order to get at its cost?

Other difficulties arise which might skew economic analysis of the effectiveness of R&D in innovation. In the United States the vast bulk of the money spent for R&D has come from the federal government, and slightly over three-quarters of those funds have gone for aircraft and missiles and electronics and communications. Can general measures of the effectiveness of R&D be obtained by examples drawn from such few fields and directed toward such specialized -- one might almost say, non-economic -- activities as military and space programs?

Despite many reservations and qualifications, some of the economic studies of R&D have been able to provide convincing quantitative arguments of its effectiveness in specific fields. For example, Zvi Griliches studied argicultural R&D and found that between 1937 and 1951 the investment in agricultural research in the United States returned between 35 and 170 percent.[95] Mansfield himself has estimated returns from R&D in the chemical and petroleum industries and has found them to be about 40 percent or more in the latter, and about 30 percent in the former.[96]

From what has been stated, it is evident that the links between R&D and innovative effectiveness are far from clear. Mansfield has done much to focus the issues, to pinpoint the basic methodological problems, to evaluate the existing literature, and to indicate the directions which future research might take. In the process, he has destroyed some common myths which have already appeared regarding R&D. For example, he has demonstrated that bigness seems to have little effect upon R&D productivity; large firms do not have proportionately more R&D than do middle-sized firms, and they might actually produce fewer important innovations. In other words increases in scale do not seem to bring proportionate increases in innovative productivity. Mansfield has also shown, however, that these matters can vary from industry to industry, and it is important that we recognize these variations among industries lest we generalize on the basis of too narrow a sample.

Earlier,Simon Kuznets had pointed out that elements of creativity and imagination and similar qualities difficult to measure also enter into the R&D productivity.[97] Another major economist, Fritz Machlup, had recognized that the critical input in R&D laboratories was the utilization of scientists and engineers, but that the mere numbers employed might not be an indication of the quality of the output.[98] This emphasis on non-economic elements helps explain the interest of sociologists, management experts, and industrial psychologists in personnel, organizational, informational and managerial problems within R&D laboratories.

In a major study entitled The Management of Innovation,[99] T. Burns and G. M. Stalker presented detailed evidence of the way in which political and status considerations affect the working organization of industrial laboratories. In other words, the social system of the organization itself affects its technical performance. Burns and Stalker found that a "mechanistic" management system with clearly defined roles and an emphasis on hierarchical authority performs effectively in relatively stable technical and market conditions, but that a less formal structure might be more successful in coping with changing conditions.

In the study of the innovation environment conducted by Arthur D. Little Company for the Department of Defense (1965), it was also found that those objective characteristics of a productive R&D laboratory which could be quantified appeared to be of less importance than such non-quantifiable factors as attitudes, motivation, interpersonal relations, and the manner in which the laboratory was managed. Much the same conclusion was reached by a study conducted by the University of Michigan Institute of Social Research under Donald C. Pelz.[100] The purpose of the study was to find ways in which organizations or working conditions in the innovative environment could be created or tansformed to evoke creative performance from talented scientists and engineers. The research recognized that the creative situation in an industrial laboratory was dependent upon a number of heterogenous factors, including the professional motivations or orientations of the scientists, their communications with their colleagues and the research manager, length of time with the group, and other elements. In what might be considered an extension of Burns and Stalker , Pelz found that creativity was increased when there were elements of both uncertainty and security in the research organization: enough uncertainty to be sufficiently creative to risk the unknown, but enough security to offset the anxiety aroused by the uncertainty. Maximum creativity would thus be found in situations containing both a high amount of dither and high security.

Simon Marcson has also probed the role and effectiveness of scientists in industrial research organizations.[101] He has emphasized the coalescence of differing orientations upon the performance of the task.

It should be obvious by now that we certainly do not know the "answers" to many of the problems raised by the new social environment for technological innovation: the R&D laboratory. Yet the trends in the literature indicate that we are approaching a much higher level of sophistication in studying these matters. The historical evidence would indicate that we can never arrive at definitive models or explanations; the ecological nature of the innovative process dictates that this be a dynamic and changing process, and hence the optimum institutional forms in one industry, firm, or even on a nation-wide basis, would be changing in response to other changes in the ecology -- and all of these variables would be changing over time. Perhaps the best that can be done is to identify those factors which bear watching and to specify those elements in certain fields of technology and certain sized firms which seem to be of most significance in affecting the innovation process.

E. Diffusion Mechanisms

Any consideration of the social ecology of innovation demands that attention be paid to diffusion mechanisms. If technology itself is regarded as knowledge, innovation must be viewed as new knowledge. The application and diffusion of this new knowledge involves all the social, economic, and political factors we have been considering. For example, the same economic factors entering into the rate and nature of inventive activity help determine the rate and nature of the application and diffusion of the innovation. The demand for profit and factor-savings, the competitive situation, and other economic parameters enter into the decision to put the invention to the test of the marketplace and its acceptance for diffusion.

Nevertheless, certain problems in the diffusion mechanism are peculiar to its social environment and require further study. Since diffusion involves the transfer of the innovation into another context from that in which it originally arose -- a different firm, a different industry, a different nation, a different culture, a different use -- it partakes of many of the elements of technology transfer. If by the transfer of technology we mean the acquisition, development and utilization of technological products, processes, and knowledge in contexts different from those in which they first developed, we must perforce recognize that such transfer requires innovation within the new context. But we know from the research of psychologists and cultural anthropologists that every innovation has broad intercultural foundations and implications; hence the process of technological diffusion is a cultural, social, psychological, and political matter, and not just the imitation of artifacts and processes.[102]

The diffusion of technology can be viewed from the perspective of many different disciplines, with each field selecting those aspects of the diffusion process corresponding most closely to its main interests. Anthropologists and sociologists, for example, focus on elements of sociocultural resistance to change and the interactions of different cultures with one another, while geographers concentrate upon spatial patterns of diffusion.[103] Economists , of course, focus on the economic variables in the transfer and diffusion of technology. They talk in terms of returns on investment, labor or capital intensiveness, resource endowments, and the like.[104]

Historians of technology are tempted to analyze the diffusion process solely in terms of the technical elements. Using such an approach, we might distinguish three phases of technology transfer: (1) material transfer, which is the simple diffusion of tools, machines and technique; (2) design transfer, wherein blueprints, formulas, books, and the like are the major medium for the diffusion of the technology; and (3) capacity transfer, where technical know-how and capabilities are acquired.[105] Such a nice schema, no matter how logical it might seem, would be misleading because developments have not always occurred in these three sequential steps. Nevertheless, this approach is useful because it calls attention to the different kinds of technical knowledge which can be diffused, and it implies that there should be a match between the type of information diffused and the needs and capabilities of the receptor.

Another way of analyzing diffusion would be to trace it, say, from military to civilian use, or among firms within one industry to another, or one country to another. Edwin Mansfield has studied the diffusion of numerical control in the tool-and-die industry, concentrating on the rate at which the percentage of users of an innovation grows, the characteristics of the early and later users, and the diffusion of innovation within particular firms. Numerical control (a method for operating machine tools by means of taped numerical instructions) first came into commercial use in 1955. Certain factor savings -- reductions in scrap costs, capital and labor costs (the skill is built into the machine), and inventory costs, as well as easier scheduling -- gave this innovation an advantage over former methods of milling. Mansfield discovered that the larger machine shops tended to adopt numerical controls earlier than the smaller ones, that the decision-makers in those firms which adopted it tended to be younger than those which did not adopt it, and, comparing the tool and die industry with others, that this innovation was assimilated more rapidly than it would have been in railroads, steel, or coal -- which seemed to support the hypothesis that innovations spread more rapidly in less concentrated industries. In other words, the size of the firm, the nature of the industry, and then a non-structural factor -- the age and college training of the managerial group -- affected the diffusion.

Still another way of treating the diffusion problem would be to look at the mechanisms involved in the process. Viewed in this functional manner, the problem becomes one of transferring information from the innovator (the information generator) to a new user. There are three major ways in which this can be done, none of them mutually exclusive: (1) artifact-to-person coupling; (2) person-to-person contact; and (3) person-to-literature linkage.

Because the foremost writers on diffusion are academicians, they are perforce immersed in literature. Hence they sometimes tend to overlook the importance of the actual demonstration of the innovation to the potential user. Thus the study of artifact-to-person diffusion has been largely neglected, yet it is perhaps the most powerful of diffusion mechanisms. Indeed, the efficacy of this technique is known to every primary-school pupil under the descriptive title of "Show and Tell." Every technical salesman, wherever practicable, carries samples or demonstrator models with him for the "show-and-tell" effect. Trade shows and exhibitions form part of every trade organization and professional society meeting. This means that there might be some quantitative data available -- attendance figures, or, buried within corporation ledgers, some analysis of sales derived from showing innovations to potential users -- which might be helpful in arriving at generalizations regarding the efficacy of this diffusion method. However, no one has thought to incorporate such materials into a general model of innovation diffusion.

There is, however, some historical information on the impact of world's fairs and international exhibitions on technological progress and diffusion.[106] Much of this literature centers on the role of such exhibitions in creating a sociocultural climate of acceptance of technological growth. Little attention has been given to the way in which exhibitions fostered the diffusion of specific innovations, although there is the well-known case of Britain's introduction to the American technique of interchangeable parts at the Great Crystal Palace Exhibition of 1851 in London. This led to the dispatch of a commission under the great machine-tool designer, Joseph Whitworth, to see the "American system of manufactures" in operation; on the basis of the Whitworth commission's report, the British equipped the Enfield Armory with American machine tools, and hence the diffusion of innovative devices and processes.[107]

There have been many studies of the diffusion of information through the medium of scientific and technical literature.[108] For the most part, such studies employ citation counts to measure the diffusion of ideas, and it is not surprising that such diffusion has been studied by epidemiologists who liken the transmission of ideas to the spread of infectious diseases.[109]

People-to-people transfer of technical information is being increasingly studied. Thomas Allen's concept of the "technological gatekeeper,"[110] the individual within the laboratory who informally links his colleagues with outside sources of information, is being extended by work in progress on the "informational entrepreneur" carried on at the Georgia Institute of Technology. The Georgia Tech study stresses the passivity of most current information systems (even highly computerized information storage-and-retrieval systems represent nothing more than a fast librarian), and the need for a more active and flexible scheme of information diffusion which would allow for feedback from the user to the generator of information, as in the highly successful Agricultural Extension Service.

The mobility of professional engineers can be an important factor in diffusing technology. Gilfillan claims that the mobility of labor, industrial spying, disclosure in patents, and the inability to seal off the research and productive processes tend to erode the differential level of knowledge among firms.[111]

When an engineer moves from one firm to another, or when a scientist or technologist moves from a government laboratory into private industry, or vice versa, he carries with him knowledge and information, some of which may legally be transferred to his new employer. Many unpatented processes and techniques can be diffused by the movement of people from firm to firm, from industry to industry, or from country to country. Indeed, in the mid-1960s, the "brain drain" from other nations to the United States was held responsible for the "technological gap" between the United States and other nations. The "brain drain," while exaggerated in its impact, represented not so much the transfer of actual devices and designs as it did the transfer of technological capabilities.

None of our three mechanisms for diffusion of innovation exclude one another. A person sees an innovation at a trade show (artifact-to-person) and tells someone else about it (person-to-person), or he reads about it (person-to-literature) and refers someone else (person-to-person) to that same item (person-to-literature). Whatever be the mechanism for the diffusion of innovation -- artifact-to-person, person-to-person, person-to-literature -- it has been institutionalized to some extent. Trade organizations and professional societies, for example, provide an organizational means for the operation of the mechanisms. The annual meetings of such organizations usually have exhibition space where the "show-and-tell" element of technological diffusion is emphasized. At the same time, personal contacts in the bars and corridors as well as listening to papers presented at the sessions provide means for transferring information. The socialization function of such meetings might be much more significant than their diffusion function; but diffusion is itself a socialization process.

Trade organizations have publications, but the role of trade publications in diffusing innovation has been neglected. Again, some forms of data are available. Controlled-circulation trade publications are constantly checking, by means of questionnaires, on the roles and positions of their readers, their interests, and the like. Other publishers, such as McGraw-Hill, with open-circulation specialized trade publications constantly survey their readers to determine what articles, advertisements, and new products attracted their attention. In addition, many publications include return postcards for asking for further information from advertisers. All these sources would provide data for investigating the role of such publications in diffusing information regarding innovations; at present, their chief use is by the advertising and promotion departments of the trade journals.

The federal government has played a major role in diffusing information about scientific discoveries and technical innovations. The most successful of its efforts has been the agricultural extension service, and a good deal of the success of American agriculture during the 20th century is testimony to the effectiveness of this device.

The agricultural program, fostered by the federal government beginning with the Morrill Land Grant Act of 1862 which founded the land-grant colleges for promoting the agricultural and mechanical arts, combines all elements of the innovation process within it. Agricultural research stations, both state and federal, usually connected with the land-grant agricultural college, generate the discovery or invention; and the extension service, through county agricultural agents, makes certain that the innovations thus produced are applied and diffused.

The county agricultural agent not only transmits scientific, technical, and economic information (in a form and manner aimed directly at the farmer-user), but he also serves as a means for feedback from the farmer-user to the information-generating source. For example, when the individual farmer is faced with a new problem, the county agricultural agent transmits this user need to the information-producing source, which then seeks out or adapts old information or creates new information to provide the answer. In other words, direct feedback from the user helps in the problem-definition phase of the innovation process. The agricultural research station, faced with the problem, has access to past literature and to laboratories for carrying on experimentation. It can thus perform all the innovation stages from idea generation and invention through the research and development phase. Then the county agricultural agent carries the information to the farmer for application and diffuses it among other potential users. The county agricultural agent thus serves as an important feedback link in the ecology of the innovation process.

When J. Herbert Hollomon was Assistant Secretary of Commerce for Science and Technology, he sought to develop an industrial analog of the agricultural extension service. His efforts proved unavailing, because the Congress never approved of the program as a whole nor provided sufficient funding for those parts of which it approved.

Nevertheless, there are non-agricultural and non-governmental modes of innovation diffusion which resemble somewhat the agricultural extension service and which have exhibited some success in the marketplace. One example would be the "detail men" in the pharmaceutical industry, who go directly to the users (doctors) to present them with the literature and samples of new drugs. At the same time, they feedback to their employers the informational needs of the doctors; although not medical men themselves, they provide the crucial coupling of information between generator and user. Interestingly enough, there seems to be no serious study in the information sciences literature regarding the role of the drug detail men in diffusing knowledge.

The "sales engineer," the salesmen of technical products, is a similar diffusion device. He brings information of new technical products and processes directly to the customers, and at the same time serves as a conduit whereby the technical needs of his customers are transmitted to the manufacturer-innovator whom he represents. To the best of our knowledge, there have been no scholarly studies of the role of technical salesmen as diffusers of information nor as articulators of consumer needs and hence as problem definers for innovators.

One widely heralded government attempt to diffuse information regarding innovation is the NASA Technology Utilization Program.[112]

Through contracts with Midwest Research Institute, Stanford Research Institute, Denver Research Institute, and others, NASA has actively sought, in accordance with its mandate, to transfer the scientific and technological "spinoff" of the space effort to the civilian economy. Despite good intentions and the expenditure of considerable money and effort, this attempt to diffuse innovation has had only mixed results. In an unpublished research study prepared for the NASA Office of Scientific and Technical Information, Kranzberg and Rossini have indicated flaws in NASA's Technology Utilization Program: it does not allow for feedback, and when user needs cannot affect the information generation source, the information-coupling mechanism is weak. On the other hand, in that part of its mission relating directly to civilian aeronautics, NASA has an excellent record -- reaching back to its old days as the National Advisory Committee on Aeronautics, when its innovations were directly in line with user needs.

The defects in the NASA Technology Utilization Program are further proof of the ecological nature of the innovation process. If there is no feedback, no interplay, among the different phases, the innovation process becomes flawed and imperfect. In the case of NASA, civilian user needs could not directly influence the problem-definition and idea-generation phase of the innovation process. Instead, NASA's Technology Utilization Program places undue emphasis upon serendipity; the innovations arising from the space program might happen to be useful for civilian industry, but it is not the task of NASA to make them so nor to embark upon innovations with that end in mind. In order to apply NASA's technological innovations, the civilian user must frequently re-define his own problems, rather than having the innovation produced in answer to his needs from the very beginning.

An important new mechanism for technological diffusion has recently come to public attention, although it has been around for some time: the multinational corporation. Unlike international aid and development programs sponsored by governments, which have served as devices for the transfer of technology to the less-developed nations, especially in the period since World War II, the multinational corporation diffuses technology to already-industrialized nations. Multinational corporations take on many different organizational forms, with some allowing a high degree of autonomy to their international parts while others maintain a high degree of centralized control; they differ also in diffusing innovations, some representing only material transfer, others diffusing design transfer, while still others developing capacity diffusion. Although there is a vast literature on the diffusion of technology to less-developed countries through government and international aid and financial programs (e.g., the World Bank), there are only beginning to be serious studies of multinational corporations. Most of these, however, deal with political, financial, and international monetary problems arising from the operations of multionals, and little attention has yet been paid to their role in the diffusion of innovations.[113]

There remains one major channel for the diffusion of innovation in institutionalized form which we have not yet treated: the educational system. After all, formal schooling is the prime institutional device whereby society attempts to transmit information from one generation to the next. What role does it play -- or should it play -- in the diffusion process?

Except at the upper levels of graduate education, where aspiring doctoral candidates are actively engaged at the research front, the educational system cannot provide a very powerful or speedy mechanism for the diffusion of innovation. Indeed, unless his professor is himself at the cutting edge of knowledge, the engineering student makes do with instruments and devices which can adequately acquaint him with the principles and techniques but do not represent the latest innovations. His scientific information is also apt to be somewhat dated; Derek Price has pointed out that engineering textbooks are usually filled up with the packed-down scientific information which is some one to two decades old, and Edwin Layton has suggested the need for "engineering scientists" as intermediaries to "translate" scientific information into terms which are meaningful to engineers in their special work.[114]

In engineering education, increasing emphasis is being placed upon "design" or "creative engineering," as some engineering educators call it. This trend became apparent at a 1965 conference based upon the assumption that inventors and innovators are the moving element in technological change, which is the province of "creative engineering," and that the creative requisites of innovation can be encouraged and taught. The conference advocated research on the processes of technological change, to be undertaken on a multi-disciplinary basis, inducements to faculty and students to participate in courses involving innovation, and cooperation among universities, industry, foundations, professional groups, and government to develop and support education for innovation. The conference did not call for scrapping of present engineering education; indeed, one of the panels of the conference pointed out that some of the requisites for creative engineering already existed in present engineering education.[115]

Engineering education has transformed itself in the period since World War II. Instead of teaching existing techniques and tools which quickly become obsolete, the emphasis has been on acquainting the students with basic scientific and technological principles which could be applied to a variety of circumstances. This shift has done much to retard the obsolescence rate of engineers. Nevertheless, the proliferation of short courses and continuing education programs at engineering institutions indicates that advances in technology rapidly make practicing engineers obsolete; they must go back to school to learn what is newest and latest in their field. Hence continuing education programs can provide a major means for disseminating information about innovations, but there are no scholarly studies indicating their contributions in this respect.

Even if our formal system of engineering education does not provide an adequate mechanism for the diffusion of innovation -- that is not its primary task -- it still remains essential for other parts of the innovation process. Education provides the means for instilling the capacity to innovate. It diffuses the ability to utilize innovations and to produce further innovations.

Education helps embryonic innovators master the essential underlying knowledge, acquaints them with the types of approaches which can be employed, and inculcates the problem-solving techniques which become essential for innovation.

In contrast to the assumption that the capacity for innovation can be taught is the conviction among many that innovation is such a highly individualistic matter that it is not properly the subject of formal education. Indeed, there is some feeling among the general public that innovators are likely to be "off-beat" or eccentric individuals, and that formal education tends to stifle innovative propensities. Charles Kettering expressed this point of view when he exclaimed, "An inventor is an engineer who doesn't take his education too seriously." The idea behind such statements is that education produces highly disciplined scholars, and such discipline hampers the free rein of imagination which is necessary for creative work.[116]

Finally, it should be noted that most of our large engineering schools -- Purdue, MIT, Carnegie Tech, Georgia Tech, Stanford -- have associated with them management schools, business schools, or industrial engineering schools and departments which emphasize the managerial and economic elements of the innovation process. The contribution of engineering education to the innovation process is thus not confined solely to technical elements but encompasses many of the other ecological factors in the innovation process. Particularly notable is the fact that some of the engineering schools have pioneered in science policy studies. Hence they are in a position to contribute studies of the role of the government in decision-making regarding technological innovations, studies in the process of technology transfer, especially among nations, the diffusion of innovations among various levels of government, and the like. Although still relatively new, the field of science policy studies is flourishing, and it promises to add much to our knowledge of the role of the government, both normative and prescriptive, in the innovation process.

Alfred North Whitehead, one of the major philosophers of our times, stated that one of the principal achievements of the last century was "the invention of the method of invention." He meant several things by this: one was the purposive nature of the inventive act; men actively sought to make inventions -- whether out of private greed or concern for the common weal -- in a sociocultural climate which was far different from that which dampened innovative efforts throughout the centuries. Second was the development of a rational technique for the innovative process; mathematics, logic, and experiments were applied in a synergistic merging of science and technology leading to a multiplication of inventions. To the extent that our educational process focuses upon these elements of education of scientists and engineers, it remains a major force in the innovative process. Heretofore, we have concerned ourselves with the first element in Whitehead's analysis, the sociocultural climate, the economic motivations, the political factors, and the like. Now we turn to the technical background whence innovations emerge and the role of that technical environment in helping determine the nature and course of innovative activity.

IV. The Technological Background

The innovation process, as we have seen, is an ecological one, involving the complicated interplay of many factors. Heretofore we have concentraded our attention on the "external" elements -- social, cultural, political, and economic factors -- determining the course and nature of innovation. But technological innovation is inherently a technical act. Previous technical innovations help to set the problems posed for future innovations; the existing level of technology provides pathways for future development, while at the same time providing constraints upon innovation. Just as the "external" factors influence the nature, direction, and rate of innovation in technology, the "internalities" of technology provide both opportunities and limits to innovations in specific fields at given times.

Proof of the importance of the technological factor is shown in the fact that very few innovations constitute "giant leaps" in technology. Technology is cumulative in nature, as are most fields of knowledge, and most advances have come through small incremental changes rather than through quantum jumps.

New inventions are based upon previous inventions, and they evoke still newer inventions. In some cases, innovations derive from putting together elements which were already in existence. For example, the great invention of printing, which was to have such an impact upon Western man,[117] derived from bringing together several different elements, some of which had been known for a long time, to create something new. The press as such had been used for centuries in many different processes -- in wine-making, in olive oil, and the like. But printing also involves paper, ink, and movable type. The Chinese had invented paper some 1300 years before Gutenberg, and by the middle of the 12th century, paper had superseded parchment and vellum for writing purposes in Europe. In addition, the Chinese had previously used wood blocks, and these had come into use in Western Europe for printing playing cards. But ink presented a problem: the ink used for printing on wood block was made with a water-soluble base which ran when used with metal type. However, Renaissance painters were experimenting with pigments mixed with linseed oil, and Guttenberg adapted this medium to provide a suitable ink. Finally, separate type was already known to bookbinders, who used it to stamp titles on bindings of books; what Gutenberg added was to cast this moveable type in an adjustable mold. The invention of printing, therefore, like that of many other devices, rested upon many prior inventions. Gutenberg's contribution was a creative synthesis -- sorting and selecting the essential elements from devices, techniques, and products already known and utilized in different fields, and combining them in a new form for a special purpose.

Similarly, television was basically a putting together of two other inventions, the radio and cathode ray tube, and each of these were the products of still earlier inventions. To take a different example, Henry Ford did not invent the assembly line for mass production, as many people have thought -- and Ford himself claimed. The separate features of the assembly-line process -- the conveyor, the specialization of labor, the interchangeability of parts -- were already well established in American industrial practice by the time Ford came along. What he did was to combine all these features into a closely co-ordinated system with amazing increases in efficiency and productivity.

Another type of innovation is to extend devices or techniques to new and slightly different uses. Given the phonograph, for example, an office dictating machine represents only a simple extension of the same basic principles and mechanisms -- and it is not surprising that one man, Thomas Edison, could invent both. Or, once magnetic tape was available, it could be applied to a number of purposes -- videotaped television programs, computer records for banks and stores, tape recorders, and a whole host of other uses -- all of them requiring new innovations or adaptations of older inventions.

The same holds true for processes and techniques. In his study of DuPont rayon plants, Samuel Hollander sought for the sources of increased efficiency in modern industrial plants.[118] Defining technological change as "changes in the technique of production of given comodities by specific plants, designed to reduce unit production costs," and including managerial and organizational changes in his definition, Hollander found that technical change accounted for some 35 to 100% of the total net reduction in unit factory costs at the five factories studied. He then found that "minor" technical changes accounted for over two-thirds of the unit-cost reductions attributible to technical changes. These were not the "major" changes dependent upon formal research and development mechanisms; instead, they were done by plant personnel intimately linked with operations. These minor technical changes added up to fairly large-scale technical advances which produced large economic savings. Thus Hollander supports the generalizations made by his mentor Fritz Machlup on the economic importance of "minor improvements" in technology.

We tend to overlook these minor changes which together add up to major innovations. So bemused and enchanted are we by such breakthroughs as the transistor, nylon, atomic energy, and the like, that we focus our attention almost exclusively upon such major innovations. But these might be atypical. In our quest for knowledge of the process of innovation we might be misled by concentrating on these major jumps and neglecting the more usual causes of innovation, namely, the small incremental changes.

Whether major or minor, giant leaps or baby steps, innovations are dependent upon the technological level of the times.

A. Technological "Readiness"

Even when there is undoubted demand for an invention and when all the sociocultural factors are favorable, the innovation cannot take place unless and until men develop the technical capacity to produce it. For example, many people had ideas for flying machines in the centuries before the 20th. During the 19th century a considerable body of aerodynamic theory was built up by scientists, engineers, and glider pioneers. But one thing was lacking -- and that was a compact engine which would provide sufficient power in relation to its weight; not until the latter part of the 19th century was the internal-combustion engine developed, so that the airplane as we know it could be invented. In 1628 the Italian Giovani Branca designed a turbine engine, but the mechanical means to shape metals sufficiently well to make a turbine workable were not developed until the 19th century. In 1766 the Russian Polzunov invented a steam engine very similar to that of James Watt -- but the level of technological knowledge in mid-18th century Russia was apparently so low that no one was able to repair Polzunov's boiler when it sprang a leak in November 1766, after being in operation only some four months. Hence Polzunov's "invention" never became an innovation.

The development and application, if not the invention, of Watt's steam engine is an example of the importance of technological "readiness." A series of prior and contemporaneous developments set the technological stage for Watt's steam engine: the shortage of charcoal for smelting, necessitating the substitution of coke, and hence the need to pump our coal mines; the invention of puddling by Cort, which simplified and cheapened the production of wrought iron, thereby increasing the demand for smelting fuel; and John Wilkinson's improvement of the boring mill (1775) which made it possible to bore cylinders to the fine limits of accuracy required by Watt's engine. Wilkinson had devised his machine in order to bore cannon, but Watt saw that this technique could also be used to produce cylinders for his engine, and the efficiency produced by having a sufficiently close fit between piston and cylinder helped make Watt's invention a success. It is interesting to note that the full utilization of Watt's steam engine was hampered at first by a shortage of skilled labor; nevertheless, the level of technical expertise in Britain was such that his invention could be immediately applied, even though its diffusion lagged until mechanics could acquire sufficient experience to operate and maintain the engines.

The importance of technological "readiness" is reinforced by the concept of "technological convergence." Nathan Rosenberg has pointed out how technological developments in one field make possible developments in others.[119] An example of this would be the large number of horseshoe patents during the 19th century. Schmookler had explained this in terms of demand -- but that explanation raises more questions. After all, most of the problems involved in the shoeing of horses were of long standing, and the real questions are why the demand was satisfied in the particular manner in which it was, and why it was satisfied at that particular time in history. The answer appears to lie in the convergence of metallurgical and machine-tool developments during the 19th century. Functionally defined, the horseshoe industry is a metal-using industry, and all but a few of the many new horseshoe patents issued in the 19th century were based upon better ways of casting, rolling, bending, and machining of metals. The machines for forging, rolling, cutting, and bending the shoes were themselves complex machine tools, and the products of an advancing machine tool industry; horseshoe patents also converged, then, with the standardization and interchangeability of parts.[120]

Considerations of technological "readiness" allow us to extend Schmookler's analysis based upon demand. Demand has a cultural history, and it cannot be separated from the cultural context in which it occurs. But -- and this is extremely important -- demand also has a technological component; it cannot be divorced from the technical ability to satisfy the demand.

Patent activity in horseshoes ultimately waned, caused by a decline in demand occasioned by the introduction of the automobile. This shift too had technological dimensions. People had always wanted an efficient means of individualized transportation; with the advent of the automobile, this demand took the form of the automobile. In other words, demand operates within technological parameters. People may wish for something or dream about something, but they can present no specific demand for it until technology makes it available.

The concept of technological readiness would include the need to develop ancillary technologies before some inventions can become innovations. Additional technology frequently has to be developed -- on order, as it were -- to transform an idea or a device into a marketable commodity. For example, the transistor was patented in 1948, but the original point-junction transistor was not completely marketable or dependable. Ancillary technologies, such as those dealing with very pure materials and single crystals free of imperfections, were necessary before the transistor could become commercially feasible. These technologies in turn made possible many advances in the fundamental understanding of the properties of solids, and these advances in solid-state understanding led in turn to other devices ranging from integrated circuits to magnetic memory cores and thin films and, eventually, things such as masers and lasers. It can be seen that a single innovation can evoke a whole chain of causation in both science and technology.

B. Technological Imbalances

While technological innovations can create opportunities, they also create pressures which influence the inventive process. Because technologies are so interdependent, changes in one element can create "imbalances" which necessitate changes in other technologies. For example, more powerful automobile engines require more powerful braking systems. Improvements in the cutting tools in lathes, with the use of high-speed alloy steels, required accommodations in other parts of the machine in terms of control,lubrication, disposal of waste material, and the like. Sometimes auxiliary equipment is necessary in order to make certain innovations work better; for example, the early Bessemer converters had disadvantages until Alexander Holley developed a removeable bottom shell to reduce the down-time of the converter.

Sometimes the attempt to meet a technical imbalance can bring about a major change in the original innovation so as to create a new one. Thus a series of technical imbalances in grinding machines started with the introduction of artificial abrasives and led eventually to grinding machines which performed precision machining of light alloy steels instead of merely using grinding for finishing pieces formed on lathes.

The concept of imbalances is not confined to changes induced in a single kind of tool or device. There can also be imbalances between different elements of related technologies.

Thomas P. Hughes refers to this kind of imbalance as a "reverse salient," and he likens it to a bottleneck in a system of production.[121]

A sequence of imbalances of this kind is to be found in the textile industry in the 18th century. Richard Kay's invention of the "flying shuttle" speeded up the weaving process, upsetting the usual ratio of four spinners to one weaver; either there had to be many more spinners to supply a weaver with sufficient thread or yarn, or else spinning had to be similarly quickened by innovations in that field. A series of inventions by James Hargreaves, Richard Cartwright, and Samuel Crompton speeded up the spinning process; then Cartwright set about mechanising the weaving operation in order to take full advantage of the now-abundant yarn produced by the new machines. The result was the power loom. These machines lowered the price and hence created a large new market for cotton textiles, but another bottleneck developed in the supply of raw cotton, where the chief difficulty lay in the amount of labor involved in picking the seeds from the bolls. This problem was solved by Eli Whitney's invention of the cotton gin, which more than trebled the amount of cotton a man could pick free of seeds per day and provided sufficient raw materials for the busy spinners and weavers. Thus innovations in one field produced a need for inventions in other related fields. Here is a case where invention was the mother of necessity, rather than the other way round.

C. Problem-Solving

An important element of the technological level is the skill and knowledge of the inventor and the propensity of the engineer to "improve" processes, products, or devices. Engineers like to think of themselves as "problem-solvers." Of course, all disciplines are problem-solving; engineers address themselves to the solution of technical problems. The major thrust of their efforts along these lines is to increase efficiency. Indeed, if there is anything which might be called the "technological imperative," it is the demand for efficiency.

Unfortunately, "efficiency" can mean many different things. Throughout most of our industrial history, technological efficiency has been measured in terms of input-output, and that has usually been construed along narrow economic lines. Only recently has there been pressure to view input and output in non-economic terms, although many engineers feel uncomfortable in dealing with non-economic factors because some of them (e.g., environmental quality) are difficult to quantify. Nevertheless, the engineering profession remains committed to the quest for efficiency.

This pursuit of efficiency means that a number of well-trained minds are constantly striving to "make things better." When some social critics raise the embarrassing question of what is meant by "better" (for instance, better than what?), the engineer admits of no such doubts. To him, "better" means something which functions more "efficiently" than its predecessors:

a machine which will produce things faster or more cheaply, a product which will be more economical or durable, and the like. Any product, device, tool, or technique presents the engineer with a problem-solving challenge to make it "better."

Machlup, referring specifically to technical problems in the production process, claims that much invention takes place because there are problems to be solved. In attacking these problems, engineers invent things which might become innovations. Nathan Rosenberg has also stressed this technical aspect of innovation. To make new products and processes practicable, Rosenberg points out, "There is a long adjustment process during which the invention is improved, bugs ironed out, the technique modified to suit the specific needs of users, and 'tooling up' and numerous adaptations made so that the new product (process) cannot only be produced but can be produced at low cost."[122] The desire of the inventor to improve things, to make them more "efficient," thus continues throughout the phases of the innovative process and is not confined solely to the inventive stage.

Closely related to this desire of engineers to improve products is the concept of "technological momentum" advanced by Thomas P. Hughes. In a case study, Hughes showed how engineers working for I. G. Farben, the giant German chemical firm, became so immersed in the technical problems involved in making synthetic gasoline, that they, and the giant corporation of which they were a part, were willing to submerge their political consciences and support the Nazi regime because it would allow them -- indeed, encourage them -- to continue their work. "The commitment of engineers, chemists, and managers experienced in the process, and of the corporation heavily invested in it, contributed to this momentum," states Hughes, and thereby led the corporation and its engineers to cooperate with an extremist political party.[123] To put it in other terms, the momentum of their technical interests led these engineers to become "technocrats," wholly absorbed in the technical problem of how to accomplish certain ends without any questioning of the ends themselves.

This type of concentration upon the technical problems themselves without reference to their possible human and social implications has led many social critics to question the value system underlying our advancing technology, and has even led some individuals, such as W. H. Ferry, to suggest a moratorium on technological innovation. The striving for technological "perfection," embodied in the imperative of efficiency and the concept of technological momentum, is a prime element in many innovations. It indicates that there is a pursuit of technological improvement for technology's sake, not just for economic gain.

Other non-economic factors also enter into the problem-solving propensities of engineers. For example, much invention occurs in response to the play element in human nature. The most serious study of the play factor in human activities is Johan Huizinga's study on Homo Ludens, Man as Player.[124] Huizinga believes play to be intrinsic to many different domains of human activity, for example, religion, law, war, art, and literature. To Huizinga, play is a "free" activity, with nothing forced or compelled about it, outside of real life, and not leading toward material gain or profit, while at the same time "absorbing the player intensely and utterly." Huizinga's emphasis on play might be applicable to much inventive activity, if not to the entire innovative process. Indeed, its importance in invention can be seen in the following phrases frequently heard among engineers: "I was fooling around in the laboratory one day..." or "I was playing around with this motor when I happened to think that we could improve it if we...."

The fact is that inventors are fascinated by technical devices and processes. They like to "play around" with them, seeing their potentialities and limitations. Linking Huizinga's concept with Schumpeter's thesis, we might also find the entrepreneur looking upon innovation as a game from which he derives pleasure, perhaps just as the gambler enjoys taking risks. Whatever be the wellsprings of human psychology to which the "play" element corresponds, the fact is that it represents a technical element or, at the very least a non-economic element in the inventive process.

The same can be said of aesthetic motivations to technical change. Here again there is little literature on the subject. However, the subject has been broached by Cyril Stanley Smith, the metallurgist and historian, who has pointed out how many improvements in materials throughout the ages have arisen from aesthetic considerations: "It was precisely the artists's search for a continued diversity of materials that gave this branch of technology its early start and continued liveliness despite an inner complexity which precluded scientific scrutiny until very recently." "The attitudes, needs, and achievements of artists have provided a continuing stimulus to technological discovery...."[125]

Many scattered historical items tend to reinforce Smith's view of the importance of aesthetic elements in technological creativity. Technologists seem to love to achieve symmetry of design, to simplify to basic elements -- just as the sculptors and architects of classical antiquity sought for symmetry, balance, and simplicity.

The point is that various elements in the creative process can derive from non-economic bases -- play, art -- and the literature scarcely touches upon these vital human forces in the innovative process. Even if economic elements are the overwhelming factors in innovation, the creative imagination of the inventor -- whether motivated by money, aesthetics, play, love, or hate -- is a necessary component in the complex interplay of ideas, applications, and financial accounting which enter into the innovation process. The technical factor need not always be equated with the economic factor.

D. Science-Technology Interactions

The technological background includes elements of human skill and capacity -- in a word, of knowledge. Since, in the 20th century, the basis for technical knowledge is increasingly derived from science, there is widespread belief that technology is an offshoot of science, that technology is applied science, and that there is a simple linear cause-and-effect relationship between science and technology.

The first clear statement of the notion that scientific discoveries breed technical applications came from Sir Francis Bacon in the 17th century. Bacon thought of knowledge as power, and of scientific knowledge as especially powerful in making nature useful to mankind. Bacon's idea of the eventual application of science for useful purposes has remained a major element of the mythology of science. Its hold upon the American mind is evidenced by Joseph Henry, the first secretary of the Smithsonian Institution and a major figure in America's scientific community in the 19th century, who had a "deep rooted belief that the useful arts should and actually do depend wholly on discoveries in pure science."[126] So solidly was this view imbedded in American scientific thinking that over a century later, Alan F. Waterman, the first head of the National Science Foundation, claimed that there is "statistical evidence that most of the body of science ultimately achieves practical utility."[127] Of course, it is impossible to quarrel with that kind of statement, even though Dr. Waterman never adduced the "statistical evidence." If one points out some item of basic research which has not yet received practicable application, one can always be told, "Just wait until 'ultimately' comes along."

The same naive view of basic science as being essential to technological innovation has also been put forward by more sophisticated students of the R&D process. In their discussion of military R&D, for example, Merton J. Peck and Frederick M. Scherer delineate a four-stage process, with basic scientific activity being the first step.[128] Similarly, Marschak views the inventive process as a sequential decision-making process, with basic research as the first stage in a rigorous linear development.[129]

Some outstanding historical cases would seem to lend credence to this view that basic scientific research leads to innovation. Over a century ago Faraday's researches in electricity unquestionably laid the basis for the development of the electrical industry, one of man's most useful technologies. During our own century theoretical speculations about the nucleus of the atom had a shattering impact on warfare and international politics and then on many peacetime technologies. Even more recently, basic research in solid-state physics led to the development of the transistor with its myriad uses.

If these spectacular examples were typical of all science-technology relationships, the problem of stimulating inventive activity -- although not the entire innovation process with its great dependence upon economic factors -- would be immensely simplified. Massive support of basic scientific research would be the key to unlock the door to innovation; our problem, then, would be narrowed down to linking the inventions produced by this support of basic science with the marketplace.

Unfortunately, however, science-technology interrelationships are not quite so simple. For one thing, the historical data tend to show that the examples cited above may be atypical. The simple linear cause-and-effect model of science-technology relationships simply does not conform to the sophisticated, subtle, and complex interrelationships which characterize the interplay between science and technology, and which are further complicated by the intrusion of other factors. Second, the examples provide little guidance for linking together the social need and human wants, to which technology ministers, with the type and nature of the basic scientific research; or, as Nobel laureate Charles H. Townes has put it, research planning becomes a problem of uncertainty because of the "surprise" element in the technology emerging from basic research.[130]

Today's close relationship between science and technology is very modern in origin. Throughout history science and technology addressed themselves to different problems, pursued different methodologies, and were practiced by different groups. True, science and technology must have been closely related -- if not identical -- at the dawn of human history. Magic and the rituals of the hunt and food gathering were both sciences and technologies. By classical antiquity, however, sociocultural developments had forced a wedge between the two. Science was largely speculative, a matter of philosophical enquiry about the nature of man and the universe, and such speculations were confined largely to a leisured class of intellectuals; technology, however, stemmed from the arts and crafts usually carried on by slaves and other humble folk in ancient and, later, medieval times.

Nevertheless, science and technology crossed paths occasionally during the Middle Ages. For example, the mariner's compass, which came to Europe during the 13th century, aroused interest in magnetic phenomena. The scientific need for measuring time resulted in the development of clocks, which in turn helped develop more scientific instrumentation. Thus a dialectical process involving chains of interlocked scientific discoveries and technical advances began to characterize the relationships between science and technology.[131]

Historians have long engaged in dispute about how much and in what way the conceptual science of the Scientific Revolution was shaped by the contemporary technology.[132] Even in the 17th century, the relations between science and technology did not present a simple flow pattern from one to the other; instead, the evidence sustains the disorder one would expect of dialectical processes, that is, the posing of questions from one to another, with solutions suggesting new problems to each. This is not dialectical in the Hegelian-Marxist sense, but in the Greek sense of the Socratic dialogue, with the dialectic proceeding from questions to answers to succeeding questions and answers.

The role science played in the Industrial Revolution is equally obscure. The great technological feats of the mid-18th century -- the hallmark inventions of the Industrial Revolution -- came largely from men who were tinkerers and gadgeteers, without formal training in science, although they were not scientifically illiterate. Most of the great achievements in the age seemed to rely little if at all upon scientific findings -- although there remains much dispute among historians about this.[133]

We can trace the origins of a scientific technology to the close of the 18th century. John Smeaton's work in systematically investigating the potentialities of the Newcomen engine and Watt's horesepower diagram are early manifestations of the development of engineering science as a special discipline, standing about midway in the science-technology spectrum. Ideas of precision, quantification, and mathematization led to the development of engineering "laws" as the 19th century progressed. Technology began to employ the instruments and methods of observation, experimentation, and measurement which science had already adopted during the Scientific Revolution. Of course, all technological devices and processes had worked, since the beginning of time, according to scientific principles, but technologists until the 19th century, were, for the most part, blithely unconcerned with such matters. Their method was to cut and try. If it worked, fine; if it did not work, try again, without any special consideration of the scientific principles involved. But, in the 19th century, new technologies arose, founded upon developments in science, especially electromagnetism and chemistry, which could scarcely advance at all unless the technologists had at least a passing acquaintance with basic scientific findings. Furthermore -- and this is fundamental -- engineers soon learned that scientists had not investigated all the meaningful phenomena or, if they had, they had omitted much of direct interest to the engineers because the scientists had been interested in something else. Hence it became necessary for technologists to conduct their own scientific investigations directed toward fulfillment of technological goals.

Good examples are Orville and Wilbur Wright, studying the possibilities of heavier-than-air flight at the beginning of this century. They had to conduct aerodynamic investigations on the lift properties of wing shapes at possible velocities as a means of controlling a plane in flight. From their investigations they invented the airpoil, which made powered flight feasible, especially with the newly-developed internal combustion engine which provided a compact but powerful source of power. The Wright's investigations were scientific though directed toward technological goals. Edwin Layton has pointed out that scientific engineering of this type followed two lines of development in the 19th century.[134] One involved building directly on the foundations of science. For example, the science of mechanics was extended to create new, technologically-oriented sciences in such areas as materials and hydraulics. A second line of development used the experimental methods of science to found new sciences built on existing craft practices. Here many engineers, unless they came from science-oriented schools like the French École Polytechnique, foundered upon their own ignorance of science and mathematics. For example, few scientists have had a greater impact on technology than did James Clerk Maxwell, but his influence was indirect, since few engineers could understand him; it required a creative effort by Oliver Heaviside, a British engineer, to translate Maxwell's equations into a form usable by engineers. The engineering scientists mastered these difficulties and served as intermediaries between pure science and technical application.

Science-technology relationships have thus broadened out into a wide spectrum of interactions. This broad spectrum includes, at one end, the scientists who pursue scientific investigations of their own choosing with little technical apparatus and no concern about eventual applications. At the other end are those engineers who are unconcerned about the latest scientific findings and make use only of the textbook science they picked up in school. And then there is a large middle ground where science and technology come together in a variety of relationships, with scientists using technology in order to do their scientific experiments and engineers putting the latest scientific findings to work in the most sophisticated fields of technology.

Yet we are still at an early stage in our understanding of these intricate interrelationships. In their study of the Sources of Invention, John Jewkes, David Sawers, and Richard Stillerman state: "It is not known whether there is a necessary connection between the growth of scientific knowledge and the growth of technology and invention or, if there is a connection, what are its laws." Joseph Ben-David has argued that we cannot anticipate and plan for major breakthroughs in science-based technology.[135] While admitting that there is no necessary connection between scientific and technological activities, he nevertheless claimed that steps can be taken to increase the probabilities of scientific advances being exploited by technologists through increasing the motivation and opportunity to find uses for science.

Ben-David thus raises the question of linking together the basic science with useful technology. However, as Townes has pointed out,[136] it is difficult to decide what types of technology are wanted for the future, and even more difficult to figure out just which basic research will contribute information needed for these technological advances. Scientists themselves are not a reliable guide to the potential use of basic science. For example, Einstein's deduction of the equivalence of mass and energy should have given some inkling of the possibilities of nuclear energy early in the century, but when the field of nuclear physics opened up a few decades later, a Herald Tribune headline of 1933 read: "Lord Rutherford Scoffs at Theory of Harnessing Energy in Laboratories." Rutherford, the father of nuclear physics and one of the greatest experimental physicists of his time, was quoted as saying: "The energy produced by breaking down of the atom is a very poor kind of thing. Anyone who expects a source of power from the transformation of these atoms is talking moonshine." Dr. I. I. Rabi of Columbia University, a future Nobel laureate on the basis of his work in the same field, confirmed Rutherford's negative pronostication. Yet only five years later the unlooked-for phenomenon of fission was discovered, and the possibilities of nuclear energy were clear to even the most skeptical.

The point is that we cannot make sure just where our science is leading us, and we cannot be certain that leadership in basic scientific research will lead inexorably and inevitably to technological innovation and leadership. For example, just as the time -- mid-18th through early 19th century -- when English science was slipping from the peak represented by Newton, Britain was assuming technological leadership in textiles, steam power, and metallurgy, while France, despite her preeminence in science, lagged behind Britain in industrial progress. During the 19th century Russia produced many great scientists and mathematicians, but these had no impact upon Russia's industrial development. Also, when America was thrusting toward industrial leadership in in the 19th century, American interest and accomplishments in basic science were not on a par with her technological advances.[137] In the period following World War II, Japan advanced dramatically in technological matters, but the development of a top-rank scientific community in Japan has followed, rather than preceded, its industrial spurt.[138] Indeed, the fields of science being developed in Japan are those particularly related to industries in which they compete in international markets.

While industrial growth and scientific advance can be independent of one another, there are some cases where the two have gone hand in hand. Particularly notable in these respects are Germany's leadership in chemistry and the chemical industries in the late 19th century and, of course America's scientific and industrial supremacy in atomic energy, computers, electronics,and aerospace fields during the past decades.

One reason why scientific and technological advance do not always go hand in hand is that much technological innovation is not necessarily dependent upon new scientific discoveries. For example, the development of various machines in 19th century America -- such devices as the reaper, typewriter, barbed wire, sewing machine -- did not involve the discovery of any new scientific principles; all they required was mechanical skill and ingenuity.

Furthermore, Americans were able to further the development of inventions originating in Europe; the automobile is a prime example. The same tendency to utilize innovations from abroad exists today; in the past two decades major technical breakthroughs in glassmaking and steel making have been imported from Europe; OECD studies point out, "U. S. firms have turned into commercially successfully products results of fundamental research and invention originating in Europe. Few cases have been found of the reverse process...."[139]

The point is neither that science is unnecessary nor essential for technological innovation; that is dependent upon the technical field, its level, and a number of other factors.

Yet in some cases it would seem that science and technology can feed upon each other; they can have a synergistic effect to enlarge the capacities of both. A good example would be the interplay between science and technology in the invention and development of the transistor. This arose from work in solid-state physics, and the invention of the transistor in 1948 led to a number of devices for transistorized radios, memory cores for computers, integrated circuits, and other sophisticated devices. In turn, these technologies stimulated further research into the fundamental properties of solids -- and these advances led in turn to other devices. In other words, the dialectic gathered momentum, with science and technology reinforcing one another.

Recognition of the complex relationships between science and technology has stimulated efforts to analyze their effects on innovations in various fields. One of the most ambitious was Project Hindsight, the name given by the Department of Defense for a series of studies of recent science and technology utilized in weapon systems.[14] It was initiated in 1964 to establish the effectiveness of the $10 billion invested by the DOD in basic and applied research since 1945, and to determine which, if any, management patterns or practices appeared conducive to a particularly high payoff. Teams of scientists and engineers "dissected" each of 20 weapon systems to identify each contribution from recent (post-1945) science and technology which was clearly important either to improve system performance or to reduce cost. Each contribution was termed an "event," defined as a period of creative effort ending with new, significant knowledge or with the demonstration of the applicability of a new engineering concept. Events were divided into Science Events or Technology Events, with Science Events being subdivided into Undirected Science (in which the object of the work is the advancement of knowledge, without regard to possible application) and Applied or Directed Science (in which the object of the work is to produce specific knowledge on understanding of phenomena needed for some particular use or uses). Of the 710 Events identified, only 9% were classified as Science Events -- 8.7% as Applied Science, and only 0.3% as Undirected Science.

Publication of the interim Hindsight report, with its diminution of the role of basic science in weapon innovations, raised a great hue and cry among scientific researchers, especially those in academia. So great was the outcry that the final report of Project Hindsight explicitly stated that none of its findings should be interpreted as a disavowal of the value of very fundamental research in science; the finding suggested only that such research is most likely to be utilized when undertaken in a purposeful manner, that is, when deliberate attempts are made to relate the research results to specific problems.

It is not surprising that the National Science Foundation, responsible for the funding of basic science, reacted (1967) by underwriting an investigation by the IIT Research Institute for a systematic study of the role of basic scientifc research in technological innovation.[141] That report, TRACES (Technology in Retrospect and Critical Events in Science) can be regarded as academia's reply to Project Hindsight. Key scientific events which led toward five major technological innovations were traced. Unlike Hindsight, TRACES did not deal with weapon systems, but with other developments of social and economic significance: birth control pills, the electron microscope, videotape recording, ceramic-metallic materials, and matrix isolation. Instead of setting a backward time-limit at 1945, TRACES went back more than a century in studying the scientific roots of certain innovations. Dividing its key events into non-mission research, mission-oriented research, and development and application, TRACES attributed 70% to non-mission research, only 20% to mission-oriented research, and 10% to development and application. Furthermore, the number of non-mission events peaked significantly between the 20th and 30th year prior to an innovation, whereas Hindsight found a delay of only five to ten years between the DOD investment in research and the payoff.

TRACES and Project Hindsight are not entirely contradictory. Had Project Hindsight looked further back in time or investigated a wider field than weapon systems, its conclusions might have been much more closer in percentages in those of TRACES (after all, practically every development in electricity can be traced back to Ohm's Law!) On the other hand, TRACES might have come up with somewhat different figures had it chosen a different set of innovations, particularly some involving mechanical rather than chemical, biological, and electronic devices. The fact of the matter is that both studies agreed that both fundamental and applied research do play roles in innovative activity and that mission-oriented research becomes increasingly important as the final innovation approaches. Without realizing it, both studies showed that dialectical processes occur between science and technology which often establish certain interdependencies for the solution of specific problems.

Later studies have tended to validate the basic findings of Project Hindsight, namely, that most innovation does not result from the direct application of new scientific discoveries. This was the conclusion of Project Sappho, at the University of Sussex in England.[142] It was also the conclusion of Sumner Myers and Donald G. Marquis in a study of Successful Industrial Innovation.[143] An even more thorough study of 84 innovations carried on under the direction of J. Langrish by the Department of Liberal Studies in Science at the University of Manchester, arrived at the same conclusion.[144] The Langrish study found it difficult to pinpoint any innovations deriving from curiosity-oriented research; it found that only two innovations of the 20th century arose from basic research motivated by scientific curiosity rather than by utilitarian goals: nuclear power and silicones. In the case of the innovations studied at Manchester, the need-pull was more important than the discovery-push.

Equally important was the conclusion of the Manchester group that innovations do not have single unique origins, but arise from a combination of factors. Perhaps the most important of these is the presence of a market -- a reinforcement of the Schmookler demand thesis. That thesis is also reinforced by a British study entitled On the Shelf, a Survey of Industrial R&D Projects Rejected for Non-Technical Reasons.[145] In almost all cases where inventions were not actually applied, it was because of the absence of a market. There were other reasons too, some of which were closely tied to market demands: sometimes it was too expensive; sometimes there was cheaper competition which was inferior, but still would harm the possible market; sometimes it was the absence of capital; and so on. So, just as innovations do not have unique origins, so it can also be said that their failure to achieve the status of innovations might also be due to several causes.

If we were to accept the general conclusions of all the above-mentioned studies of the science-technology relationship (with the notable exception of TRACES), we would be left with the false conclusion that fundamental, or curiosity-oriented, research does not enter to any large extent in the innovative process. On the basis of such a conclusion, support of basic science as leading to innovation would be an act of public faith in scientists and the scientific enterprise -- "faith that, with the support of society, serendipity will function and scientists will continue to discover things that they did not know they were even looking for."[146] But such an act of faith might not be necessary, for the most recent studies of the relations between science and technology indicate that seeking the origin of innovation in terms of previous scientific discoveries is a naive and inadequate approach to the problem. A more sophisticated approach is necessary, and we have recently been moving in that direction.

For one thing, we should not tie scientific discoveries too tightly to predetermined goals. As Simon Ramo has said, "Man did not invent the automobile by breeding better and better race horses." More important, we should not regard scientific research as just a starting point for technical innovation; it is a part of virtually all of the phases of the innovation process. Scientific discoveries, whether mission or curiosity-oriented, enter into different phases of the innovative process; they are part of the ecological feedback mechanism where problem-definition and idea generation, invention, R&D, and application weave back and forth in a dialectical manner in order to arrive at an innovation.

A second-generation study carried on by M. Gibbons and R. D. Johnston at the University of Manchester[147] considered the technological aspect of some 30 recent innovations from the standpoint of the interaction between both scientific and technical knowledge and the scientific and technological communities. The innovations examined arose most frequently from the conjunction of a technological need or opportunity and a positive assessment of actual or potential market -- acting as joint stimuli. The investigators further found that the information used to resolve technical problems in these innovations originated approximately equally from three general sources: outside the company; from within the company, excluding the problem-solver himself; and from the problem-solver's own expertise. Over one-third of such information originated from scientific activity, with the rest principally technological. By coupling the information source with the user, Gibbons and Johnston thus demonstrated a positive contribution of science to industrial innovation. Or, to put it in metaphorical terms, their study showed that science was the nursemaid of innovation, just as the market was its mother. Even though the innovation might not be directly induced by science, the scientific endeavor was supportive of innovation.

A recent study by Battelle Columbus Laboratories[148] shows how science enters into the innovative process at various points. Concentrating on case studies of the heart pacemaker, hybrid grains, electrophotography, input-output economic analysis, organophosphorus insecticides, oral contraceptives, magnetic ferrites, and videotape recording, the Battelle group concluded that basic or fundamental research -- that is, non-mission oriented research -- fathers events which are important in all phases of the innovative process. However, the scientific influence is more important in the earlier than in the later history of an innovation. Furthermore, the investigators found that certain technological achievements, such as hybrid grain, involved more than one innovation. Some of the other conclusions reached by the Battelle group are similar to those stated earlier, for example, the importance of the entrepreneur in promoting innovation, the recognition of need (the market or demand factor), and the like.

Melvin Kranzberg has attempted to formulate push-pull models of the interactions of science and technology in the innovation process; although his models relate specifically to weapon systems, they are also applicable to non-military technology.[149] In his most complex model, Kranzberg shows how scientific discoveries or technological innovations can serve as pushes for innovative developments, just as can military needs. In the course of the technological development, it is frequently realized that more basic knowledge must be sought if workable items are to be obtained; the result is a push from technology directed toward science in order to acquire the requisite science so that there can be further development of the hardware. But, since changes in one hardware item often require changes in other components, additional pushes are exerted for specific research or technological innovation to answer the newly arisen needs. Yet at the same time, the military (or civilian corporation) pushes for some undirected scientific breakthroughs that might provide the potential for new capabilities. Hence, both basic science and mission-oriented science can be involved in the developmental and application phases as well as in the problem generation and inventive phases of the innovation process.

None of the case studies or models mentioned above -- even that of Kranzberg -- gives us a complete and definitive picture of science-technology relationships in the innovative process. Nevertheless, it is clear that we are now viewing their interaction in a more sophisticated manner; no longer are we confined to the naive and narrow view of the one-way application of science to produce technological innovations. We now recognize that technology induces science, just as science sometimes induces technology, and that these continue to interact throughout the phases of innovation. Several studies are underway which attempt to delineate the interactions more specifically in different fields of technology and in different organizational contexts. In all cases we would expect to find that science and technology enter into a feedback relationship in the ecological process of innovation.

V. Some Brief Illustrative Case Studies

For analytical purposes we have described separately the many factors entering into the complex ecological system of innovation. Two questions immediately arise: (1) To what extent does our abstract analysis accord with empirical data? (2) How can we synthesize the separate elements into a meaningful description of the innovative process?

Perhaps the best way to answer both questions is to follow the story of some actual innovations from start to finish. What follows, therefore, are a few brief case studies of some important innovations where we have ample historical data and which serve to illustrate the analytical and synthetical elements of our ecological approach to the innovative process. Each case study emphasizes different aspects but all of them demonstrate the complexities involved in the socio-ecological innovation system.

A. James Watt and the Steam Engine

The story of James Watt and the steam engine highlights several of the ecological factors in the innovation process: the technological level and capability of the time, problem definition and idea generation, the role of the entrepreneur, and the crucial importance of the R&D phase in arriving at a saleable invention.

At the beginning of the 18th century water was the main source of power in England;no factory could be established far from a stream powerful and swift enough to work its machines. In some parts of England the mills were crowded into narrow valleys, where, by constructing dams, artificial falls could be created which would provide sufficient power. But in most parts of England there were no such narrow valleys and the water was slow moving; the only practicable method was to create artificial waterfalls, raising the water to the level of a reservoir by means of a pump. There was thus a great need for an efficient pumpting device -- to help create the waterflow which ran the fulling-stocks, millstones, saws, bellows and ore-crushers of early modern industry.

The need for an efficient pumping device was even more apparent in the mines of England -- to pump out the water. The tin mines of Cornwall and the collieries of the English midlands could not, like the mills, be moved to banks of streams where water could power the pumps to drain the mines. Nor could windmills be used effectively throughout England, for there was no steady supply of wind like that blowing across the flat Netherlands. And treadmills powered by horses were inefficient and hence costly.

The problem of pumping out the mines was magnified by the growth of the extractive industries at the beginning of the 18th century when Abraham Darby discovered (1709) that coke could be used for smelting iron. Charcoal, which had been used previously, was in short supply, for England had been partially deforested as a result of the rapacious demands of the growing iron industry for charcoal fuel. Darby's discovery that coke could be used in place of charcoal to smelt iron under certain conditions meant that there was a growing demand for coal -- and hence even greater demand for a new source of power to pump out the coal mines. The English metallurgical industry -- from colliery to smelter to foundry -- was economically threatened at the beginning of the 18th century by the scarcity of charcoal and the limitations of waterpower (for example, the average period of working in an ironworks whose blast was produced by a bellows worked by waterpower was only 40 weeks a year, and even this period was considerably reduced by a dry season which curtailed the limited flow of water available). There was ample economic incentive, then, for a new source of power coming from the coal mines and the iron industry, to say nothing of the Cornish tin mines and other industries which also suffered from a shortage of power.

To meet this need for power -- and also setting the stage technologically for Watt's later invention -- was the "atmospheric" engine devised by Thomas Newcomen, an ironmonger and smith from Dartford, who furnished iron tools to many of the Cornish tin mines and hence was familiar with their problems of pumping water from the mines and with the high cost of doing so by means of horses. Newcomen's engine, which was first put to work in 1712, was really a primitive steam-engine; it is called an "atmospheric" engine because while steam pressure was used to push a piston to the top of a cylinder, the piston was returned to the bottom of the cylinder by condensing the steam through a cold jet of water and allowing the pressure of the atmosphere to force the piston back, and thus raise a pump-rod. The cycle was then started again by admitting more steam into the cylinder. It was clumsy and inefficient, but it was cheaper than using horses to power the pumps drawing water from the Cornish tin mines. By 1725 Newcomen's engine had come into widespread use, on the Continent as well as in England, to drain water from mines, and also to pump water to a height where it could be used on a water-wheel to power machinery.

The Newcomen engine thus set the technological stage when James Watt came on the scene. As instrument maker to the University of Glasgow, Watt was given a model of a Newcomen engine to repair in 1763. He repaired the model easily, but in the course of doing so he was impressed by its great consumption of steam. To utilize steam efficiently, the cylinder would have to be kept at 100°C, so there would be no condensation during the piston's unpowered stroke; but to form an effective vacuum for the power stroke, considerable cooling water had to be injected into the cylinder, cooling it well below 100°C. For two years, Watt tinkered with the Newcomen engine; thinking the heat loss might be owing to conduction of heat through the cylinder walls, he tried a wooden cylinder rather than the brass one. It did not solve the problem of heat loss -- the problem which he had very accurately identified.

Then came the idea generation: Watt tells us that while strolling on the Glasgow Green "on a fine Sabbath afternoon" early in 1765, he hit upon the idea of condensing the engine's steam not in the operating cylinder, as Newcomen had done, but rather in a separate condensing chamber. This act of creative insight was the basis of Watt's steam engine; it was, of course, essential to the inventive process, but what followed is perhaps even more illustrative of the significance of the succeeding phases of the process of innovation.

Watt conceived his brilliant idea of the separate condenser in 1765, but it was not until 1769 that he received his first patent, and it was more than a decade later, 1776, that the first Watt engine was brought into commercial use. What happened during the eleven years between Watt's act of insight and the successful commercial installation of his first engine proves the importance -- and difficulty -- of the developmental stage in transforming an idea into a practicable innovation. Here, the question of defining the technical problems and viewing them in their economic context was to prove crucial -- and this was to be largely the work of Matthew Boulton, not of Watt who had the original idea.

On the Monday following his Sunday afternoon walk, Watt began work on a small model to test his idea of a separate condenser. "In three days," he says, "I had a model at work nearly as perfect as any which had been made since that time." Watt had his invention -- but he still did not have an innovation. For the model, although successfully demonstrating the principle of the separate condenser, was far from a full-scale steam engine; it had to be scaled up and still made to work efficiently and reliably. That task consumed the next ten years. It was accomplished only through the application of Watt's technical skill and of more managerial and entrepreneurial expertise than Watt himself possessed.

For one thing, Watt had no capital. He could not work full time at developing a full-scale engine; he had to take on other work, chiefly as a surveyor, in order to keep himself alive. Eventually (1768), he found financial backing from Dr. John Roebuck, an enterprising industrialist who was a partner in an iron works producing major components for Newcomen engines and who was also involved in a coal-mining venture. But Roebuck's coal pits had flooded, and atmospheric engines were not powerful enough to drain out the water; if Watt's engine were successful, it might, among other things, save Roebuck's otherwise lost investment in the coal mines. For a two-thirds share in Watt's invention, Roebuck paid off Watt's debts (amounting to more than £1,000) and bore the cost of the patent in 1769. But Roebuck, suffering financial embarrassment from his other ventures, soon found it impossible to support Watt's developmental work on a full-time basis. In 1770 Watt was forced to abandon work on his steam engine and return to surveying in order to earn a living. He did not resume full-time work on the steam engine until 1774, when he found a new partner in the person of Matthew Boulton. What happened was that Roebuck went bankrupt in 1773; his share in Watt's invention, which the bankruptcy receivers valued at only a farthing, was taken over by Boulton in discharge of the debts owed him by Roebuck. Boulton, owner of a metallurgical works at Soho, near Birmingham, was interested in Watt's engine for two reasons: His Soho factor was short of power -- summer water shortages made his water wheels inoperative and necessitated the expensive employment of horses to propel his machinery; and he also foresaw the profit potentialities in producing steam engines for mines and factories throughout the world.

Boulton replaced Roebuck as Watt's partner, and they established the firm of Boulton & Watt to make steam engines. The combination of Watt's technical skill and Boulton's managerial ability and financial support eventually paid off.

The technical difficulties were immense. Watt built successive models, increasingly larger, in an effort to scale-up his device to a commercially practicable machine. Along this hard road were baffling technical problems. For example, there was the problem of piston-cylinder fit. To make the engine work efficiently, the piston had to fit tightly within the cylinder, so as not to dissipate the vacuum. Yet excessive friction also had to be avoided to maintain efficiency. Too tight or too loose, the engine would not work properly, and techniques of metal working of the time were primitive, making a tight metal-to-metal fit difficult. Watt tried one approach after another, experimenting with tin, copper, wood, and cast iron for his cylinders and pistons to find materials retaining strength and durability while being worked to the closest possible tolerances. He thought of square pistons, round pistons, flexible pistons; he experimented with piston disks and piston rings of different materials -- leather, pasteboard, cloth, cork, oakum, hemp, asbestos, and various lead alloys. He tried a variety of materials to help seal the piston: -- mercury, oil, graphite, tallow, horse dung, and vegetable oil. Here is a case showing how additional inventions were necessary to make the original invention a successful innovation. New problems kept arising, and Watt had to produce new solutions to cope with them. Had Watt had his way, and lived, his experiments would probably still be going on today. We would have models but no finished product, for Watt was a perfectionist. He wanted to try every possible combination of materials and arrangements in an effort to make his engine as perfect as possible.

Boulton, the practical manager, kept Watt's perfectionism within bounds. He had Watt employing his technical skills not only to strictly mechanical problems of the engine itself but also to simplify the fabrication of parts; to devise procedures for the construction and maintenance of the engines; to prolong the life of the engine components; to avoid expensive materials; and to increase the operating efficiency of the engine. Again, new problems, new ideas, new "sub-inventions." In the end, the problem of piston-cylinder fit was largely resolved by John Wilkinson's invention (1774) of a boring-mill -- originally for cannon -- capable of boring cylinders to a tolerance of no more than "the thickness of an old shilling," a remarkable feat for that time.

By the time (1776) that Boulton and Watt installed their first two engines -- one to pump out a coal mine and the other to blow the blast-furnaces at Wilkinson's factory -- Watt's steam engine had been brought to a degree of efficiency where it consumed less than a third of the coal which the improved Newcomen engines of the times used. The firm was immediately besieged with orders from the Cornish tin mines where the price of coal was high and where the cost of pumping limited the depth at which mining was practicable. Boulton and Watt charged a royalty to the users equal to one-third of the saving in the cost of fuel as compared with the other engines then in use; and Boulton estimated that their steam engine was only half as expensive as the number of horses for which it was substituted in those places which still used horse power rather than Newcomen-type engines.

Even when Boulton and Watt had developed and sold successful pumping engines they continued to make improvements: in 1782 Watt made the engine double-acting -- that is, admitting steam alternately to each side of the piston -- thereby developing twice the power from the same cylinder volume; he also devised a sun-and-planet gear to convert the engine's reciprocal action to rotating action, thereby giving it wider application in manufacturing industry. He made other improvements too, and by the time Watt's patent, which had been extended to 1800, ran out, the firm of Boulton & Watt had constructed 496 engines. Of these, 164 -- mostly the earlier ones -- were used for pumping purposes, 24 served blast-furnaces, and the remaining 308, especially those with rotary motion, functioned directly as prime-movers for machinery.

In 1800 Watt retired from the firm; he was a rich man, and in a very real sense he had been made so by Matthew Boulton. Boulton's managerial skill and business sense -- Watt had no stomach for dealing with bankers, suppliers, or customers -- were essential to the process of transforming Watt's idea into a full-scale, commercially successful steam engine. Yet the story of the steam engine, as most of us learned it, tells little about the developmental work and the ancillary inventions which followed Watt's creative insight, even less about the entrepreneurial and managerial role of Matthew Boulton. Watt had his bright idea while taking a Sunday afternoon walk, and, presto, there was the steam engine! If Boulton is mentioned at all, he is relegated to the position of a financial backer, almost a silent partner. So run the myths of history. In actual fact, Boulton deserves equal credit with Watt. The steam engine could not have been developed without the contributions of both.

There still remains the question of the amount of science involved in Watt's fundamental discovery of the separate condenser. Historians of science and technology have argued over the question of whether Watt's invention was derived from Joseph Black's work on latent heat. But Watt had never been a pupil of Black, had never attended his lectures, and although he was very friendly with Black, he independently observed for himself that "water under the form of steam could contain more heat than it did when water." Watt was not an untutored tinkerer, as were many other inventors of the Industrial Revolution, but he was not trained in science, and his understanding of the physical processes that he was involved in altering was sometimes faulty. Nevertheless, Watt "regarded the steam engine in...a scientific light"[150]; but it was his own scientific approach, not science derived from the professional scientists of the time. The arguments surrounding the amount and degree of "science" in Watt's invention have probably generated more heat among scholars than was generated by his original steam engine.[151] The important fact to us, however, is that Watt's steam engine invention indicates the importance of going back and constantly reformulating problems and getting new ideas at many different phases of the innovation process; it also demonstrates the significance of external factors and individuals in bringing the invention to fruition as a successful innovation.

B. Robert Fulton and the Steam Boat

Problem definition in economic terms and commercial application, calling upon business acumen and decision-making talent, are represented in Robert Fulton's "invention" of the steamboat. Actually, Fulton "invented" very little from the technical point of view, but was remarkable in his ability to perceive that the problem of innovation in the case of the steamboat required special attention to the economic parameter.

Every schoolboy "knows" that Fulton invented the steamboat in 1807. Not so. All the elements for innovation of the steamboat were already present when Fulton went to work on the problem. The need for steam propulsion on inland waterways was already apparent to many: the boat itself -- really a barge for a heavy engine and its fuel -- had been around for several thousand years, and the steam engine as a workable device had been perfected several decades earlier by Watt. Indeed, some 35 steamboats were built before Fulton's Clermont, all of them propelling themselves with varying degrees of success, and Fulton had studied all the previous experiments carefully.[152]

What Fulton did was to re-define the problem: it was no longer wholly the technical problem of getting a steam engine to propel a boat through water -- the feasibility of that had already been demonstrated -- but the problem of getting a steam engine to propel a boat through water at a profit. Fulton's technical contribution to the invention is at best obscure. We attribute the steamboat to Fulton largely because he made it a commercial success. And he did that largely through the exercise of economic and political insight rather than technical creativity.

Unlike most of the steamboat experimenters who preceded him, Fulton did not attempt to design or build his own engine, for he aimed at developing a steamboat, not designing a steam engine. He brought one from Boulton & Watt, and that gave him a much more efficient engine than his competitors. He coupled that engine to paddle-wheels, already used by others and easily the most efficient propulsive device available when the prime mover was a low-speed steam engine.[153] Previous steamboats had been so inefficient that their fuel supply left little room for cargo. Fulton's combination of an efficient prime mover and an efficient propulsive device gave him the cargo capacity that could make his steamboat service commercially profitable. Perhaps most important, Fulton was an able political negotiator as well as a businessman. To obtain financial backing he had entered into a partnership with Robert R. Livingston, a leading member of the New York Squirearchy and a former chancellor of New York state and American minister to France. Livingston persuaded his friends in the New York legislature to grant the Fulton-Livingston partnership a monopoly franchise for steamboat operations on the Hudson River, where growing trade assured the profitability of the venture.

Ironically, Fulton's one original technical contribution was the hull of his boat: a highly unorthodox barge design that proved a definite hindrance in moving the boat through the water. By its second season the *Clermont*'s hull was rebuilt along more traditional lines. Plainly, Fulton's contribution to the invention of the steamboat was in non-technical problem definition and in the application stages of the innovative process. He alone perceived that the goal was commercial success rather than a technical triumph, and he utilized his skill at handling people to transform the ideas and technical devices of others into a great innovation.

C. Alexander Graham Bell and the Telephone

The telephone provides examples of the importance of the technological level and of interrelationships among different phases in the innovative process. In 1838 Samuel F. B. Morse first demonstrated to members of Congress that a wire carrying an electric current could be made to carry a message simply by making and breaking the current in a coded fashion -- the familiar dots and dashes of the code which he himself devised. When Morse's telegraph proved a resounding commercial success, it was inevitable that a number of scientists and inventors should turn their attention to the problem of transmitting a message over electric wires by the sound of the human voice itself, without the cumbersome system of dots and dashes. By the end of the 1860s several investigators had already figured out the scientific principle involved, namely, the problem of superimposing a variable current, induced by the human voice, on a continuous current -- that is, modulating a current by the sound of the voice, and then of course, translating the current back into the sound at the receiver end.

The input from science thus came very early in the game; the principle was simple enough, but realizing the idea in a practical device was not easy. Many investigators came close --

the times were ripe in terms of technical knowledge and expertise -- and by 1870 it was no longer a question of "whether" but "when" and "by whom" a telephone would be invented. The honor eventually went to Alexander Graham Bell, who in 1876 successfully combined ingenuity and insight in the developmental stage of the innovative process which enabled him to build and demonstrate a practicable telephone.[154]

That other inventors had also reached the same insight is attested by the fact that on February 14, 1876, when Elisha Gray sought to file a patent for a telephone, he discovered that Bell had already applied for a patent for a similar mechanism earlier that very same day. The prolonged patent litigation which followed the success of Bell's system also demonstrates how close others had come to reaching the same telephonic device as had Bell. Others had the right idea, and some had even made devices which almost worked. Bell alone closed the loop of the innovation by the successful development of the transmitter and receiver. Yet Bell's original telephone scarcely was an entire system. It required a number of auxiliary inventions, and many other technical and economic problems had to be surmounted before a telephone system was technologically operative and commercially significant.

D. Edison and the Electric Light

Thomas A. Edison came closest to doing the complete job of innovation by himself -- from definition of the problem to profitable application and diffusion -- in his innovation of electric lighting. As Thomas P. Hughes has pointed out in his biography of Elmer Sperry,"Only the naive inventor assumes that the challenge is to invent an arc lamp, an electrical generator, a streetcar, or an automobile." Edison saw things in their entirety, and one of the major reasons for his success was that he saw that the problem was to develop an electric lighting system, not just devise an incandescent bulb.[155]

As far back as 1860, the scientific principles and technical requirements of a successful incandescent light bulb were known and had been tried by many inventors. Some had even succeeded in producing lamps which would burn for a short time -- in the laboratory. When home electric systems turned into a successful reality, each claimed to be "the" inventor of the electric light. But of course they were not the inventors at all, for they had taken only the first steps of the innovative process. Edison's concentration on the developmental application, and diffusion stages, with all the auxiliary devices and business services essential to a functioning electric light system, actually produced electrical lighting.

Perhaps the best demonstration of the qualities necessary for the inventor-entrepreneur, who can transform an ingenious idea into a profitable product, comes from Edison's approach to the electric light system. Having already made a great deal of money through his earlier inventions -- stock ticker, quadruplex telegraph, carbon microphone, phonograph, and many others -- Edison was seeking a new technical problem to which he could turn his talents and from which he could extract a profit. Visiting a Westinghouse factory which turned out electric generators and arc lamps, Edison was impressed with the progress made in electrical devices and immediately decided that electrical illumination was a promising field. Even before he started work on the electric light, he had perceived the problem clearly: "I have an idea that I can make the electric light available for all common uses, and supply it at a trifling cost, compared with that of gas....The trouble is in finding a candle that will give a pleasant light, not too intense, which can be turned on or off as easily as gas."

In his search for a proper filament for the incandescent bulb and in his efforts to develop the system around it which would make it commercially exploitable, he was methodical, self-confident, systematic, and persistent -- trying all approaches. But he never lost sight of the fact that he was selling illumination and that his major competitor was the gas burner. When he began to market the electric light, his bulb was sixteen candlepower, the same as that given off by a common gas jet; in his monthly bills, the lights were referred to as burners, and the customer was billed for light-hours, not kilowatts. And his economic foresight was remarkable; he began marketing his bulbs at 40¢, even though they cost $1.25 to make; within five years he had brought the production cost down to 25¢, but he kept the 40¢ price while the sales skyrocketed into millions.

Edison's achievement was as much a triumph of entrepreneurial ability, managerial expertise, and economic reasoning as of technical ingenuity. For example, the work on the lighting system was carried on at Edison's Menlo Park laboratory, which is sometimes considered the prototype of today's industrial research laboratories. True, industrial research was carried on there -- on the carbon telephone transmitter, the phonograph, and many other devices as well as the electric light -- but it was not carried on by teams of research specialists as in today's laboratories. When Edison needed an expert in a certain field, he simply went out and hired him -- and then Edison told the expert what to do, and exactly how to do it. In the case of the electric light, Edison hired Francis R. Upton, a mathematician with some training in physics, to calculate lamp resistance, voltage, and conductor size because Edison recognized his own limitations in mathematics; Upton later conceded that Edison taught him the elementary facts of applying mathematics to electrical questions. Like most of the other workers at Menlo Park, Upton was a hireling, not a collaborator; Edison knew exactly what he wanted and how to get men to work out the details of his ideas.

For those who believe innovation consists only in the production of a working model, the invention of the incandescent lamp is usually dated October 21, 1879. On that day, Edison lit the first practical lightbulb. Yet that was but one of a long series of events, and it was followed by many other developments which were necessary before the entire system worked. Indeed, the publicity stunt at Edison's Menlo Park research laboratory was only the start of his system. He had to design and build the base for lamps, the wiring for houses, the underground cable system for the streets, a meter for measuring the amount of electricity used by a customer, a generator, and --

crowning the entire effort in 1882 -- a central power plant, New York City's famous Pearl Street Station.

Edison thus enlarged the bright idea of the incandescent lamp to the innovation of the electric lighting system. He possessed the financial backing, the economic enterprise, and a largeness of conception, all denied his contemporaries who concentrated on but a single element of the inventive process without viewing the process as a whole.

Edison was the most productive inventor in American history, with some 1093 patents to his name. It is clear that his success rested as much on his combination of managerial expertise, financial skill, economic foresight, and practical wisdom as it did on his technical ingenuity and inventiveness.

E. The Transistor

The development of the transistor brought into play many of the characteristics which we have noted in our ecological view of the innovation process: the linking together of need and opportunity; the progressive redefinition of both technical and scientific problems; the ongoing nature of invention within the innovative process; the synergistic effects of the science-technology dialectic; the need for interdisciplinary research and development for today's sophisticated technology; the organizational pattern of a modern industrial research laboratory; the need for ancillary inventions in order to complete the innovation and perfect it; and the diffusion of the innovation among different technologies, different industrial firms, different industries, and the transfer of the technology abroad.[154]

On the day before Christmas of 1947, Walter H. Brattain recorded in his notebook at the Bell Telephone Laboratories the discovery of the "transistor effect" which he and his colleagues had noted in an experiment the day before. Announcement of this discovery was made several months later, and in June 1948 the "point-contact" transistor was patented. A few years later Brattain's colleague, William Shockley, had improved this to the "junction" transistor, the father of today's devices. By the time in 1956 that the three co-discoverers of the transistor, John Bardeen, Brattain, and Shockley, were awarded the Nobel Prize, the transistor was already a common device.

Bardeen, Shockley, and Brattain, it must be noted, were physicists, not engineers. Their discovery of the transistor effect and the invention of the devices were based on studies of semiconductor materials stretching back almost to the beginning of this century, and on the application of quantum theory, also derived from the first quarter of this century. Bell Telephone Laboratories sponsored their research because Mervin Kelly, head of the Laboratories, had the vague hope that such basic research upon a purely scientific porblem might bave commercial results which would help meet the unprecedented demand for telephone service following World War II.

During the 1930s and 1940s the Bell system had successfully introduced electromechanical switching to replace the manual switching of the early days of the telephone. But the myriads of relays, switches, and vacuum tubes required a high outlay in initial cost as well as expensive and skilled maintenance. As the telephone "explosion" took place -- at a pace which vastly exceeded the concomitant postwar "population explosion" -- Bell officials realized that an inexpensive, reliable, and long-lived electronic switch must be found lest the system break down completely under its own size and complexity.

The importance of management decisions in directing the work of the solid-state research team at Bell Labs was unwittingly emphasized by Shockley at an interview in Chicago in 1969, slightly more than two decades after the great discovery. Shockley said that he had been recruited from MIT in 1936 by Dr. Kelly, who at that time was head of the Vacuum Tube Department at Bell Labs. By the time Shockley reported for work, Kelly had become Vice President for Research, and on Shockley's first day at the Labs, Kelly gave him a sales pitch on the problem of mechanical relays and switches throughout the Bell system. All of this ought to be done electronically, said Kelly; what was really needed was a solid-state amplifier.

But Shockley did not begin to work at once upon this problem. Instead, he worked on order-disorder in alloy systems, and became acquainted with Brattain, who was working with copper oxide rectifiers. In 1939 Shockley learned of the work of Schottky in Germany, who theorized that if a negatively-charged metal plate could be brought near a semi-conductor, it would drive electrons away from the surface area and therefore change its conductivity. This concept offered the opportunity of a solid-state valve, and Shockley tried this out in 1940. It didn't work. Shockley turned to other work -- operations research -- more in line with immediate wartime needs.

When Bell Labs reconstituted its staff after World War II, Kelly set a group to work on semi-conductors, magnetic materials, insulators, and, in fact, on anything which might control electric current. Shockley and his colleagues in the solid-state subgroup went to work to find out why it was that the valve action which they had predicted according to Schottky's theories did not in fact occur. Since the earlier device had not worked, there must be something amiss in their understanding of the underlying physical phenomena. Hence the problem was redefined: it became one of finding out what was wrong with the basic physics. Bardeen was finally able to explain the defects in the theory; his idea was that there were special surface states in which the electrons could get stuck -- and so the charged place outside was not affecting the electrons inside the material at all. In the rudimentary experiments which they devised to test Bardeen's new theory, they were led directly to the transistor action. But even after they had discovered the transistor effect, it took them some two years before they were really able to make any effective transistor.

Note how the problem was first to make a solid-state amplifier; then the problem became one of supplying the missing physics. Once the missing physics had been supplied, the problem was again how to apply this knowledge to produce an effective solid-state amplifier. These problems could not be solved by physicists alone. While the Nobel Prize went to the physicists, the initial semi-conductor research group at Bell Labs included, in addition to the theoretical physicists, a physical chemist and an electronic circuit engineer. Indeed, some thirteen men played a major role in the work leading to the transistor, and this included physicists, chemists, electrical engineers, and metallurgists. In a sense, the concept of a "generalist" inventor was retained, but the individual inventor was replaced by the sum of several specialists.

Once the original invention had been made, much time and effort went into improving it. Brattain-Shockley's original semi-conductor device had amplified the current passing through from twenty to forty times; by successive improvements it became possible to amplify the current more than 40,000 times. Also efforts were made to improve reliability and dependability while reducing costs. In order to do so, even greater understanding of solid-state physics was required. So the dialectic between science and technology resulted in improvements in scientific understanding of solid-state devices, and these in turn led to technical improvements which in turn raised new scientific questions for the "pure" researchers. Within a decade after the discovery of the transistor effect, an amazing amount of scientific knowledge had been accumulated regarding the elemental semi-conductors of germanium and silicon. As scientists learned more about the surface sensitivity and characteristics of semi-conductors, they developed transistor-based devices which enabled them to cut down the size of the circuit (micro-electronics) and to integrate the circuits.

Integrated circuits, so-called because their components are built as inseparable parts of one solid chip, were first developed in 1958 by Jack Kildy at Texas Instrument Company. They are a direct outgrowth of the transistor, an example of one innovation giving birth to another. Based upon the same principles of solid-state physics as the transistor, integrated circuits depend largely upon the unique electrical properties that silicon crystals acquire when alloyed with precise amounts of materials such as boron or phosphorus (these additives are known as dopants; different dopants produce different reactions at the behest of the designer). In order to arrive at these, new scientific investigations and technological inventions had to be made in order to produce purer crystals and to investigate their scientific properties and characteristics when alloyed with different dopants.

Progress in integrated circuitry has been so rapid that now a tiny silicon piece can contain a completely integrated circuit with about 250 individual components, including about 50 transistors; dozens of integrated microcircuits can fit on a penny. Microcircuits were only 100th the size of transistorized circuits which they replaced, which in their turn were one-tenth the size of the vacuum-tube circuits which they had replaced.

Microcircuits immediately increased the reliability of electronic circuits. Since they are so tiny, several of them can be installed in a unit to take over automatically when one fails, thereby extending the life of electronic equipment. Furthermore, microcircuits are cheaper than conventional circuits: they can be produced by automation, reducing a complex set of operations into a single manufacturing operation. Besides that, they are easier to install and replace.

Bell Labs first applied transistors to their own switching problems. But any device which so improved reliability and lowered costs was soon applied in other fields. The government encouraged the development of microelectronic devices for military and space equipment, and the advances produced for those uses, especially in computers, then found their way back to the civilian economy.

The quick diffusion of the transistor is remarkable. Bell sold licenses to produce transistors to other firms, mostly those which had been engaged in the production of vacuum tubes, which the transistor had made almost obsolete. Within two years after the development of the junction transistor in 1951, some 25 firms had been licensed to manufacture it. Thus, by the time it was only five years old, the transistor had sounded the death knell of one industry and given birth to another. It was widely diffused into various other products, including hearing devices, heart pacemakers, and the ubiquitous transistor radio. Furthermore, the manufacture of transistorized devices spread worldwide, with components being produced in low-labor cost but technologically sophisticated places. The diffusion of the products was worldwide. Illiterate peasants in India and South America listen to transistorized radios; Nomadic bedouins in the Arabian desert hear their calls to prayer on their transistors; and the diffusion has even gone "out of this world" through use in the space program.

The success of the transistor and its descendents accelerated certain tendencies long apparent in the innovation process: the close ties between science and technology, and interdisciplinary research teams in industrial laboratories. Of course, the industrial research laboratory was by no means new, but the examples of the transistor emerging from Bell Labs, the pre-war development of nylon in the duPont laboratories, and the wartime Manhattan project, all helped to make the industrial research laboratory with its teams of dedicated scientists and engineers into the characteristic place for scientific and technological advance in our times. The transistor was not the work of any single individual; it was the joint work of three men, who themselves had the backing and facilities of a large industrial research laboratory. As "big science" replaced "little science," the problem of stimulating technical innovation was enlarged from providing motivation for individual scientists and engineers to the additional task of finding appropriate organizational patterns and directing the flow of information within large laboratories.

While the example of the transistor demonstrates the ecological nature of the innovation process as well as the synergistic effect of a breakthrough on both science and technology, one cautionary note must be added. Development of the transistor might not be typical of the innovative proces. It represents a rare combination of scientific discoveries and technological advancements occurring in a particularly favorable environment:

Bell Labs had ample resources in money, equipment, and personnel to link together many different aspects of the innovation process, and its management had sufficient foresight and understanding to couple needs together with scientific-technical knowledge and to define its problems properly. Not all innovations can be conducted under such favorable circumstances, and perhaps in other fields the pattern of interaction among the different phases of innovation is quite different. In other words, a spectacularly successful innovation in one technology must not mislead us into thinking that this provides a perfect model for all other such endeavors. If anything, the ecological nature of the innovative process means that there can be shifting patterns which operate with differing degrees of effectiveness in different fields at different stages of development.

VI. Conclusions -- and a Beginning

Our brief historical case studies reveal the same complexity as any ecological system. They still do not give us a generalized model covering every aspect of innovation. Indeed, they show us different kinds of people engaged in different kinds of innovative activity in different fields. The fact that 88% of mechanical inventions are taken out by individuals, whereas 97% of chemical invention patents are taken out by corporations illustrates the differences in both the technical and organizational contexts within which innovations can occur. Innovations in chemistry or electonics seem to come from organizations with a strong bent toward basic science; mechanical inventions tend to come from firms with strong engineering staffs or from individual inventors.[157] We can recognize that broad pattern, but we do not know if that pattern is necessarily fixed or if changes could be made by a different mix of scientists and engineers on research staffs, by differing sets of organizational and managerial inputs, or by changes in the many other socio-cultural-economic-political aspects of the innovation process.

Another shortcoming affecting our study is that most of our material derives from studies of successful technological innovations. There are few accounts of failures in the literature. Yet, as every researcher knows, control groups are necessary in order to prove or disprove the validity of any models or theoretical assumptions. Our concentration upon successful innovations has deprived us of information which might highlight the special conditions which make for success as compared with those which lead to failure. As the sponsors of Project Sappho have pointed out, "It is not possible to discriminate between alternative hypotheses (to explain successful innovation) unless failure in innovation is also taken into account." We therefore need studies of aborted and unsuccessful innovations in order to help us pinpoint specific factors making for success or failure; these might form the basis for testing generalizations about the innovative process in different fields of technology or different levels of industry.

Our concepts of technological innovation might also be skewed because most of our concepts have arisen from consideration either of the great mechanical inventions dating from the Industrial Revolution or of today's science-based developments arising from physics and chemistry. In either case, we have concentrated upon physical technology and, by and large, have neglected revolutionary innovations in the life sciences, including agriculture -- which might represent different ecological parameters altogether. Yet the model of the strong coupling between users and researchers in the agricultural extension service might provide helpful suggestions for improving and stimulating innovations in fields involving physical technology.

Another problem involved in coming to grips with innovation is the difficulty of measuring some of the fundamental parameters in the process. Edwin Mansfield has pointed out the difficulties in measuring inputs (e.g., measuring the amount of R&D which is really devoted to innovation rather than routine testing and quality control); measuring outputs (e.g., relating innovation to productivity increases in individual industries, the contribution of R&D to economic growth, or the amount of return which can clearly be traced to technological changes); the skewing of R&D measurements caused by the fact that much of it is governmental in nature and has concentrated on defense and space applications which might not lend themselves to ordinary economic indexes; the influence of factors outside of R&D on productivity; and the like.[159] Despite a plethora of statistics, we are not always certain that we are measuring the right thing nor that we are correlating the most meaningful variables.

Although we have an increasing degree of mathematical sophistication and computer aids with which to manipulate our growing amount of data, the measurement problem might become increasingly difficult. The reason is that economic input-output studies, the basis for much of our past measuring of the role of technological innovation, might no longer suffice in the future. Companies are now being forced, by existing legislation regarding environmental control and perhaps also by forthcoming legislation involving technology assessment (that is, measuring the human and social consequences of technological innovations before they are applied), to take into account social and human costs which have heretofore defied quantitative measuring techniques. The study of "social indicators" is still in its infancy, and decisions regarding expenditures for development and application of innovations will have to be taken without an adequate quantitative basis and with one eye on the chancy political process. Problems of measurement will thus grow more acute as these new factors enter into our complex equations -- about which we are none too sure at the present time.

There is also the possibility that the parameters might change in what Daniel Bell calls the "post-industrial society" which is coming into being.[160] In post-industrial society, changes in the character, nature, and size of knowledge will be the major source of social change. If that is so, one of the most promising fields for future innovation will come not from expansion in physical technology or even in the life sciences, but in "software" innovation. Such innovation includes, according to Bell, the rise of a new intellectual technology, the creation of systematic research through manipulation of R&D budgets, and the codification of theoretical knowledge. In turn this would involve changing management techniques, computer programs for designing new things, mathematical simulation of organizations and economics -- indeed, great innovations in the application of information storage, retrieval, and manipulation.

"Software" innovation in post-industrial society might be far different from our current concentration upon innovation in physical technology where certain physical constraints provide limitations to our potentialities, if not to the imagination of our science-fiction writers. For example, there were limits to improvements in horseshoes beyond which 19th-century technology could not go. The horse's hoof simply did not conform to the requirements of standardization, interchangeability, and precision which characterized the new industrial techniques; the hoof grew, and it grew unevenly, and in order to remain healthy required skilled individual treatment every few weeks. High-level machine technology could not alter that fact. Hence one factor in the failure of the horse as a means of transportation is in some degree the inability to transform the horse and its foot into a precise, interchangeable, and standardized component in order to fit into an advancing industrial complex based to a large degree upon such factors.

There are, however, counter-examples which demonstrate how such natural factors can be made to fit the requirements of the machine. In the case of the mechanical tomato picker, the mechanical elements presented no special problems; but the success of the innovation depended upon the "re-design" of the tomato to a hard-skinned variety which would ripen uniformly and be susceptible to mechanical picking. /5/ The tomato thus became a standardized component which lent itself to mechanical manipulation. The story of chicken "factories" also illustrates how physical organisms can be standardized and subjected to automated feeding and assembly-line methods of growing, killing, dismembering, and distribution and cooking. Colonel Sanders is really one of the great innovators of our time, whose "fast foods" innovation has revolutionized the eating habits of a whole generation of Americans, to say nothing of industrializing the farmyard!

We still have much room for innovations which will be based upon the mechanical principles of standardization and interchangeability. Nevertheless we are already beginning to see how the ability to manipulate information, which Bell claims will have so great an effect upon post-industrial society, enables us not only to standardize but also to surmount the barriers posed by such standardization. Innovations can now be "custom-ized" and tailored to individual tastes. Henry Ford, it will be recalled, is reputed to have said that the customer can have any color he wants providing it is black. Now, however, the computerized assembly line can produce different color combinations with various options to fit the needs of each individual buyer. Perhaps the future direction of innovation will be away from standardized to individualized forms, and to that end, software innovations will be necessary. But because innovation is an ecological system, software needs will inevitably induce further hardware inventions.

* * *

Whether in our current industrial society or the coming post-industrial society, innovation will still be a necessary and pervasive social activity. Despite the disorganized state of our knowledge and the fragmentary nature of the studies which exist, we actually know a great deal about the process of innovation. Most important, we have begun to recognize that the process corresponds to an ecological system. Changes in one element affect the other elements and feedback to the original changes. Far from being a sequential linear process, where things go from one step to another, each step is involved in all the others -- and not in a cause-effect relationship but a dialectical or feedback relationship. Thus, for example, problem definition and idea generation does not occur solely at the beginning stage of the innovation process; instead, problems are re-defined and new ideas generated during the other stages. Similarly, considerations for application -- especially those involving economic costs -- enter into every stage of the process. Decisions are made all along the line, and each decision affects the mutually dependent variables involved in the process. Although we treat these dynamic variables as separate process phases for analytical purposes, our analysis must always reflect the feedbacks which are inherent in any ecological system.

The ecological nature of the innovative process means that its study cannot be the province of any single scholarly discipline. Heretofore the economists have been the most active students, but the complex nature of innovation and the interrelations of technology with the structure, functioning, and values of society require that a host of disciplines must bring their expertise to bear upon the process. We need psychologists to look into the creative wellsprings of invention; sociologists to depict the patterns of organizational structure within which innovation occurs in industrial research laboratories; information specialists to trace the flow of knowledge among the phases of the process; systems theorists to comprehend the complexities of the ecological framework; management specialists to pinpoint the organizational factors involved in decisions; decision theorists to explain the decision process as it applies to technological innovation; political scientists to analyze the nature and impact of governmental policies on innovation in various fields; communications specialists to follow the patterns of diffusion; historians to follow the changing character of innovation; and,of course, scientists and engineers to acquaint us with the scientific and technical problems and the limitations and potentialities of science and technology.

In order to study the innovation process in terms of an ecological system, we must take stock of existing knowledge. We must see what we do know so that we can learn what we do not know and that we must study further.

There is no lack of serious and important writings on the innovative process -- although we lack information on some of the critical items. Attempts to stimulate innovation based upon such incomplete information have been chancy at best. We do not know what kind of innovations are likely to occur, the applications which might be made of new scientific discoveries or innovations, or how existing models actually work out in practice.

Instead, we have a "mythology" of innovation, not only in the popular views of the subject but also among scholars who have investigated selected aspects. For example, we think that putting resources into R&D -- "throwing dollars at problems" -- will necessarily result in innovation, without taking time to consider just what kind of R&D would be most effective, the optimum structuring of the R&D organization and effort, and the like. We also tend to think that if we produce more scientists and engineers that we will have more innovation -- the democratic faith in the advantages of education -- but we have not correlated the education itself with the innovation process. We have faith that basic science somehow will produce objects of utility for mankind, but we still do not have a clear idea of how this basic science can best be translated into usable products for man's welfare (or sometimes for mankind's "dis-benefit"). We do not know how, or if, innovation can be guided to meet the nation's needs in such areas as urban transportation, pollution control, and housing. Indeed, we are still uncertain about how much of our R&D effort to stimulate innovation should be the work of the federal government and how much should be done through private industry to be most fruitful in its innovations.

Technical innovations have fueled the American economy in the past and have elevated the American standard of living to its present peak. But we cannot rely upon the momentum of past technical achievements or the current mythology of innovation to continue to stimulate economic activity or to make certain that innovative activity is directed toward the social well-being of contemporary and future America. Britain's decline from industrial leadership during the latter part of the 19th and 20th century demonstrates the folly of smugness and the failure to maintain an innovative thrust in comparison with newer nations arriving on the industrial scene. 162 Today's evidence of international competition -- many times based upon the successful exploitation of American innovations by other nations -- would seem to make it imperative that we continue to innovate and, indeed, that we accelerate the pace of innovation. At the same time, the recognition that certain technical innovations have unforeseen and deleterious social and human consequences forces us to strive for newer and better ways of making and doing things. Indeed, if the prophets of gloom and doom are correct, we must innovate if only to keep our present standard of living from declining!

We cannot reach such goals without clear understanding of the innovative process itself. If we wish technology to contribute to the achievement of national goals and objectives, we must analyze the pattern of technological innovation, identify the decision-making points in the innovative process, point out those incentives which stimulate innovation as well as the screens or hurdles which hamper it, study the people who play a part in the process as well as the institutional context in which they function, distinguish among different fields and types of innovative activity, determine means for measuring the social returns from research and innovation -- indeed, investigate all aspects and facets of this manifold and significant activity of innovation.

Those who make decisions regarding federal activity in the area of technological innovation must have this kind of information if they are to make wise and productive decisions, just as industry must also be informed. We hope that this state-of-the-art project will not only tell us what is known presently about innovation but also provide basic information for assessing the validity of current theories and models, and the wisdom of their application in federal and corporative policy toward R&D. At the same time, we try to indicate what further research is necessary in order to gain knowledge which will enable us to make the wisest and most productive use of our R&D efforts, and also to direct our future efforts.

We recognize that decisions cannot always wait until all the information is available. One can never have all the knowledge nor possess the divine wisdom necessary for making infallible decisions, especially in cases where there are so many mutually dependent variables; hence action must sometimes be taken on the basis of imperfect and imprecise knowledge. Nevertheless, the search for further information must go on so that succeeding decisions may be made with a sounder foundation in fact and theory. Certainly knowledge and understanding of the innovation process is essential for the future of American society.

Footnotes

1. R. N. Solow, "Technical Change and the Aggregate Production Function," Review of Economic Statistics 39 (1957): 312.

2. Sumner Myers, "Attitude and Innovation," International Science and Technology (Oct. 1965): 91.

3. Robert D. Buzell and Robert E. M. Nourse, Product Innovation in Food Processing, 1954-1964 (Boston, 1967).

4. Myers, p. 92.

5. "A Booming Technology, A Better Environment: Can We Have Both?" Bell Telephone Magazine (May/June 1970): 25-26.

6. Chaps. 1 and 2 in Melvin Kranzberg and Carroll W. Pursell, Jr., eds., Technology in Western Civilization (New York, 1967) vol. II.

7. Change and History (N.Y., 1952), p. 45.

8. This functional, or operational, definition is that employed by the National R&D Assessment Program of the National Science Foundation. See that Program's "Summary of Program Plans," dated Nov. 1, 1972.

9. Business Cycles (N.Y., 1939), vol. I; see also Schumpeter's The Theory of Economic Development (Cambridge, Mass., 1949).

10. Man the Maker (N.Y., 1958), p. 5.

11. See James P. Baxter, Scientists Against Time (Boston, 1946), and Guy Hartcup, The Challenge of War: Scientific and Engineering Contributions to World War II (Newton Abbott, Eng., 1970).

12. H. J. Habakkuk, American and British Technology in the Nineteenth Century: The Search for Labour-Saving Inventions (Cambridge, 1962).

13. A Study of History (10 vols., N.Y. & London, 1934-54).

14. "The Role of Climate in the Economic Development of Nations," Land Economics 47 (Nov. 1971): 340.

15. For example, see John W. Oliver, History of American Technology (N.Y., 1956), pp. v, 1.

16. "Some Racial Comparisons of Inventiveness," Mankind Quarterly 9: 129; see also Gilfillan's Supplement to the Sociology of Invention (San Francisco, 1971), pp. 73-87.

17. See Joseph Needham, Science and Civilisation in China (many volumes, Cambridge, 1954ff.)

18. Lynn White, jr., "Tibet, India, and Malaya as Sources of Western Medieval Technology," American Historical Review 45 (1960): 515-26.

19. "Singles and Multiples in Scientific Discovery; a Chapter in the Sociology of Science," Proceedings of the American Philosophical Society 105 (1961): 470-86.

20. The classic exposition of the "great-man" theory of history is to be found in Thomas Carlyle, On Heroes, Hero-Worship, and the Heroic in History (London, 1840).

21. John B. Rae, "The Invention of Invention," chap. 19 in Kranzberg and Pursell, Technology in Western Civilization, vol. I.

22. Myron A. Coler, ed., Essays on Creativity in the Sciences (N.Y., 1963);

23. Hereditary Genius (N.Y., 1870).

24. J. McKay Cattell, "Families of American Men of Science," Popular Science Monthly 86 (1915): 404; Catherine M. Cox, "The Early Mental Traits of 300 Geniuses," in L. M. Terman, ed., Genetic Studies of Genius (Stanford, 1926), vol. 2.

25. The Making of a Scientist (N.Y., 1953).

26. C. W. Taylor, ed., The First Conference on the Identification of Creative Scientific Talent (Salt Lake City, 1956).

27. See H. H. Anderson, ed., Creativity and Its Cultivation (N.Y., 1959).

28. Some of this literature is analyzed in H. A. Shepard, "Major Researches in Creativity," Research Management 2 (1959): 203-20. See also Morris I. Stein and Shirley J. Heinze, Creativity and the Individual: Summaries of Selected Literature in Psychology and Psychiatry (N.Y., 1960).

29. See Carl R. Rogers, "Toward a Theory of Creativity," ETC, Review of General Semantics 2 (1964): 249; L. S. Kubie, Neurotic Distortion of the Creative Process (Lawrence, Kan., 1958).

30. Carnegie Corporation of New York Quarterly 9 (July 1961).

31. The classic expression of the role of the entrepreneur in innovation is Joseph Schumpeter, Business Cycles (N.Y., 1939); see especially Vol. I, pp. 85-86.

32. Matthew Josephson, Edison (N.Y., 1959).

33. Thomas Parke Hughes, Elmer Sperry: Inventor and Engineer (Baltimore, 1971).

34. Jack A. Morton, Organizing for Innovation (N.Y., 1971).

35. William Rodgers, Think: A Biography of the Watsons and IBM (N.Y., 1969).

36. The Building of Cultures (N.Y., 1928).

37. Abbott Payson Usher, A History of Mechanical Inventions (rev. edn., Boston, 1954).

38. Homer G. Barnett, Innovation: The Basis of Cultural Change (N.Y., 1953).

39. Benjamin Farrington, Greek Science (2 vols., Harmondsworth, 1949), vol. 2, p. 166.

40. Lynn White, jr., Medieval Technology and Social Change (Oxford, 1962), ch. 3.

41. Lynn White, jr., "The Historical Roots of our Ecologic Crisis," Science 155 (March 10, 1967): 1203-7.

42. Lynn White, jr., "Technology and Invention in the Middle Ages," Speculum 15 (1940): 141-59.

43. Robert K. Merton, "Science, Technology, and Society in Seventeenth Century England," Osiris 4, pt. 2 (1938): 360-632; reprinted in book form (N.Y., 1970).

44. Lewis S. Feuer, The Scientific Intellectual (N.Y., 1963).

45. R. H. Tawney, Religion and the Rise of Capitalism (N.Y., 1926).

46. J. B. Bury, The Idea of Progress (reprinted., N.Y., 1955); for the later period, see H. Stuart Hughes, Consciousness and Society: The Reorientation of European Social Thought, 1890-1920 (N.Y., 1958).

47. E. M. Thompson, The Making of the English Working Class (N.Y., 1964), ch. 14.

48. This resulted in President Lyndon Johnson creating a National Commission on Technology, Automation, and Economic Progress.

49. "Attitude and Innovation," International Science and Technology (Oct. 1965): 92.

50. Elting E. Morison, Men, Machines, and Modern Times (Cambridge, Mass., 1966), ch. 2.

51. "Organization and Research and Development Decision Making Within the Decentralized Firm," in The Rate and Direction of Inventive Activity: Economic and Social Factors, National Bureau of Economic Research Special Conference, Series #13 (Princeton, 1962), pp. 388-89.

52. Walter Adams and Joel B. Dirlam, "Big Steel, Invention and Innovation." Quarterly Journal of Economics (May 1966): 167-89.

53. Donald A. Schon, Technology and Change (N.Y., 1967), p. 161.

54. "Resource and Output Trends in the United States Since 1870," American Economic Review Papers: Proceedings 46 (1956): 5.

55. "Technical Change and the Aggregate Production Function," Review of Economic Statistics 39 (1957): 312.

56. (Cambridge, Mass., 1966).

57. The Coal Question (3rd edn., London, 1906), p. 101.

58. See Jacob Judd, "America's Wooden Age," Technology and Culture 15 (Jan. 1974).

59. The Theory of Wages (London, 1932); Value and Capital (Oxford 1939).

60. W. E. G. Salter, Productivity and Technical Change (Cambridge, 1960).

61. W. Fellner, "Does the Market Direct the Relative Factor-Savings Effect of Technological Change?" in The Rate and Direction of Inventive Activity.

62. This is the thesis of Habakkuk (see fn. 12); it is disputed somewhat -- and for one sector of industry -- by Peter Temin, Iron and Steel in Nineteenth-Century America: An Economic Inquiry (Cambridge, Mass., 1964), who stresses demand factors.

63. "Economic Development and the Transfer of Technology," Technology and Culture 11 (1970): 550-75.

64. W. T. Nordhaus, Invention, Growth, and Welfare (Cambridge, Mass., 1969), ch. 1.

65. See Richard R. Nelson, Merton J. Peck, and Edward D. Kalachek, Technology, Economic Growth, and Public Policy (Washington, D.C., 1967), pp. 68-72.

66. *Theory of Economic Development*, ch. 4.

67. White, *Medieval Technology and Social Change*, ch. 1.

68. For a standard account, see Eli Heckscher, *Mercantilism* (2 vols., 2nd edn., N.Y., 1955); for special case studies, see Warren Scoville, *The Persecution of the Huguenots and French Economic Development, 1680-1720* (Berkeley, 1960), and J. Clayburn La Force, "Technological Diffusion in the 18th Century: The Spanish Textile Industry," *Technology and Culture* 5 (1964): 322-43.

69. See G. N. Clark, *Science and Social Welfare in the Age of Newton* (2nd edn., Oxford, 1949); Martha Ornstein, *The Role of Scientific Societies in the Seventeenth Century* (2nd edn., Chicago, 1928).

70. Vernon Van Dyke, *Pride and Power: The Rationale of the Space Program* (Urbana, 1964).

71. Samuel I. Doctors, *The NASA Technology Transfer Program: An Evaluation of the Dissemination System* (Washington, D.C., 1971).

72. John M. Logsdon, *The Decision to Go to the Moon: Project Apollo and the National Interest* (Cambridge, Mass., 1970).

73. John G. Burke, "Bursting Boilers and the Federal Power," *Technology and Culture* 7 (1966): 1-23.

74. Arthur M. Squires, "Clean Power from Coal," *Science* 169 (August 28, 1970): 821.

75. Nathan Rosenberg, "Technology and the Environment: An Economic Exploration," *Technology and Culture* 12 (1971): 543-61.

76. William M. Capron, ed., *Technological Change in Regulated Industries* (Washington, D.C., 1971).

77. Stacy V. Jones, *The Patent Office* (N.Y., 1971).

78. See the controversy on this point among S. C. Gilfillan, Jacob Schmookler, and I. Jordan Kunik in *Technology and Culture* 1 (1960): 201-34.

79. A simple exposition of the workings of the patent system can be found in Floyd L. Vaughn, *The United States Patent System* (Norman, Okla., 1956).

80. *Invention and the Patent System*, Joint Economic Committee of the U. S. Congress, 88th Congress, 2nd session, (Washington, 1964), pp. 44-45.

81. See Vaughn, *United States Patent System*, and *Journal of the Patent Office Society* 47 (July 1965), where these questions are raised.

82. Temporary National Economic Committee, Technology and Concentration of Economic Power (Washington, D. C., 1940).

83. NAS-NRC, Publications 980-A and 980-B (Washington, D.C., 1962).

84. Journal of the Patent Office Society 47, nos. 7 and 8 (July, Aug. 1965): 417-724.

85. An Economic Review of the Patent System, Subcommittee on Patents, Senate Study No. 15, 85th Congress, 2nd Session (Washington, 1968), p. 79.

86. Harry Goldsmith, "An Olympiad of Science," Science 177 (July 7, 1972): 35.

87. John J. Beer, The Emergence of the German Dye Industry (Urbana, Ill., 1959).

88. Kendall Birr, Pioneering in Industrial Research: The Story of the General Electric Research Laboratory (Washington, D. C., 1957); Frederick A. White, American Industrial Research Laboratories (Washington, D. C., 1961).

89. The Sources of Invention (N.Y., 1958).

90. See Gilfillan-Schmookler-Kunik dispute, fn. 78 above.

91. Department of Commerce (Washington, D. C., 1967; GPO: 1967, 0-242-736).

92. "The Framework of Decision for Major Technological Innovation," in Kurt Baier and Nicholaus Rescher, eds., Values and the Future (N.Y., 1969).

93. D. S. Sanders, "Some Difficulties in Measuring Inventive Activity," in Rate and Direction of Inventive Activity (see fn. 51).

94. "Contribution of R&D to Economic Growth in the United States," Science 175 (Feb. 4., 1972): 477-85. This also includes a review of Mansfield's own major books on this subject: Industrial Research and Technological Innovation (N.Y., 1968); The Economics of Technological Change (N.Y., 1968); and Mansfield, J. Rapoport, J. Schnee, S. Wagner, and M. Hamburger, Research and Innovation in the Modern Corporation (N.Y., 1971).

95. "Research Costs and Social Returns: Hybrid Corn and Related Innovations," Journal of Political Economy 66 (1958): 419.

96. "Technological Changes: Stimuli, Constraints, Returns - Rate of Return from Industrial Research and Development," American Economic Review 55 (1965): 310.

97. "Inventive Activity: Problems of Definition and Measurement," in Rate and Direction of Inventive Activity (see fn. 51), p. 42.

98. "The Supply of Inventors and Inventions," Ibid., p. 147.

99. London, 1961.

100. D. C. Pelz and F. M. Andrews, Scientists in Organizations (N.Y., 1966).

101. The Scientist in American Industry (N. Y., 1960).

102. Melvin Kranzberg, "Comments," in Daniel L. Spencer and Alexander Woroniak, eds., The Transfer of Technology to Developing Countries (N.Y., 1967), pp. 30-31; see also Nathan Rosenberg, "Economic Development and the Transfer of Technology: Some Historical Perspectives," Technology and Culture 11 (1970): 550-75.

103. See for example, Torsten Hagerstrand, Innovation Diffusion as a Special Process (Chicago, 1967); Everett M. Rogers, Diffusion of Innovation (N.Y., 1962); Homer G. Barnett, Innovation (N.Y., 1953); Elihu Katz, Herbert Hamilton, and Morton L. Levin, "Traditions of Research on the Diffusion of Innovation," American Sociological Review 28 (1963): 237-52.

104. Rosenberg, see fn. 102.

105. V. W. Ruttan and Yujiro Hayami, "Technology Transfer in Agricultural Development," Technology and Culture 14 (1973): 119-51.

106. See Eugene S. Ferguson, "Technical Museums and International Exhibitions," Technology and Culture 6 (1965): 30-46; also Ferguson, "Expositions of Technology, 1851-1900," in Kranzberg and Pursell, Technology in Western Civilization, Vol. I, ch. 44.

107. Nathan Rosenberg, The American System of Manufactures (Edinburgh, 1969).

108. Derek Price, "Statistical Studies of Networks of Scientific Papers," Science 149 (1965): 510-15; E. Garfield, "Science Citation Index, a New Dimension in Indexing," Science 144 (1964): 649-54.

109. William Goffman and Vaun A. Newill, "Generalization of Epidemic Theory: An Application to the Transmission of Ideas," Nature 204 (1964): 225-28.

110. Thomas J. Allen and Stephen I. Cohen, "Information Flow in Research and Development Laboratories," Administrative Science Quarterly 14 (1969): 12-19.

111. Gilfillan, Invention and the Patent System, p. 91.

112. Doctors, see fn. 71.

113. An example is the 195-page report, Multinational Corporations in World Development, prepared by the Department of Economic and Social Affairs of the United Nations Secretariat for a September 1973 meeting (UN Document ST/ECA/190); it focuses on the impact of multinational corporations on the development process and international relations. A forthcoming study by Walter Goldstein, funded by the Twentieth Century Fund, is expected to deal with the technological aspects as well as the political and economic.

114. Edwin Layton, "Mirror-Image Twins: The Communities of Science and Technology in 19th Century America," Technology and Culture 12 (1971): 562-80.

115. The conference was sponsored by the National Academy of Engineering, National Science Foundation, and the Department of Commerce. Daniel C. DeSimone, ed., Education for Innovation (N.Y., 1968).

116. M. L. Clevett, Jr., "Need an Idea?" Engineer 11 (1970): 28.

117. Marshall McLuhan, The Gutenberg Galaxy (Toronto, 1962).

118. The Sources of Increased Efficiency: A Study of DuPont Rayon Plants (Cambridge, Mass., 1965).

119. "Technological Change in the Machine-Tool Industry, 1840-1910," Journal of Economic History 23 (1963): 414-46.

120. William Cooper, "Patent Activity and Technological Change," unpublished paper, Case Western Reserve University, Graduate Seminar in History of Technology, 1971.

121. "Systems -- The Past Points the Way," Consulting Engineer (Jan. 1968): 103.

122. "Economic Development and Transfer of Technology," Technology and Culture 11 (1970): 569.

123. "Technological Momentum in History: Hydrogenation in Germany, 1898-1933," Past and Present, no. 44 (1969): 106-32.

124. Homo Ludens: A Study of the Play-Element in Culture (Boston, 1955).

125. "Art, Technology, and Science: Notes on Their Historical Interaction," Technology and Culture 11 (1970): 493-549.

126. Nathan Reingold, ed., The Papers of Joseph Henry, Vol. I (Washington, D. C., 1972).

127. "The Changing Environment of Science," Science 147 (Jan. 1, 1965): 16.

128. The Weapons Acquisition Process: An Economic Analysis (Boston, 1962), pp. 27-31.

129. Strategy for R & D (N.Y., 1967).

130. "Quantum Electronics, and Surprise in the Development of Technology," Science 159 (Feb. 16, 1968): 699-703.

131. See the series of articles by Melvin Kranzberg, "The Unity of Science-Technology," American Scientist 55 (1967): 48-67; "The Disunity of Science-Technology," American Scientist 56 (1968): 21-35; "The Spectrum of Science-Technology," Journal of the Scientific Laboratories 48 (1967): 47-58.

132. See for example, Edgar Zilsel, "The Genesis of the Idea of Scientific Progress," Journal of the History of Ideas 6 (1945); Merton (fn. 43 above); and A. Rupert Hall, "Merton Revisited, or Science and Society in the Seventeenth Century," History of Science 2 (1963): 1-16.

133. For example, J. D. Bernal, Science and Industry in the Nineteenth Century (London, 1953); Milton Kerker, "Science and the Steam Engine," Technology and Culture 2 (1961): 381-90; A. E. Musson and Eric Robinson, Science and Technology in the Industrial Revolution (Toronto, 1969).

134. Layton, "Mirror-Image Twins."

135. Fundamental Research and the Universities (Paris, 1968), p. 61.

136. See fn. 130.

137. Richard H. Shryock, "American Indifference to Basic Research During the Nineteenth Century," Archives Internationales d'histoire de sicences 28 (1948): 50-65. For a counter view see Nathan Reingold, "Alexander Dallas Bache: Science and Technology in the American Idiom," Technology and Culture 11 (1970): 163-77.

138. "Japan I: On the Threshold of an Age of Big Science?" Science 163 (Jan. 2., 1970): 32.

139. Gaps in Technology: General Report (Paris, 1968), p. 17; see also Gaps in Technology: Analytical Report (Paris 1970), Book III, Ch. 2.

140. C. W. Sherwin and R. S. Eisenson, The First Interim Report on Project Hindsight: Summary (Office of the Director of Defense Research and Engineering, A. D., 642-200; June 30, 1966); Raymond S. Eisenson, Project Hindsight -- Final Report, Task I (July 1, 1967).

141. Contract NSF-C535, Volume I of this report was published Dec. 15, 1968.

142. B. Achiadelis, P. Jervis, and A. Robertson, "Project Sappho: A Study of Success and Failure in Industrial Innovations," Report to the Science Research Council (Science Policy Research Unit, University of Sussex, 1971).

143. NSF69-17 (Washington, D. C., 1969).

144. J. Langrish, W. G. Evans, M. Gibbons, and F. R. Jevons, Wealth from Knowledge (London, 1972).

145. Centre for the Study of Industrial Innovation, London, July 1971.

146. Daniel Lerner, Harmonizing Technological Developments and Social Policy in America (Philadelphia, 1970), p. 74.

147. "The Contemporary Relationship Between Science and Technology."

148. Science, Technology, and Innovation (NSF-C667; 1973).

149. "Science-Technology and Warfare; Action, Reaction, and Interaction in the Post-World War II Era, in Monte D. Wright and Lawrence J. Paszek, eds., Science, Technology, and Warfare (G.P.O., 1971), pp. 123-70.

150. Eric Robinson, "James Watt, Engineer and Man of Science," Notes and Records of the Royal Society of London 24 (1970): 224.

151. See fn. 133 above.

152. S. C. Gilfillan, Inventing the Ship (Chicago, 1935).

153. Eugene S. Ferguson, "Steam Transportation," in Kranzberg and Pursell, Technology in Western Civilization, vol. I, ch. 17.

154. Thomas M. Smith, "Late Nineteenth-Century Communications," in Ibid., vol. I, ch. 39.

155. Harold I. Sharlin, "Applications of Electricity," in Ibid., vol. I, ch. 34.

156. The best short account of the history of the transistor is Charles Weiner, "How the Transistor Emerged," IEEE Spectrum 10(1973): 24-33.

157. Nelson, Peck, and Kalachek (fn. 65), ch. [illegible].

158. Jewkes, Sawers, and Stillerman (fn. 89).

159. "Contribution of R & D to Economic Growth in the United States," Science 175 (Feb. 4, 1972): 477-86.

160. The Coming of Post-Industrial Society: A Venture in Social Forecasting (N.Y., 1973).

161. Wayne D. Rasmussen, "Advances in American Agriculture: The Mechanical Tomato Harvester as a Case Study," Technology and Culture 9 (1968): 531-43.

162. As David S. Landes puts it, "Thus the Britain of the late nineteenth century basked complacently in the sunset of economic hegemony" (The Unbound Prometheus, Cambridge, 1969, p. 336). This view of the decline of British entrepreneurship has recently been challenged; see Donald N. McCloskey, ed., Essays on a Mature Economy: Britain after 1840 (Princeton, 1971).

INVENTION - AN ASSESSMENT

Frederick A. Rossini

1. Definition and Typology of Invention

A. Scope of Coverage of this Paper

This paper considers inventive activity and its most important output, invention. Its purpose is to find out what we know at this time about invention and inventive activity, what we don't know, and what of the things that we don't know we should know.

To achieve this purpose, part one will consist of a definition of invention and a typology of invention and inventive activity. Since invention is but one part of the total innovation process, linkages between invention and the other process phases will be explored. In part two I will discuss the various contexts in which invention takes place including organizational, informational, and economic as well as technical and human. Part three will critically study the significant types of measures of invention and inventive activity. Part four deals with rewards, controls, and incentives of inventive activity. In part five I will draw general conclusions about the state of our knowledge and offer recommendations for filling gaps in that knowledge.

B. Definition of Invention: Distinction from Discovery and Subinvention

An INVENTION is a plan for a product, device, process, service, institution, or method based on novel combination of preexisting elements (scientific, technical, social, etc.) which has been and can be implemented in the real world. An invention has some potential use or satisfies some need. It is not obvious to one "skilled in the art" at the time it was made.

This definition was constructed from materials found in Schmookler (Schmookler 1966, pp. 6-10) and Kuznets (Kuznets 1962, pp. 19-24). Since what is meant by invention is so important for all that follows in this paper, some amplification of this definition might help at this point. An invention is a plan of a means to an end. The invention may serve as a means for many ends, some of them not foreseen when the invention was made. It must be novel to the extent that it is not obvious to a competent practitioner. The elements from which it is made exist, but the inventor may need to search them out. They do not need to be invented or discovered. The invention has the potential of playing a meaningful role in human social structures, e.g., the farm or the corporation or government. An invention must be in some way "better" than existing means to the same end by, say, offering better quality at the same cost, the same quality at lower cost, faster production, etc.

Kuznets insists that invention must be useful economically. But inventions are sometimes made for other than market reasons, and it may be better to include both economic and non-economic utilities in considering the usefulness of an invention.

"Not being obvious to one skilled in the art" is a means of distinguishing an invention from an improvement or subinvention. In an invention the element of novelty must be significant. This is one of the key decisions a patent examiner must make about a application to determine whether it is patentable. In many cases the distinction is relatively easy. The transistor is definitely an invention. The side handle on an electric drill is definitely an improvement. Sometimes the distinction is not easy to make. But this distinction is necessary else the most trivial improvement would be patentable (Kuznets 1962, p. 22).

Another important distinction is between invention and discovery. A discovery is new but its elements may or may not be preexisting. X-rays were unknown to scientific man before their discovery. A known substance may be discovered to be superconducting. A discovery is a piece of knowledge. It is not a plan, though it may be a component of many plans (Kuznets 1962, pp. 21-2). A discovery is knowledge without specifying any particular application for that particular knowledge. An invention may contain a discovery as one of its elements. A discovery may require an invention in order that it be made. But, as in the case of invention and subinvention, the distinction between invention and discovery is sometimes difficult to make. Is superconductivity an invention or a discovery? Obviously it's a scientific discovery, but the elements were preknown - the metal and the liquified gas. It could be a plan for making current flow through a metal without requiring an external energy source. And who knows, in the long run it may be terribly useful economically.

C. Physical and Social Inventions

Most interest has been in the area of physical inventions (called technical by Kuznets (Kuznets 1962, p.19)) though there are physical and social technologies. Physical inventions provide new products for the consumer or new devices and processes for production. The elements which make up a physical invention largely consist of knowledge about the non-human world and non-human artifacts.

Social inventions consist of "new methods of inducing human beings to compete and cooperate in the social progress (sic)" (Kuznets 1962, p.19). These include social institutions such as the Peace Corps, and means of persuading people such as advertising. Different groups and different institutional arrangements are involved in our society in social and physical inventions.

(Kuznets 1962, p. 19). Controls and incentives on physical and social invention are different, and social invention is not patentable in the U.S. Although social invention is extremely important and little studied. I will deal with it in this paper only insofar as it affects physical invention.

D. Invention in the Public and Private Sectors

Private sector invention is done by individuals and firms basically for economic profit. Most of the literature about invention deals with private sector invention and assumes implicitely that the private sector is the context in which invention takes place. However, governments, both liberal capitalist and socialist, have become heavily involved in funding activities in the development of various technologies. Part of this funding supports inventive activity. In the United States defense and space are the primary examples. There is a wide range of public sector participation in inventive activity. Inventions are made in government labs by civil servants. NASA annually has a fine output of inventions. They also are made in industrial labs on projects funded by government money. Moreover some privately made inventions have primary application in areas where the government has the primary interest. A historical example of the latter was invention in the field of military small arms. Public sector inventive activity has a different set of utilities than the profit oriented private sector. Although economic considerations are definitely a factor in public sector inventions, other utilities, including political and social, are involved. Presently with environmental regulation as a significant part of public policy, inventions involved in cleaning up pollution are going to be involved in the public sector and be supported by public money. This is a central example of the increasingly important role of invention in the public sector.

E. Alternatives to Invention

The development of novelty by way of invention is not the only possible response to solve a problem or meet a need. There are two other respones which have been used in the course of man's history and which should be considered at this time. (Schmookler 1966, p. 214).

The first response is to adapt the existant, i.e., to improve what is already there, and if the existant cannot be adapted to solve the problem and meet the need then the problem must go unsolved or the need unmet. This "traditional" reaction we now find absurd - at least when a physical modification is involved. However, even today when the invention required is in an area of social concern, one can still observe this reaction. Controversies about contraceptive devices, such as the birth control pill, and the nationalization of industries indicate ——————— significant this reaction still is.

Where the first alternative response has been associated with traditional western cultures, the second is usually associated with traditional eastern cultures. This response is to dissolve the problem or need. The solution is to make the problem "no problem" or the need "no need". The external world is merely a collection of appearances. To be attracted to that world and to be tied to it is an impediment to knowing reality. The exploration of "innerspace" becomes of paramount importance; the material world is a deceiving and attracting snare.

Cultures adapting these types of responces are at enormous disadvantage to the inventing cultures. (Schmookler 1966, p. 214). Historically inventing cultures have replaced violently or non-violently these types of civilizations. The demise of the Romanov dynasty in Russia and the coming of the Chinese People's Republic to Tibet are modern examples of these changes. The near extermination of native cultures in North America hits closer to home.

Before we laugh off these other responses as passe, useless, and certainly out of place in a state of art review of invention, we should consider that invention is in response to a need or opportunity. In situations of resource scarcity - and there is evidence that we may be approaching such a situation now - these alternative responses may become important and appropriate in certain contexts of contemporary Western cultures. The great period of modern invention in which we still are functioning took place at a time when actual physical resources appeared to be or were treated as being unlimited. It was just a matter of getting the technology to use them. Thus while economic growth may depend on technological progess by invention, keeping our environmental system intact may at some latter date depend in part or in all on some combination of invention with these two alternative responses.

F. Linkages Between Invention and Other Process Phases

The entire process of innovation is divided into five process phases:

1. Problem Definition and Idea Generation
2. Invention

3. Research and Development
4. Application
5. Diffusion

If the innovation process were linear and unidirectional, we could make a phase diagram as is shown in Figure 1. However, as in so many other areas of the world, linearity here is non-existant.

To see the difficulty, try to pick out a starting point in the process. The obvious one is problem definition and idea generation, but the problem could have well come from research and development which was undertaken to diffuse an application into a new context. Instead of following problem definition and idea generation, invention may come directly from research and development or diffusion. As we review the possible combinations we might be tempted to think that every phase is linked to every other phase; every phase comes from and goes to every other phase, like Figure 2 which is an interesting combination of speghetti and meatballs. When we look at invention and its most likely linkages with the other process phases we can get a simpler diagram as in Figure 3. Linkages 1 and 2 in Figure 3 come from the linear view which is appropriate in contexts such as invention of tetraethyl lead by Thomas Midgley at General Motors in the 1920s. (Jewkes et al. 1969, pp. 312-14). Linkage 3 indicates that an invention can arise from the research and development processes. Examples of this are nylon, driving from Carothers basic research in polymers at duPont (Jewkes et al. 1969, pp. 275ff. Mueller 1962. pp. 334ff). and the invention of the transistor by Schockley and his associates at Bell Labs. (Jewkes et al. 1969, pp. 317ff, Nelson 1962, pp. 554ff). Linkage 4 suggests that an invention can be diffused for development in different contexts such as the helicopter (Jewkes et al. 1969, pp. 257 ff). An invention can also lead to problems, as linkage 5 indicates, and our experience with the nuclear bomb verifies. Linkage 6 indicates that in some cases a particularly straighforward invention might be its own application as we find in the development of DDT. (Jewkes et al. 1969, pp. 249f).

Even in the linear view of Figure 1 the distinction between the process definition and idea generation phase and the invention phase is somewhat hazy. At some point the think-process of generating ideas fades into the activity involved in embodying these ideas into some sort of external device or system. At some point the invention itself comes into being and passes, in terms of the linear model, into the research and development phase. However, this transition is again somewhat hazy. When is something invented? This

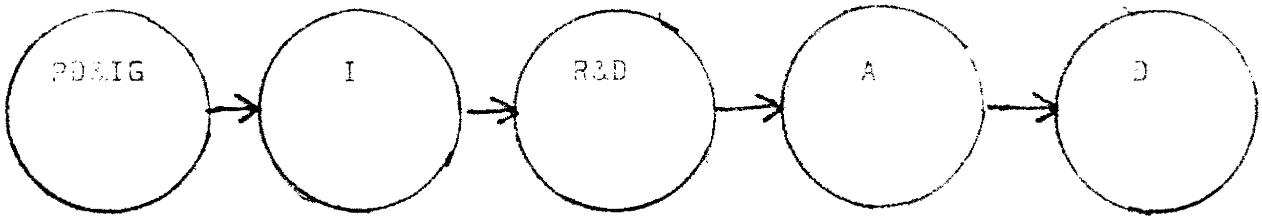


FIGURE 1

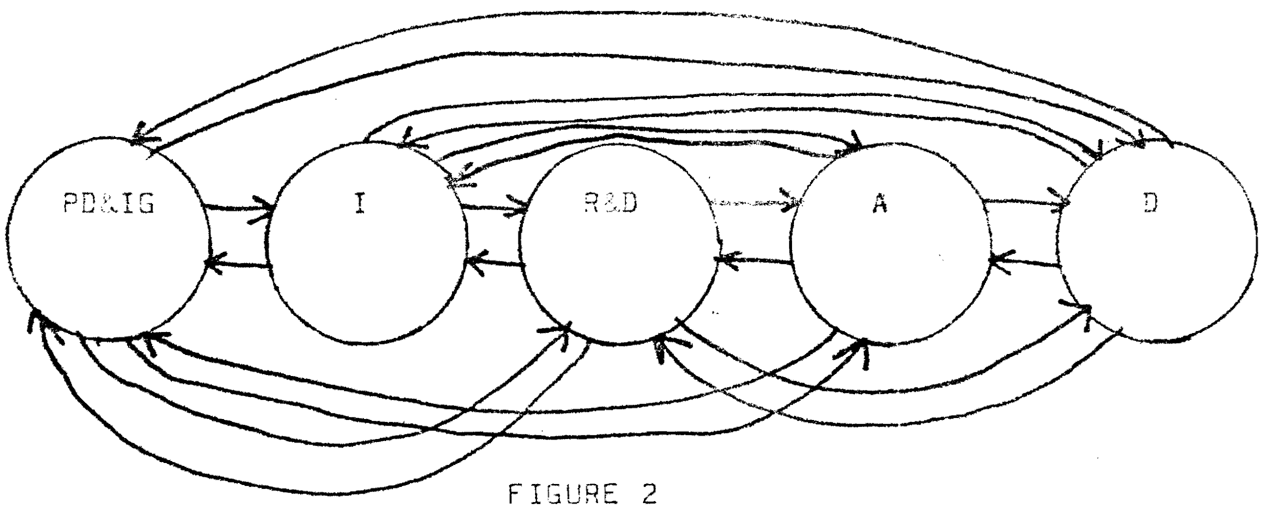


FIGURE 2

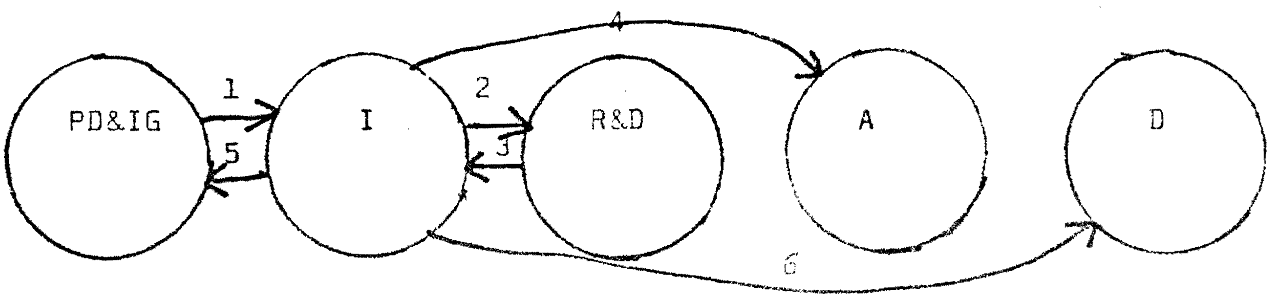


FIGURE 3

difficulty reminds one of Kuhn's (Kuhn 1970, pp. 57ff) inquiry into the exact moment that a scientific discovery was made. When did Roentgen discover X-rays? Kuhn's conclusion, which seemed quite reasonable, was that the discovery occurred sometime between the time that Roentgen first made the observation which he later interpreted as X-rays, and the time at which he had completed his observations and analyses for publication. A similiar situation exists in invention. The dividing line from both the preceding and subsequent activities is often hazy. When, for example, was nylon invented? (Jewkes et al. 1969, pp. 275ff). When Carother's assistant first noticed the fibers or when the properties of nylon were verified and a first sample in the form that it would be used was produced?

The division of the innovation process into phases allows one to focus on key aspects of the process which can be separated for purposes of analysis, if often the exact dividing lines in the historical case are somewhat uncertain.

2. Contexts of Invention and Inventive Activity

Invention and inventive activity take place in a total context which can be divided for purposes of analysis. I partition it into sections which reflect the various approaches taken in the literature and which cover, to a great degree, the whole area. The division I will make is into human and social, informational, technical, organizational, and economic contexts. These contexts are highly interactive. For example, the interaction between technological and social factors is quite significant, as when changes in transportation technologies affect the social organization of an area. Information can be treated economically, and likewise, types of organization are a function of the makeup of the economy. Thus attempts to separate the contexts of inventive activity according to some schema should be looked upon as an analytical convenience and not as a substantive statement. Research on invention necessarily focuses on certain facets of the inventive process to the exclusion of others, and organizing the material for presentation requires some attempt at classification.

A. Human and Social Contexts

Invention and inventors have existed in many different human and social contexts. For invention to be widely practiced and accepted, it must be a socially desirable or at least a socially accepted type of answer to problems, needs, and opportunities. Invention should be a social or individual value, especially a social value. To see the difference between invention as a socially accepted practice and invention as a socially frowned upon practice we could consider a situation in contemporary American society. Physical inventions of almost every sort are accepted and integrated into the economy. Their makers are awarded patents. They are usually able to develop and apply their invention without very many legal constraints. However, consider the social invention or the discovery or introduction by diffusion, if you like, of group marriage. The proponents of this invention are unable to legally develop it because it is outlawed under other statutes protecting other forms of social relations.

No patents are awarded in this area, and research and development is constantly hindered by legal constraints. Thus did the guilds of the medieval period suppressed technical invention as our society suppresses some social inventions today.

Now we want to consider the question of whether invention is exclusively the product of the individual (or research team) or whether invention is determined by existing social conditions. Whether is invention is the heroic triumph of a man of genius overcoming whatever obstacles organized society may place in his way, or whether its only determinant is the state of society, independent of any particular human talent. The former is the great man view of invention advanced by the hagiographic biographers of early inventors and engineers examplified in the Lives of the Engineers by Samuel Smiles (Hughes 1966). This work stresses the character of the engineers and their self reliance in building their works and inventions to transform the English society of the 18th and early 19th centuries: "...strong minded, resolute, and ingenious men: impelled in their special pursuits by the force of their constructive instincts". (Mackay 1905, p. 249). It was not that a social structure did not exist and that social factors worked to hinder and assist them, rather, their determination totally overcame these social factors.

Opposed to this is the contrary view of social determinism. Ogburn, for example, claims that the large number of simultaneous inventions, that is, inventions made at the same time by different persons, is an argument for showing that the conditions of the society determine the time at which a particular invention will take place and determine that it will take place. (Ogburn and Thomas, 1922). Moreover, the work of Kuznets and Merton (Kuznets 1930, Merton 1935) seems to show a decline of invention in the mature phases of an industry. Thus the exhaustion of possibilities and not the absence of great men is responsible for the decline invention in a particular industry.

The great man versus social conditions controversy has concerned itself with physical invention in the private sector of capitalistic or quasi capitalistic economies. I can find no treatments which can extend the discussions to the human and social contexts of public sector invention and social invention, in sofar as it couples with physical invention.

However, this debate does not appear to be very important. For it is clear that there are necessary conditions for an invention which exist outside of ~~the man~~ the inventor and those things which he can immediately reach, the preexisting elements of an invention. It is equally clear that the certain flash of genius or insight is necessary especially for significant inventions. Thus, it would appear obvious that some middle ground position free from extreme determinism or extreme heroics is called for.

B. Organizational Context

There is a traditional caricature of the early inventor as a lonely genius knowing little of science and working in isolation with the most primitive equipment to embody his brilliant insight. This caricature, so goes the conventional wisdom, seems to hold up to the 20th century when the industrial research laboratory with its organized and directed research groups replaced the individual inventor and condemned him to a steady eclipse until the individual inventor is no longer a significant factor in physical invention.

This is the myth. Now we want to survey the available evidence that has a bearing on the myth. Jewkes et al. have developed case histories of important inventions from the 18th to the 20th centuries. One of the key patterns emerging from this work was that the individual inventor of the 18th and 19th centuries was not isolated, as myth claimed him to be, from scientists, other inventors, technologists, and businessmen.

For example, (Jewkes et al. 1969, pp. 40f), Watt the famous 18th century inventor, who improved the Newcomen engine by adding an external condensor.

was an acquaintance of well-known scientists while he was employed as a university instrument maker. In fact, it was from his friend Black's discovery of latent heat that he was enabled to understand why the Newcomen engine was so inefficient and to conceive the basis of his invention to improve it.

During the 19th century there were organizations which acted to bring inventors, scientists and businessmen interested in the application of technology together. (Jewkes et al. 1969, pp. 60ff). Among these were the Mechanics Institute in England and their equivalent in the United States. While these institutes were engaged in providing opportunities for additional education by workers and tradesmen, they also offered a meeting place for scientists and inventors. In the 19th century, thought of as the heyday of the individual inventor, Jewkes et al. found that inventors were in intense contact with other inventors and scientists. In fact, inventors often worked together anticipating the teamwork of the 20th century industrial research lab. (Jewkes et al. 1969, pp. 62ff). It is certainly true that with the 20th century came the industrial research labs, but the importance of the individual inventor remains to date. Houdry, the inventor of the catalytic process for cracking petroleum, Whittle, who deserves credit for the jet engine, and Farnsworth, who made many basic inventions in television are examples of 20th century independent inventors.

Patent statistics indicate that although the individual inventor is still quite important, the context of inventive activity is shifting from the individual's garage to the industrial lab. The percentage of U.S. patents issued to corporations rose from 18% in 1900 to 63% in the 1956-60 period (Schmookler 1966, p. 26). In using this evidence, however, it is important to note that patents granted to individuals represent individual inventions. On the other hand, patents granted to corporations represent the work of independent inventors who assign their inventions to a corporation, inventions of persons employed in a firm's operating departments and under contract to assign patent rights to their employers, and employees of the firm engaged in research and development (Schmookler 1966, pp. 75ff).

In analyzing statistics dealing with various classes of U.S. patents it appears that some areas of invention have a higher percentage of corporate patents than others. For example, (Jewkes et al. 1969, pp. 88f.) in "Chemical and Related Arts" corporation percentage grew from 34% in 1916 to 85% in 1955, well above the trend for all patents. In the category of "Radiant Energy, Signalling, Sound and Electricity", the percentage rose from 39% in 1916 to 72% in 1945. However, in 1945 corporations held only 49% of the patents in "internal combustion engines" and 48% of the patents in aeronautics. The tendency seems to be for firms to hold the greater proportion of patents in areas such as chemicals, electricity, and electronics where the investment in technical equipment and in developing basic scientific knowledge for invention is very high, and to hold a smaller percentage of patents in mechanical areas where the equipment costs are lower and where the individual using limited resources can make greater progress (Jewkes et al. 1969, p. 91).

Organized centers of invention such as the industrial research lab, government research lab and the contract research establishment offer an alternative to going it alone for the potential inventor. Seventy-five years ago organized industrial research was in its infancy. Now, especially in high technology industries, it is a very significant part of corporate operation. Of course, the typical industrial lab is not particulary interested in inventing. Most of its output is in development of existing products. Much of the smaller fraction of effort devoted to new products tends toward modest incremental improvement with invention usually being a by-product. Rarely, as in the case of the transistor (Nelson 1962), is a program of basic research undertaken in an industrial lab with some notion in the background of a potential output in the form of a basic invention.

The independent inventor has been looked upon as a very independent breed of cat. The organizational research lab - even one devoted to basic research as Bell Labs in solid state physics - invariably imposes constraints on its employees. (But there are constraints on being independent as well, like the persistent lack of funds.) The organized research lab almost always seeks respectable academic credentials or appropriately socialized training and experience from its candidates from employment. One must either be connected with or certified by the scientific and technical establishment. This restriction tends to rule out the self taught, the eccentric, the loners of this world who would probably not be easily socialized into the smooth workings of an industrial lab or anything else. Such men have made interesting inventive contributions. The socialization of a highly individual creative person into an organization with a very clear hierarchy and goal structure is often difficult. In this regard one can consider the difficulty of Gabriel Kron in the General Electric organization. His highly novel and brilliantly inventive scientific and technical works were never fully appreciated by the more pedestrian General Electric research management (Alger 1973). But Kron remained with General Electric for many years, apparently able to accommodate, if in a constant state of tension. How does the independent mind make its peace or truce with the organization? Possibly, and this is by way of pure conjection, the independent of today may move back and forth between his own efforts and various capacities vis-a-vis the corporation or government such as employee or independent consultant, never remaining in one position for long, finishing his work and then moving on to a newer and more interesting opportunity. We need more information on the careers of contemporary inventors. Now an independent entrepreneur spinning off the small firm carrying his ideas from his former corporate home, Now an independent consultant, when consulting work is to be had, perhaps working in a job unrelated to his inventive interests to keep body and soul together until something breaks. The career profile of the contemporary inventor should prove to be a worthwhile study.

To turn now from the independent inventor and his altered role to the crucially developing organizational context of the large orgainzed research laboratory we can raise two questions modified from Jewkes et al. (Jewkes et al. 1969, p. 105):

1. Is the growing practice of combining research and manufacturing within a manufacturing corporation one which is likely to contribute greatly to the flow of invention?

2. Are there reasons to believe that very large firms or firms in a monopoly position will be more able and willing to exploit the benefits of inventive research than smaller firms or firms operating in a competitive industry?

To these questions we will add two more of our own applying to other organized contexts of invention.

3. Does the research contract firm that "does everything," such as RAND or Stanford Research Institute have any advantages or disadvantages over the single purpose industrial research organization?

4. Does the government laboratory, where the employees are civil servants, offer a stimulating environment in which inventive activity can take place?

On one hand no firm in a profit oriented economy ever engaged in research if it did not think that such research would result in an economic return for the firm. The best organized of all the industrial research labs - Bell Labs - undertakes fundamental research programs in several scientific and technical areas, equivalent in coverage and caliber of personnel to those undertaken in the best universities. These programs are concentrated in areas which management feels are of interest to AT&T at its various operating and manufacturing subsidiaries. The research is supported because of its perceived long-term profitability to the company. Many inventions have been made at these labs, a classic case being the transistor (Nelson 1962) . At least some firms have made basic inventions in their labs, nylon by duPont being another significant case (Mueller 1962) . Of course in tough times, such as in the recent recession, firms seeking to stay in business or to remain profitable cut back in those areas not involved in immediate profitability. Fundamental research is usually among the first to go. The tendency in most situations seems for the industrial lab to support the firm's current operation by helping solve scientific and technical problems and by undertaking research to make modest, and insofar as possible, forseeable improvements on the firm's output. Except in a few cases where high level management is committed to a policy of encouraging, at least in some areas, the development of basic novelty, it would appear that the industrial research lab is merely a support facility for the firms current activity. Whatever else they are, industrial labs are not invention factories, though important inventions have indeed come out of them. Different industries spend a higher or lower proportion of their income on research. In some industries, such as food, relatively little research and invention take place. In others, which are dependent on modern technology such as electronics, inventions or at least subinventions may well be a basic element of survival of the firm.

There is some controversy as to whether monopoly (the presence of a single firm in an industry such as Aloca in basic aluminum production before World War II) or oligopoly (a few firms making up an entire industry) is a better situation for technical progess and invention than a large number of relatively smaller firms in a highly competitive industry. Galbraith has raised the point in American Capitalism (Galbroith 1952, pp. 96-8) that progress requires an element of monopoly. Now are there factors in the organization of an industry which hinder or help invention? Favoring monopoly or oligopoly as a superior pattern of industrial organization in order to facilitate invention are the requirements of a reasonable size of the operation. Small size and cut-throat competition do not usually make for significant expanse on invention (except in high technology, rapidly developing in industries). On the other hand, neither does no competition, a large bureaucratic structure and a heavy investment in present capital goods. The message seems to be that the various forms of the organization of an industry are not significant determinants of inventive activity. Both AT&T, a virtual monopoly in the phone industry, and small high technology firms in highly competitive areas invent and reap the rewards of their inventions. Likewise the opposite is true of other large and small firms where the monopoly or oligopoly nor competitive situation prove to be conducive to invention, the food industry , for example. It would appear (on superficial analysis) that inventive output is not highly correlated with organization of an industry.

The third question is interesting. However, the pattern on multi-purpose, contract research organizations has been precisely that. A contract spec ifies the output and that is usually never invention. I do not know of any studies addressed specifically to invention in the multi-purpose contract research organization.

The final question is also not a gold-mine of research effort. Only certain branches of government offer the civil servant the opportunity for inventive activity. One of the most important is NASA, whose handling of inventions by its employees is explained elsewhere in the paper.

The unsatisfactory state of knowledge here is unfortunate since the contract research organizations are diversifying their output to serve a wider range of clientelle and are now submitting proposals to government funding agencies of the same sort as universities. Perhaps the problem of the inventive civil servant should best be considered along the lines suggested later in part four.

C. Informational Context

The classic case of investigation of the informational context of invention (and through it, technical progess) has been to link invention to basic scientific discoveries. This has, at various time in various milieu, reached the level of a truism whose effect has been to provide a rationale for supporting basic scientific research on the grounds that it will lead to technical progress. A couple of recent investigations into this question seems to find opposite states of affairs. Sherwin and Isenson in Project Hindsight (Sherwin and Isenson (1967) found that there was practically no information needed from recent basic science in a number of research and development projects. However, these projects were military in character and the study took the dependence investigation back only fifteen years. Also, none of the projects dealt with fundamental invention so that this result bears only indirectly on our question. TRACES (TRACES 1968) on the other hand, used a considerably longer time period and found a large number of scientific advances underlying several important civilian inventions.

To seesaw a little more, it is clear that without knowledge of the mass-energy equivalence, nuclear energy could not be tapped by man. On the other hand, the invention of the wheel clearly preceded classical mechanics. All of which tells us that we are dealing with a highly complex relationship.

It is tempting to let the matter drop at this point after the complexity has been recognized. Indeed in many areas this is a sound procedure since much additional effort will produce few additional results. Here, however, the matter is extremely critical. We are dealing with two highly prized products of our society: scientific knowledge and technical progress. To begin to unravel the relationship at a gross level, let us look at invention before modern science.

Successful inventions before the Scientific Revolution included the stirrup, the plow (White 1962), and the mechanical clock. A most copious failure in inventing was the "philosopher's stone", which had the interesting property of being able to change base metals, (lead, etc.), into gold. Perhaps this skewed and incomplete set of cases may have a use in illustrating what differences, if any, scientific knowledge could make to an inventor.

There exists a technical knowledge base of devices, practices, etc. from which the inventor must develop his inventions. Without such a starting point he could not go immediately to his invention. In order to begin, he would have to construct some knowledge base either of principles or technique or both to make the inventions possible. When does he not need the knowledge of universal principles in addition to this practical base? When the elements of an invention are such that they can be constructed, tested, and combined without some general principles. The wheel, lever, and inclined plane are such elements. In such cases the invention does not require scientific knowledge. Now where the elements are of such complexity and such distance from the sensory and intellectual experience of the inventor that in order to deal with them he must refer to a body of knowledge of principles, then this scientific knowledge is necessary for the invention. It is a necessary, but not sufficient, condition for the invention. On this analysis, it is obvious why the "philosopher's stone" was not invented, despite so much effort toward its discovery. An adequate knowledge base would have indicated that this specific quest was hopeless. While an economic opportunity existed; lead into gold is yet today a splendid economic move if it can be done cheaply), a hopelessly inadequate scientific base led to a vain quest. A necessary scientific base exists today in the mass-energy equivalence and other results from modern physics. Schmookler got a similar result from a different analysis (Schmookler 1966, Ch 3). However, Schmookler failed to distinguish the case where technical knowledge as practiced and devised is a sufficient knowledge base (Nelson et al. 1967, pp. 8-9). In summary some knowledge is always necessary for invention. In some cases of invention, a scientific knowledge base is necessary. The distinction here is whether the necessary elements of that knowledge are immediately within reach of a man of that milieu without recourse to a knowledge base of scientific principles. Scientific knowledge is not a sufficient condition for invention - neither is knowledge - and the delays and blind alleys of inventive history after the necessary knowledge base was available argue decisively for such a conclusion.

Look now at the technical knowledge base. One form of technical knowledge is the invention itself. Important inventions analogous to basic theories and discoveries. Schmookler (Schmookler 1966, Ch. 4) has made a detailed study of four industries: railroading, petroleum refining, papermaking and agriculture. He plotted on a time scale important inventions and total patents. If the embodiment of technical knowledge in important inventions was a cause or direct stimulus to further invention, patents would then follow by some interval important inventions. Despite structuring the data in favor of this assumption, Schmookler observed no such outcome after a detailed study. It is clear that this study, thought quite a detailed study for this field, can in no way claim to be bias free as Schmookler himself admits (Schmookler 1966, pp. 80ff).

While no relationship is apparent in this macro-level study linking important inventions to subsequent inventions, the question can be put on the micro-level of the individual inventor and individual inventions. Does a preceding important invention serve as a cause or stimulus for a particular inventor to make a particular invention? An inspection of the case studies in Jewkes et al. (Jewkes et al. 1969) indicates that another invention is not a significant factor in the cases considered, though these deal mainly with important inventions. Possibly there are ways other than cognitive in which invention can influence subsequent invention. The next section on the technological context of invention will look at this possibility.

The knowledge base of the inventor appears to be his own scientific and technical capabilities, the literature, scientific and technical, and his contact with other scientists and technologists. The generation and diffusion pattern of this knowledge are interesting and important topics. However, these are most properly dealt with in Problem Definition and Idea Generation and in Diffusion. Suffice it to say that the knowledge base of invention and its development is of the utmost importance.

D. Technological Context of Invention

Does technology have its own logic so that one invention inexorably leads to another as Ellul (Ellul 1964) suggests? Putting aside economic considerations (except for demand) for this section let us look at such a possibility from the technical point of view. It seems that some inventions are necessary in order for other to be made, in that they supply necessary elements, or that while providing a knowledge base, they leave gaps which can be filled by additional inventive activity.

Consider the situation in the petroleum industry in the early part of the 20th century (Enos 1962). The automobile was becoming more and more common and demand for gasoline was increasing. Thus, economically it was desirable to increase the percentage yield of gasoline from crude petroleum. Let us look at a sequence of applications to find a technical progression.

The cracking process (i.e. breaking down complex hydrocarbons into simple hydrocarbons) by heat under pressure (invented by Dewar and Redwood in 1889) was applied by Burton and Humphreys in 1913 at Standard Oil of Indiana. However, this process was not a continuous process since the apparatus had the technical defect of a sizable down period The next family of inventions dealt with continous thermal cracking processes, that is, processes which had no down time. Four processes of this sort were developed. The inventions on which the application of these processes were based occurred around 1909. With advances in the auto industry, the need for high octane gasolines was becoming significant. This could best be met technically by a process whose output was gasoline of a high octane. Thus, a new class of processes cracking petroleum by the use of chemical catalysts came into being in the late 20s and in the 30s. The first of these was a semi-continuous process of Eugene Houdry. Later improvements and invention allowed for continuous catalytic processes. It should be clear from the preceding that we cannot rule out the economic demand as an important aspect of the development of these sequences of processes. However, there is a progressive technical sequence from the invention from batch thermal cracking to continuous thermal cracking to semi-continuous catalytic cracking to continuous catalytic cracking.

Another sort of technical stimulation for invention occurs when an imbalance within a process takes place. In the 18th century textile industry Kay's flying shuttle speeded weaving, and so more thread was needed, requiring a faster spinning process. The inventions of Hargreaves, Cartwright, and others speeded up the spinning process. But now there was an excess of yarn so Cartwright invented the power loom which could make cloth much more quickly. With the production of cloth-making mechanized to such a degree a greater supply of raw cotton was needed to keep up with the machinery. This bottleneck was solved by Whitney's invention of the cotton gin. Improvements in part of a process stimulate improvements in other parts until an equilibrium is reached. Inventions serve as stimuli for other inventions in the case of inventing around. This takes place when a firm holds the patent for a product or process that other firms want to use in their business. These firms have the alternative of obtaining a license to use the patent of of inventing around the patent by inventing a product or process which is both not covered by the original patent and which provides a patentable alternative product or process. An example of this is found in Enos (Enos 1962) where Houdry's development funds for his original catalytic cracking process came from Socony-Vacuum and Sun Oil. The other petroleum companies had the choice of licensing the Houdry process or inventing around it. A number led by Standard Oil of New Jersey invented around and within several years had developed the fluid catalytic cracking process. Inventing around patents is a common occurrence in the drug industry where slight changes in chemical composition may yield patentable drugs which allow the firm to produce competitive products without needing to license another firm's ~~else's~~ patents. I particularly remember a social conversation in the late 1960's wiht a drug firm research chemist in which he referred to inventing around as a game (and a game which he was tired of playing - he wanted to quit drug research and become a cabinet maker).

E. Economic Context of Invention

One way of looking at the economic context of an invention, especially invention of intermediate goods designed to facilitate the production process, is to consider the production function (Mansfield 1968, Ch. 2), i.e., the set of representations of efficient combinations of capital and labor which will result in a given productive output. A standard representation of a production function is drawn in Figure 4.

An invention, if it is an improvement in the economic sense (c.f. section 1 above), will make some reduction of product costs. In terms of Figure 4, after invention A production function 1 will be translated closer to the origin so that production function for an output X per year will now be production function 2.

The effect of invention can either be classed as capital saving, labor saving, or neutral. Given a constant output and the relative constancy of costs of capital and labor, an invention is capital saving if it results in a greater percentage reduction in capital input than in labor input. An invention is labor saving if it results in a greater percentage reduction in labor input than in capital input. An invention is neutral if the resulting percentages reductions in capital and labor are equal.

It has been claimed (Hicks 1932, pp. 124ff, Rothschild 1956, pp. 118ff) that the relative price of capital and labor could influence the nature of invention, i.e., could <u>induce</u> inventions which were capital or labor saving depending upon the relative prices of the factors. As the percentage of labor costs to total costs increases, as it has been observed to do, labor saving inventions will be developed.

However, Salter denies that the price of factors might influence the nature of invention (Salter 1960, pp. 43-4). Fellner (Fellner 1962) found, and Mansfield agreed (Mansfield 1962), that in a perfectly competitive economy, no forces exist to channel invention to be either capital saving or labor saving. Fellner further argued that if imperfections exist reflecting macro-scarcities in the market, than the fact that the firm can't get a particular factor indicate that it will try to save on it. This rising demand in the labor saving case for capital creates a scarcity of capital - a price rise for capital - changing the direction of invention form labor saving to capital saving. This overshoot-flyback mechanism keeps invention from being directed exclusively to saving one factor.

But the basic idea behind denying any inducement of invention by relative factor price is Salter's (Salter 1960) : "When labor costs rise, any advance that reduces the total cost is welcome, and whether this is achieved by labor saving or capital saving is irrelevant. There is no reason to assume that attention should be concentrated on labor saving techiques unless because

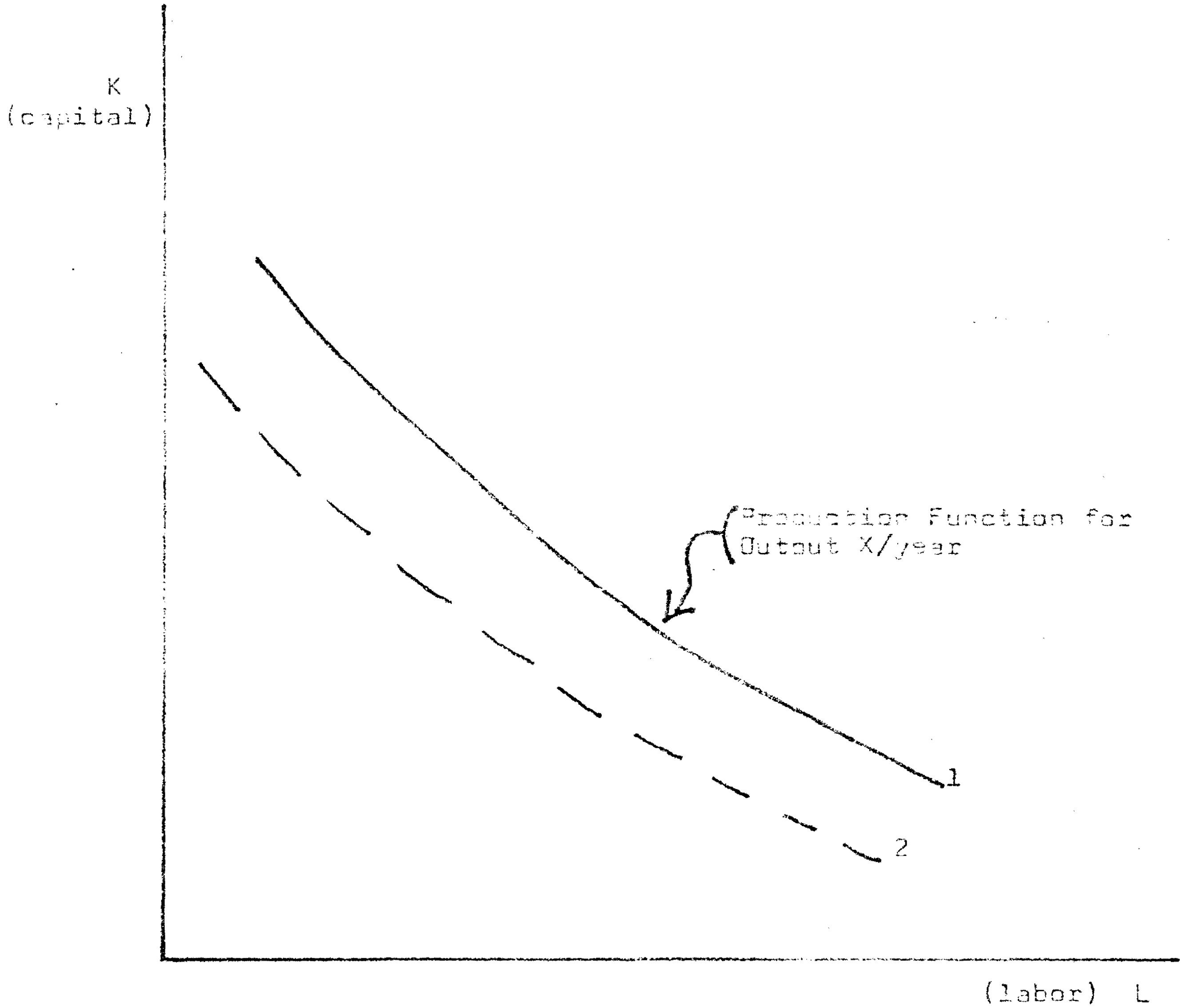


FIGURE 4

of some inherent characteristic of technology labor saving knowledge is easier to acquire than capital saving knowledge."

Kennedy (Kennedy 1965) has tried to develop a theory of induced invention by assuming that the entrepreneur knows the functional relationship between the proportion of labor and the capital saved through a given amount of innovative resources. This function is postulated to have certain mathematical properties. Ahmad (Ahmad 1966, pp. 351ff) has critized Kennedy's theory and Mansfield (Mansfield 1968, p.25. note 12) has questioned the existence and realism of Kennedy's crucial function.

Ahmad (Ahmad 1966) has attempted to develop an analytic basis for the concept of induced invention showing how it normally occurs in the inventive process. However, he assumes that the technical knowledge of the entrepreneur is neutral (in the sense discussed earlier, it is not biased toward either labor saving or capital saving invention as time passes). This assumption is questionable, to say the least, and without it in Ahmad's analysis, induced invention is not required, i.e., factor saving of either sort of neutrality is possible in any situation.

On balance it appears to me that this general line of analysis indicates the key role of knowledge in inventive activity which, though it clearly has an economic aspect, is treated in this sort of analysis only as an input to the economic process with certain specified economic properties. Now knowledge, as Salter pointed out in his earlier quote, can create a bias (i.e., electricity generation is biased toward water power and away from nuclear power when nuclear power is unknown). Precisely here is a point of intervention, but a point where (at least in our present society) is non-market in character. For the government is not a profit making enterprise and its role in subsidizing the generation of knowledge is paramount. Almost all scientific knowledge is free to the user who has the facilities to input it. The cost of generating and diffusing this knowledge is largely undertaken by non-market institutions. Technological knowledge is in part proprietary and patent protected. But enormous parts of it have entered into the free zone, and the only cost is retrieval and input facilities.

~~Now~~ Looking at the macro-level, Schmookler has argued that invention, at least capital goods invention, followed the investment in industries. Invention responds to opportunities created by demand rather than by supply. In a detailed analysis dealing with the relation of investment to capital goods invention, Schmookler (Schmookler 1966, Ch. 6) analyzed time series comparisons for the railroad capital goods . investment patterns covering over a century. His findings are that both the long term trend and the long swings in both investment and capital goods inventions exhibit great similarities with the difference that the lower turning points in major cycles occur in investment before they do in capital goods patents. He found similar results in the petroleum refining industry. Additionally, Schmookler (Schmookler 1966, ch.7) also did cross section comparisons of capital good patents and investments in over 20 industries, comparing logarithm investment in 1939 and 1947 with logarithm capital goods patents in the three years following these two dates. He obtained very high correlations.

Schmookler explains his result as follows: (Schmookler 1966, p.206)

1. Invention is largely an economic activity pursued for gain.
2. Expected gain vaies with expected sales of goods embodying the inventions.
3. Expected sales of improved capital goods are largely determined by present capital goods sales.

(Schmookler's use of patent statistics to make statements about invention will be dealt with in the section dealing with measures of inventive activity.)

From the broadest point of view, the first statement is the most important. It says that invention is part of the market process (but note at least that this point of view can ,on the basis of the data advanced,apply only to private sector for physical inventions in a technologically sophisticated market economy with some possible additional qualifications). Invention is not primarily to satisfy social or human need, to know,to perfect technical means. It is to make a buck. Thus technological change (Schmookler 1966, p.207) is an economic variable rather than a variable external to the economic system. Also it is important to note that Schmookler's analysis deal with capital goods invention in terms of the industry in which they will be used, not the industry from which they arose. Advancing scientific and technical knowledge bases in certain areas will be tailored and applied to those spots (in aggregate) where the best profit opportunities (determined by Statements 2 and 3) exist . Thus, a new metal working invention will be more likely applied to the making of automobile components than to the fabrication of buggy whip handles.

It would be interesting to apply this analysis to industries where the government largely creates the demand such as defense and space. The problem here is that,according to Schmookler (and seen in more detail later), patent statistics are best correlated with invention in the period before 1940, exactly the period before government invention became significant. If such an analysis were practical, and if the results were similar to the railroad industry, the level and the direction of government support of industries would determine the level and direction of invention for those industries just as in the private sector case analyzed by Schmookler.

Schmookler (Schmookler 1966, Ch. 9) also argued for demand as the key factor in the dissemination of consumer goods invention. Here he falls back on the consumer's preference. But to my knowledge the role of consumer preference in demand seems to be involved with advertising and the creation of demand as well as uninfluenced preferences arising from the existing state of affairs. Lacking the detailed analysis of the capital of goods case, the role of demand as expressed in consumer preference in determining consumer goods invention seems to be an open question.

There is one final point to be considered in dealing with the economic context of invention. This point deals with the costs of scientific and technical knowledge and is extremely crucial. However, it is my feeling that the economics of information best dealt with in the section dealing with problem definition and idea generation and with the diffusion. The economics of knowledge is a topic which cries for more work because in our contemporary situation has become an extremely crucial input.

3. Measures of Inventive Activity

A. Input Measures

What goes into inventive activity? Supplies, facilities, but most crucially, information, and people, who embody much of the information. A measure in the traditional sense implies some scale as a comparison with what is being measured. Perhaps as Ernest Adams has suggested, we might broaden the concept of measurement to one of getting needed information. In this case, the information we need is how much inventive activity is going on. "Inventive activity" for our purposes can be defined as taking a set of ideas or a problem and making the idea work, efficiently or otherwise, or soving the problem, i.e., going from the idea to the invention (Kuznets 1962, pp. 34f, Schmookler 1962, p.44). (The problem of correlating inventive activity with the actual number of inventions is one which we will implicitly discuss later.)

One immediate possible way of measuring inventive activity by input is to consider expenditures for research and development. Here we have a number

In dollars which is obtainable. However, problems arise from trying to use research and development expenditures as an index of inventive activity. The first problem is that this measurement excludes private inventors since it is the sum of the expenses of corporate, government, and other organized non-profit endeavors. Now individual inventors are important (Jewkes et al. 1969) in invention, For they still receive a significant fraction of patents. Thus this loss of information is significant. Moreover, it is not clear that research and development is the same thing as inventive activity. Research and development includes three type of activities. First is non-mission oriented research, general scientific and technical investigations with a direction but not designed to achieve specific, conrete goals. In the industrial context, examples of non-mission oriented research are the work of Carothers at duPont (Meuller 1962) and Shockley's research on semi-conductors at Bell Labs (Nelson 1962). The second is mission oriented research in which the research activity is undertaken with a specific, concrete goal in mind. The third, development, is concerned with creating a prototype product,device, or process out of preexisting elements and improving it until it meets criteria of use. The typology which I am using is that of TRACES (TRACES 1968), modified to spell out that development takes place before as well as after invention. Other typologies of research and development are found in Kuznets (Kuznets 1962, p. 34) and Schmookler (Schmookler 1962, p. 43). This partition is chosen because it reflects nearly the totality of what can be lumped under research and development and indicates that inventive activity is only that part of development which takes place before the prototype is completed. Typically R&D expense figures are not broken down conveniently to yield by dollar or percent the cost of inventive activity. Even if they were, however, there is the problem of determining whether in fact the cost of inventive activity is a measure of inventive activity. The problem arises here because the aptitude and ability of men differ considerably. Are 5 hacks equal to an Edison? 10 hacks? 100 hacks? There is no easy answer. The salary cost of 100 unproductive hacks may well be 50 times that of a creative engineer with an high inventive aptitude. So, is 50 times the inventive activity taking place? It would not appear so.

Approaching inventive activity from the input side by research and development expenditures leads to insurmountable difficulties in creating a measure of inventive activity. For additional elaboration of difficulties involved see Sanders (Sanders 1962, pp. 53ff).

There is the possibility of trying to measure the human input to inventive activity, which of course is the most important input. All we need is a good scale for inventive talent (Kuznets 1962, pp. 31ff). Since, as indicated before, salary cost won't give an effective measure, the only way to go is to some psychological scale analogous to intelligent quotient (IQ). One problem here is that the difference between humans in inventive potential (IP) are qualitative rather than quantitative; A threshold of inventive potential is required for a particular invention. If inventor A with an IP of 100 invents device I in T hours, giving a 100 T hours of IPT (Inventive Potential-Time), it is not necessarily true that three other inventors each with an IP of 50 and each working three T hours could make the same invention (in 450 IPT). Because of the threshold difficulty the scale is not interval, and again we lack a basis of comparison. But the situation is even more complex as inventors may have different levels of capability in different field of invention. Thus, we go back to a multi-dimensional ordinal scale, that is, a collection of lists of inventors in different areas ranked according to their inventive capabilities. For example, Smith may be better than Jones in electronics. Peters may be better than Smith in chemistry, etc.

If one wants to construct a production function for invention, consider "that every act of production here may change the production function itself. All production functions assume a given state of knowledge. The purpose of input in this case, however, is to change the state of knowledge. Hence, successive acts of production may accur under unique production functions. Moreover, different projects at the same time operate under different production functions for similar reasons." (Schmookler 1962, pp. 49-50).

In summary, no input measures of inventive activity currently exist. Sanders (Sanders 1962, p.63) finds input measure the most promising of all measures of inventive activity and urges further work. His perceptive analysis is crucial to understanding difficulty with all sorts of measure input as well as others of inventive activity. Kuznets (Kuznets 1962, p. 42) encourages further work along the lines of studies in specialized human abilities, and considers it approprate to down play the importance of monetary measures in this context.

B. Output Measures

Inventive activity result in inventions, a definite output. In this section we will consider the output, dealing with all measures of output except patent statistics which we will consider as a measure of invention in the next section.

Invention has as an output technological change, but technological change is a result of the entire innovation process including application and diffusion. Invention, being only one factor of that process, has a partial role (Sanders 1962, pp. 64ff).

It does not seem that the sources of technological change can be separated sufficiently to allow us to be able to use some measure of technological change as a measure of invention.

Kuznets (Kuznets 1962, pp. 24ff) suggested that the technical potential, i.e., the effect of the invention on further technical change and on the progress of technology in general, and economic potential, i.e.. the contribution of the invention to cost reduction or to the production of new goods in the economy, may be good output measures of an invention. The aggregate of these measures for all inventions would represent the output of inventive activity. The problem is to construct ways of measuring these potentials. Intuitively, these concepts have considerable appeal. One evaluates the potential of an invention as a measure of it. Now if this potential is evaluated in time at the point of the invention, this potential looks into the future which in the ordinary course of things is unknown. To evaluate the future, economic and technical, of an invention would require some relationship between the invention at some time and state of affair in later time. Now it is intuitively obvious that social and political climate, other inventions, and many other things will have impact upon these future states giving a wide range of potential outcomes to the inventions. Moreover, not all inventions are developed, and of the number that are developed, not all are applied. The diffusion of these applications may be board or narrow. This extrapolation into the future is fraught with incredible amount of complexity. It may be possible that by reconstructing the economic and technical potentials output of past inventions, we can arrive at some greater insight into measurement of this sort. But this possibility seems rather slight in view of the difficulty of getting informalion about all relevant aspects of most important inventions, and in the cases of other inventions, getting much useful ~~invention~~ information at all.

Another alternative is to say that the invention is the invention and that this is the output measure. But here, we fall back on getting some scale as to what an invention is worth, such as Kuznet's potentials, i.e., how much is each invention worth to our society, what is theoutput of the average invention, ~~or~~ Alternatively we can ~~go~~ count the number of inventions. The counting of inventions leads us to our next section which deals with patents statistics.

C. Patent Statistics

The key to whether patent statistics are a measure of inventive activity lies in the answer to the question of how patents are related to inventions, i.e., does a patent count provide an index of a number of invention made in the field during the period of time covered by the patent count. Schmookler has made a most extensive use of patent statistics in his work on the economics of invention, and we will rely heavily on his work to develop a positive case for the use of patent statistics(Schmookler 1966, Ch. 2). If the number of patents correlates as a constant over time with ~~reflects~~ the number of inventions, then, foregoing the attempt to judge the quality of worth of each invention by assuming an average constant in time, the patent count gives some approximate measure of invention and inventive activity.

Thus, if we know the number of patents, we can proceed. Now the patent system is most protective toward the individual private inventor who needs this protection since he lacks the means to develop his invention in most cases. During this century, corporate research and development efforts and government research and development efforts have been increasing, and they have moved to dominate inventive activity, especially in science based fields such as chemistry and electronics as opposed to, say, mechanical invention where science based development is not as dominant. In the 1930s and 1940s two trends began which made patenting less attractive to corporate inventors. The first of these was a stronger anti-trust enforcement policy. Court decisions tended to restrict the freedom of patentee to the extent of requiring compulsory licensing of his invention either freely or for reasonable royalties for corporations which held a monopoly or an extremely strong competitive interest in a particular industry. Thus, far from giving a monopoly to a patentee, this pattern of judicial decision was making use of the disclosure required to obtain the patent to break up a monopoly. Besides this, the percentage of contested patents invalidated in court proceedings rose from 33% in 1925-1929 period to 65% in 1945-1949 period (Schmookler 1966, p.31). At the same time, the time lag between patent application and patent granting increased from about a year before World War I to from 3 to 4 years during World War II. The situation has not improved that much since. In addition with the increasing of government funded research and development, liens were put on much of patentable output from R&D so that the government could use freely whatever was patented. With these disadvantage, from the corporate points of view, firms began to try to live without patenting some of their invention. As it turned out, the hardship was less than expected. For by being first and commanding adequate resources to capitalize on invention, the firm had a built-in lead over its competitors who even under the patent system could still invent around its patents. Those firms to which subjected to compulsory licensing derees were the leaders in curtailing patenting (Scherer et al. 1958, pp. 124-54). Schmookler (Schmookler 1966, pp. 32-33) indicates that there is evidence that large firms concerned with possible anti-trust proceedings by the U.S. Department of Justice have also curtailed their patenting practices.

With the movement of inventions away from the empirical and toward the scientific (Stafford 1950, p. 347), the costs of inventive activity have increased, and together with the increasingly stringent patent office requirements for novelty, and the non-patenting phonomenon indicated before have altered, over time, the patent-invention correlation. The basic patent system was developed in the days of the independent inventor, with little or no corporate R&D existing. It has not been substantially altered. Although Schmookler is able to show that patent statistics reflect at least the direction of the trends; he feels that their use is most significant for the period before 1940 when non-patenting of inventions was a relatively minor phenomenon. He claims that we can learn something from the judicous use of patent statistics. Indeed, much of his long-term research program relies on this data.

Kuznets (Kuznets 1962, p. 37) points out that the difficulty with using patent statistics is the differences of magnitude of the inventions patented (recall that his output measures for this were the technical potential and economic potential of an invention). For example, the invention of the contraceptive pill is of a much greater magnitude than the invention of cigarette lighter. This difficulty could be dealt with if the average magnitude of each patented invention were constant over a long period of time. But given the difficulties mentioned earlier in determining the potentials of an invention, this constancy would appear to be an assumption. On the empirical plane, it lacks support.

Sanders (Sanders 1962, p. 69) feels that using patents as an index of inventive activity depend on :

1. The proportion of inventive activity resulting in patented inventions must remain invariant over the span of time during which patents are deemed to serve as useful index.
2. The input per average patent must remain invariant

While Kuznets worries about output, Sanders worries about input over time - both reasonable concerns. To address Sanders' first point, we discussed earlier reasons why proportion of inventive activity resulting in patented invention is decreasing, i.e., the phenomenon of non-patenting is becoming more important. To the second point, we have seen that the input required for science-based invnetions is generally higher than input required for empirically-based invention. With the proportion of science-based invention rising, the second condition is not satisfied.

Schmookler's answer (Schmookler 1962b, pp. 78ff) is to the effect that in general these two effects are not significant in the period before 1940 in which he place most reliant on patent statistics. He concedes the difficulty for the period beginning with 1940, and hence essentially eliminate the possibility of placing strong reliance on patent statistics for work dealing with the present and future.

D. Critique and Prospects of Measures of Inventive Activity

The proceeding discussion indicates that there is no single or combination of totally acceptable measures of invention or inventive activity. Given some places to look, we have found the situation in which even those who rely on such measures only use them critically and with care. Let me review the three areas I covered one by one, discussing their prospects and pitfalls.

First there are input measures. As Kuznets has pointed out the most valuable of these is some measure of the quality of human effort involved in an inventive activity. But the measurement of any sort of creativity or human ability has proven extremely difficulty (e.g., in the recent concern over cultural bias on IQ tests). The basic questions here is whether attempts to develop information in this area would prove rewarding enough to justify the expense and effort. I would venture the opinion that constructing scales, especially of an interval sort, seems almost hopeless. However, one might usefully investigate investigate certain human characteristics such as patterning and configuration making abilities to develop useful qualitative information about inventive talent and direction. This research might lead to a provisional typology. Care should be exercised here in the design of research.

Turning to output measures not related to patented statistics, for example, Kuznets' "magnitude" expressed in technical potential and economic potentials, is an output measure. The methodology of developing such output measures seems very similar to the methodology of the technology assessment of innovations. But invention is only one phase of the innovation process and there are many alternative possibilities of development and application before the invention is diffused. I feel this sort of output measure would be more appropriately studied for the later phases of the innovation process. The methodology of technology assessment is presently not in very good shape despite tremendous enthusiasm for technology assessment. The problem of knowing the futures is considerable, and indeed has no realistic solution. However preliminary effort toward the simulation of some reasonable future possibilities is being made as in The Limits of Growth (Meadows et al. 1972). I do not feel that it is worthwhile at this point to develop this sort of output measure for the invention phase of the innovation process.

The final sort of measurement is patent statistics, also an output measure. The debate over their usefulness briefly mentioned in my text is quite vigorous. No one completely believes in patent statistics, and no one wants to throw out patent statistics as totally useless. Yet, the more recent the period of time they deal with, the less useful they admittedly are. Perhaps the wisest course is to follow Kuznets' suggestion to study the total patent application (Kuznets 1962, p. 40) rather than merely counting each application as if it were one unit. For an tentative example, one might investigate the information content of patents, tracing time lags between the development of the information and the use of the information in the invention process. One might study information trends in various areas of patenting to see if average information content of patents in different fields correlates with research and development expenditures in these fields by time series comparisons. Of course these suggestions presume that a way of evaluating information content which would be satisfactory of the purposes could be established. The evaluation of information content seems, at least at first glance, to be somewhat easier than the problems we tried to face earlier.

In summary, I believe that developing measures of invention and inventive activity in the sense of uniquely determined interval scales seems inappropriate at this time. However, this does not preclude research into the areas where these measures have been traditionally sought in order to map out territories where useful knowledge can be gained, such as typologies, configurations, etc. This work may provide information which will lead to a better understanding of the level of inventive activity, even if no strictly interval scale can be provided.

4. Rewards, Incentives, and Controls of Inventive Activity

A. The United States Patent System - Traditionally the patent system has been the mechanism of rewards, incentives, and controls of inventive activity in the U.S.

The idea behind the United States patent system is to secure for an inventor a 17 year monopoly on his invention. To obtain a patent, an inventor must disclose his invention to United State Patent Office, pay a fee, and wait a few years until the Patent Office has determined whether his invention is worthy of patenting by being a sufficient advance over the existing art on which no previous patent exists. If a patent is awarded, the inventor receives his 17 year monopoly which, however, he must prepared to defend in the courts at his own cost against infringers. If he wishes, he may grant to others for a mutually agreeable compensation a license to make use of his invention during the period of its protection, or he may assign his patent to an organization or other individual.

Begun in 1790, the patent system has been unchanged in its major features since examination of applications for novelty was begun in 1836. Since then its development has consisted in a series of improvements. One of the most recent of these is the "Disclosure Document Program" (Yonaites 1973). The patent statutes appear to be written with the inventor being an individual independent of any organization. However, with the great social changes in the United States from 1790 to date, two other clasees of inventors have come increasingly into prominence. The first is the corporate inventor who functions as a corporate empolyee usually in a research and development department. Such corporate employees, upon accepting employment with the firm, agree usually at that time to some arrangement as to disposition of any invention they might make in the course of their employment with the firm. Hopefully, this arrangement will be beneficial to both parties. The most typical case is that all inventions belong to the firm, which then rewards the employee in porportion to the importance of his invention for its operation. Possibly if the invention is irrelevent to the firm, the firm may let the employee patent it himself for his own benefit.

The second class is invention in the public sector either with employees of corporations working on government R&D contracts or grants or government employees working at government facilities. The government usually has a lien on the patentable output of any effort it funds and the right to any invention of its employees. In the latter case, NASA, for example, is careful to reward its inventors by making monetary grants to them, determined by a NASA board in accordance with the perceived importance of the invention. It is my recollection that these grants may typically be of the order of a few hundred dollars, with awards for important inventions running into a few thousand dollars. It is clear as the government becomes more involved in research and development and in providing basic goods and services (defense, space, health care, etc.,) public sector invention will grow in importance.

B. Problems with the Present System and Plans for its Improvement -

It is the contention of this section that the main problems with the present system of rewards, incentives, and controls arise from the changed social, political, technological, economic, and informational conditions in United States since the patent system was first devised in 1790. While this may seem a truism, it has some interesting consequences.

As has been pointed out earlier in the paper, the patent system grow up before corporate invention, public sector invention and social invention become significant in the United States. It has remained essentially static since 1836, when the examination for novelty of patent applications began. Now when one talks about rewards, incentives, and controls of inventive activity, one is talking about individuals, profit and non-profit organizations, the government and the mechanisms which regulate these. However, one first thinks of the patent office because that's where it all begin. But to think of the problems of the present system, it appears that except for the patent office the components of present system appears only indirectly involved with inventive activity, an informal system in the sense that the mechanisms are not so expressly directed toward invention. As Markham points out (Markham 1962, pp. 595-6) "... The soundest conclusion is that in recent years no more than one-quarter to one-half of total inventive activity in the United States is governed by the profit inventive and the traditional means of public control as embodied in patent and anti-trust policy."

Now, the principal means of controlling inventive activity presently is via the principal economic input to research and development, the Federal budget. The Federal budget has become the principle mechanism regulating inventive activity, since over half the research and development funds as of 1969 (and this has not decreased in the interim) come from the Federal Government (Markham 1962, p. 601). The Federal budget as a mechanism for controls and incentives for inventive activity has not been scrutinized in great detail.

Looking now only at the patent system, Markham (Markham 1962, p. 597) stated that the justification for the present patent system rests on two premises designed to increase social and individual economic benefit by getting a flow of inventions whose social value is higher than its social cost:

1. Perspective rewards during the 17 years of patent protection will exceed research and development and disclosure costs. Perspective rewards would achieve such costs in fewer cases if the 17 year property right were not granted. Therefore, the patent system induces inventive activity that otherwise would not be undertaken. Stated differently, in the absence of patent protection, the social benefits of inventive activity often exceed the private rewards; the patent system is designed to narrow the difference.
2. Perspective gains to society in the form of new products, processes and disclosed knowledge the patent system encourages exceeds the social cost of the 17 year monopoly grant.

Markham's suggestion (Markham 1962, p. 602) for improving the present patent system is to consider the large industrial lab as the primary organizational context of invention and to divide inventions into two categories. The first is major technological break-throughs, such as catalytic cracking, the transitor, and float glass. The second category is smaller, incremental changes. Each category would recieve different protection. Major technical break-throughs would be protected for long period of time. Incremental improvement inventions for short period of time. The problem is, of course, drawing the line. But this difficulty is easier to solve than the fundamental problem with Polanyi's proposal for patent reform (Polanyi 1944). Polanyi's basic idea is that using knowledge benefits the user but that the patent system tends to restrict the use of knowledge. His solution is to have all inventors make public disclosures of their inventions which could be freely used as long as the user submits to the invention regulatory body, the information necessary to determine the social value of the invention. The inventor is then given a monetary award from public money based on the social value of his invention. The difficulty is arriving at the social value of an invention, a problem analogous to the problem of output measures mentioned earlier.

Another mechanism now widely used for regulating invention and innovative activity is government anti-trust policy. A patent convey a monopoly to its holder. As pointed out earlier in the paper, the recent tendency has been for the courts to require certain patentees to licence their patents either freely or for a reasonable royalty. This effectively cancels the monopoly grant of the patent. Thus, the patent system, by disclosure requirement has become a means of government anti-trust policy. The role of anti-monopoly action in providing control for incentives for invention requires more exploration.

Now what emergese from this brief sketch is that <u>explicitly</u> the patent system is <u>the</u> incentive control mechanism for invention. However, in the present state of affair, anti-trust is also important, and Federal budget allocations crucial for control of incentives for invention. It is to the total picture, and not only to the patent system in isolation, that any comprehensive proposal for improvement and/or reform of incentives controls of invention must address itself.

C. Evaluation of Reward, Incentives, and Control on Inventive Activity -

At this point in time, the control mechanism on the incentive for invention must be considered comprehensively, i.e., no patent system reform apart from dealing with the interface of the patent system with anti-trust policy and with the government's research and development policy. It should be clear by now that the leading control of inventive activity is the Federal budget. Although this control is indirect in that it does not influence (generally speaking) the production of specific devices, it can move inventive talent and activity toward a general area of interest. To look at inventive activity as private activity exclusively is a great error. Public sector invention, invention within the private sector for the public sector, and the mixed private-public sector situation should be an important part of any comprehensive plan. While the patent system and its implications are socially important and should not be neglected, the patent system is going to play a less and less significant role as it's basic concepts are altered by government policy in the research and anti-trust areas.

I have not yet discussed proposals for direct grants to inventors because the government <u>is</u> making direct and indirect grant to inventors (in the public and mixed public and private sectors) by ways of salary support. Direct grants to independent individual inventors would appear to require a description in general of what it is the individual hopes to invent (research and development and innovation grants should be dealt with elsewhere.) This puts the situation roughly on ~~the~~ a par~~t~~ with ~~the~~ supporting ~~by research and development funds,~~ industrial and civil service employees who invent by Salary and R&D funds.

Because of the already existing support it does not appear at this time that direct grants to inventors (as opposed to developers or innovators) are necessary.

~~Now~~ What elements should a comprehensive plan for controls and incentives of inventive activity contain?

1. National technical priorities, i.e., what areas of technology the government should fund?
2. Policy of use of invention, a plan for government, and industry, non-profit organizations, and individuals.
3. Policy of reward for invention dealing with the various classes of inventors ~~covered~~ subsidized by Federal funds and by private funds.
4. Safe-guards for the individual who chooses to invent, whether he invents as an independent individual or in some organizational context.

These elements should be intergrated into a single plan; because each impacts on the others, i.e., who can use an invention is important in determining how the reward should be given to the inventor.

I know of no effort to develop such a policy. Research appears to be required to supply a knowledge base for such development.

5. Conclusions and Recomendations

What is known about invention deals largely with physical invention in the private sector. This limitation in our knowledge is critical only if other aspects of invention are important. The bulk of this paper has shown that they are. The area of public sector invention, as indicated by the level of Federal budget expenditures for research and development, is particularly critical. The present lack of a generally instructive guide or typology of what constitutes public sector invention and the matter and expent of its importance is a key lacuna. Also, social invention (for the purpose of this study) insofar as it impacts on physical invention needs considerable clarification. How and why is the difference between the treatment of physical and social invention and inventors in our country? Do social inventions stimulate physical inventions and vice versa?

On an even more global scale, all alternatives strategies to inventing, expressed by the conservative attitude of using the existant for as long as possible and only making the minimum improvements necessary, and the traditional eastern attitude of meeting a need by making it go away, applicable in the contemporary context, faced as we are by a population boom and natural resource crisis?

Finally there are specific areas within the coverage of this essay which merit attention, such as measures and rewards of inventive activity.

In general our knowledge is knowledge of what was important in the past, what is currently considered somewhat important, and what will be less important in the future. It is past experience which structures our knowledge about invention and gives it a ranking of importance.

The recommendations that follow pinpoint important areas of invention where our knowledge could be improved. I try to be as clear as possible without spelling out particular research projects in detail. The numbering is not in rank ordering.

Areas in invention needing more detailed study:

1. Alternative Approaches to Invention - In a general way, it might be appropriate to explore the technical and social aspects of adopting alternative approaches to the solution of problems which our society is accustomed to solve by the production of novelty, i.e., invention. In our changing resources and population context these approaches may be useful in selected contexts. They are:
   a. Minimizing novelty to the least improvement possible; making as few changes as possible in the surrounding environment.
   b. Desolving the need either by focusing attention elsewhere or encouraging behavioral patterns in which the need no longer exist.

   (A need is a need in a particular context.)
2. Public Sector Invention - We need a comprehensive study setting out the structure of public sector invention and its links with the private sector, the patent system, anti-trust policy, etc., pinpointing areas for detailed study. We badly need more data in this area and the first step is to identify the area and provide a structure in terms of which we can get the data.
3. Social Invention as it Impacts Physical Invention - Social conditions favoring or hindering physical inventions are in part the

the result of social inventions. We need an exploratory study marking out social invention, its sources and treatment in our society, and how it impacts physical invention.

4. The Information Context of Invention - The generation of diffusion of technical (and scientific) knowledge used in invention is an area of great importance. Studies in the diffusion pattern with particular emphasis on the role of the government as a generator and diffuser of technical and scientific information would be useful. Also consideration of the economic aspects of scientific and technical information is important.

5. Human Qualities Needed for Invention - Carefully desinged studies of the human qualities involved in invention, especially in the combination of existing elements into novelty, are appropriate. These studies should not be directed at the production of interval measure scales, but rather an identifying types and configurations of human inventive traits.

6. Career Profiles of Contemporary Inventors - A most useful study would deal with the career profiles of private, industrial and government inventors with emphasis on the organizational context in which they function various phases of their career. Their sources and uses of technical and scientific information, and their economic relationships should also be considered.

7. Comprehensive Study of the Rewards, Controls, and Incentives of Invention - This study should include patent policy, anti-trust policy, and the Federal budget. Its goal is the development of a framework in which the contemporary situation in this area can be addressed comprehensively. This study would serve as a knowledge base to develop a coherent public policy in this area.

In general. I urge that studies should be confined to areas of contemporary importance which will continue to be at least as important in the future, and presently unimportant areas which on the basis of trends, it appears will be important in the future.

BIBLIOGRAPHY

Ahmad, S., 1966, Economic Journal, 76, 345.

Alger, P. L., 1973, "The Evolution of an Engineering Scientist", in H. H. Happ, Ed., Gabriel Kron and Systems Theory, Schenectady, N. Y., Union College Press.

Ellul, J., 1964, The Technological Society, New York, Random House.

Enos, J. L., 1962 "Invention and Innovation in the Petroleum Refining Industry", in RDIA*.

Fellner, W., 1962, "Does the Market Direct the Relative Factor-Saving Effects of Technological Progress?", in RDIA*.

Galbraith, J. K., 1952, American Capitalism: The Concept of Contervailing Power, Boston, Houghton Mifflin.

Hicks, J. R., 1932, The Theory of Wages, New York, Macmillan.

Hughes, T. P., Ed., 1966, The Lives of the Engineers: Selection from Samuel Smiles, Cambriges, Mass., M.I.T. Press.

Jewkes, J., Sawers, D., and Stillerman, R., 1969, The Sources of Invention, 2nd Ed., New York, W. W. Norton.

Kennedy, C., 1964, Economic Journal, 74, 541.

Kuhn, T. S., 1970, The Structure of Scientific Revolutions, 2nd Ed., Chicago, University of Chicago Press.

Kuznets, S., 1930, Secular Movements in Production and Prices, Boston, Houghton Mifflin.

Kuznets, S., 1962, "Inventive Activity: Problems of Definition and Measurement", in RDIA*.

Mackay, T., Ed., 1905, The Autobiography of Samuel Smiles L.L.D., London, John Murray.

Mansfield, E., 1962, "Comment on Fellner's Paper", in RDIA*.

Mansfield, E., 1968, Technological Change, New York, W. W. Norton.

Markham, J. W., 1962, "Inventive Activity: Government Controls and the Legal Environment", in RDIA*.

Morton, R. K., 1935, Quarterly Journal of Economics, 49, 454.

Meadows, D. H., Meadows, D. L., Randers, J., and Behrens, W. W., 1972, The Limits of Growth, New York, New American Library.

Mueller, W. F., 1962, "The Origins of Basic Inventions Underlying du Pont's Major Product and Process Innovations, 1920 to 1950", in RDIA*.

Nelson, R. R., 1962, "The Link Between Science and Invention: The Case of the Transistor", in RDIA*.

Nelson, R. R., Peck, M. J., and Kalachek, E. D., 1967, Technology, Economic Growth and Public Policy, Washington, D.C., The Brookings Institution.

Ogburn, W. F., and Thomas, D., 1922, Political Science Quarterly, 37, 83.

Polanyi, M., 1944, Review of Economic Studies, 11, 61.

Rothschild, K. W., 1956, The Theory of Wages, Oxford, Blackwell.

Salter, W. E. G., 1960, Productivity and Technical Change, Cambridge, Cambridge University Press.

Sanders, B. S., 1962, "Some Difficulties in Measuring Inventive Activity", in RDIA*.

Scherer, F. M. et al., 1958, Patents and the Corporation, Boston, J. J. Galvin.

Schmookler, J., 1962a, "Comment on Kuznets' Paper", in RDIA*.

Schmookler, J., 1962b, "Comment on Sanders' Paper", in RDIA*.

Schmookler, J., 1966, Invention and Economic Growth, Cambridge, Mass., Harvard University Press.

Sherwin, C. W. and Isenson, R. S., 1967, Science, 156, 1571.

Stafford, A. B., 1950, "Trends in Invention in Material Culture", Ph.D. thesis, University of Chicago, unpublished.

TRACES, 1968, Technology in Retrospect and Critical Events in Science Vol. 1, Prepared for the National Science Foundation by the Illinois Institute of Technology Research Institute (Contract NSF-C535).

White, L., 1962, Medieval Technology and Social Change, Oxford, Oxford University Press.

Yonaites, G. A., 1973, Popular Science, 203, 36.

* The Rate and Direction of Inventive Activity: Economic and Social Factors, A Report to the National Bureau of Economic Research, Princeton, N. J., Princeton Princeton University Press.

This paper is addressed to the R&D (research and development) phase of the innovation process. It is difficult for this writer to cleanly separate R&D from the other phases of the process. To an extent R&D includes all of the other phases of the innovation process--problems are defined and ideas are created, inventions are constructed, applications result, and diffusion to other organizational functions is accomplished. As this paper evolves it will become clear that there is considerable overlap between R&D and the other phases, especially with problem definition and idea generation and with application.

It is necessary, however, to partition the innovation process into phases in order to identify areas of study and to adequately report the state-of-art. Hence, somewhat artificially, an R&D phase has been identified and defined as "the developmental work undertaken to convert the invention into a usable, feasible end result." This definition of R&D apparently is consistent with the view held by many industrial R&D laboratory directors. For example, the associate director of one corporate laboratory of a large, United States firm stated: "Our function is that of a convertor. We convert ideas and materials into products and processes." (27) It is clear that a major function performed by R&D is that of a convertor.

The definition is restricted in that it focuses almost exclusively on development and conversion. The earlier stages of research which result in the "ideas and materials" for conversion are not identified in the definition. Thus, while the definition is useful for partitioning the innovation process, it is too restrictive for use in limiting the focus of this paper. In addition to the "developmental work undertaken", the paper will also address the processes by which the ideas emerge and become known within the R&D organization and by which the laboratory's output is transformed into products and processes.

With these initial points as background, it is now possible to sketch the remaining content of the paper. The first section contains basic definitions and structual concepts. The exogenous considerations are investigated in section two and are used to provide the perspective necessary prior to study of R&D within the firm. The remaining sections concetrate on R&D within the firm and deal with, respectively, sources of ideas for R&D projects, R&D project selection and resource allocation, motivation and performance in R&D, and transfer of R&D output within the firm.

I. R&D: Underlying Definitions and Concepts

A. Distinctions Based on Characteristics of the Work Performed

Numerous authors (74,101,103,132,133,158) have defined and differentiated the work performed as R&D according to the amount of technical uncertainty and to the extent that the effort is directed toward organizational objectives. In this format, terminology is typically developed as follows:

1. Basic (pure) research is ordinarily concerned only with the extension of the boundaries of knowledge, without any technical or commercial objectives in view: it seeks basic principles and relationships.
2. Applied research seeks new knowledge having specific technical and commerical applications, typically in the form of new or improved products or processes: it has a specific practical payoff in view.
3. Development begins with a model or concept which has been proven to be technically feasible, but which requires further change due to production or market needs: it attempts to reduce research findings to practice.
4. Engineering refines the knowledge and brings it to its first use or market introduction: it is the refining which leads to commercial exploitation or other practical end uses.

When this perspective is taken, the R&D process is usually conceptualized as the flow from basic research to applied research to development to engineering and, eventually, to some end item or information which is useful to the organization. Figure 1 is an expanded model which summarizes this viewpoint.

Gordon (68) has proposed that the classic distinctions between basic and applied research be abandoned and that they be replaced by more useful distinctions such as urgency and predictability. Following Gordon, urgency refers to the speed with which the results are needed by the potential user of the research results and predictability to the extent to which the steps to obtain the new knowledge are assumed knowable prior to the actual research by the persons administratively responsible for the research (68, pg. 3). Based on this distinction, Gordon is able to draw several interesting speculations regarding the administration of research and expected behaviors during research.

Recently, it has been conjectured that, rather than being utilized as an unrelated, alternative distinction, Gordon's variables could be utilized to determine if a specific R&D activity is primarily oriented toward basic or toward applied research (27). The specific role played by the independent variables, urgency and predictability, is conjectured to depend

(Figure adapted from Quinn,(131) H.B.R., pg. 70, March-April 1960)
H.B.R. = Harvard Business Review

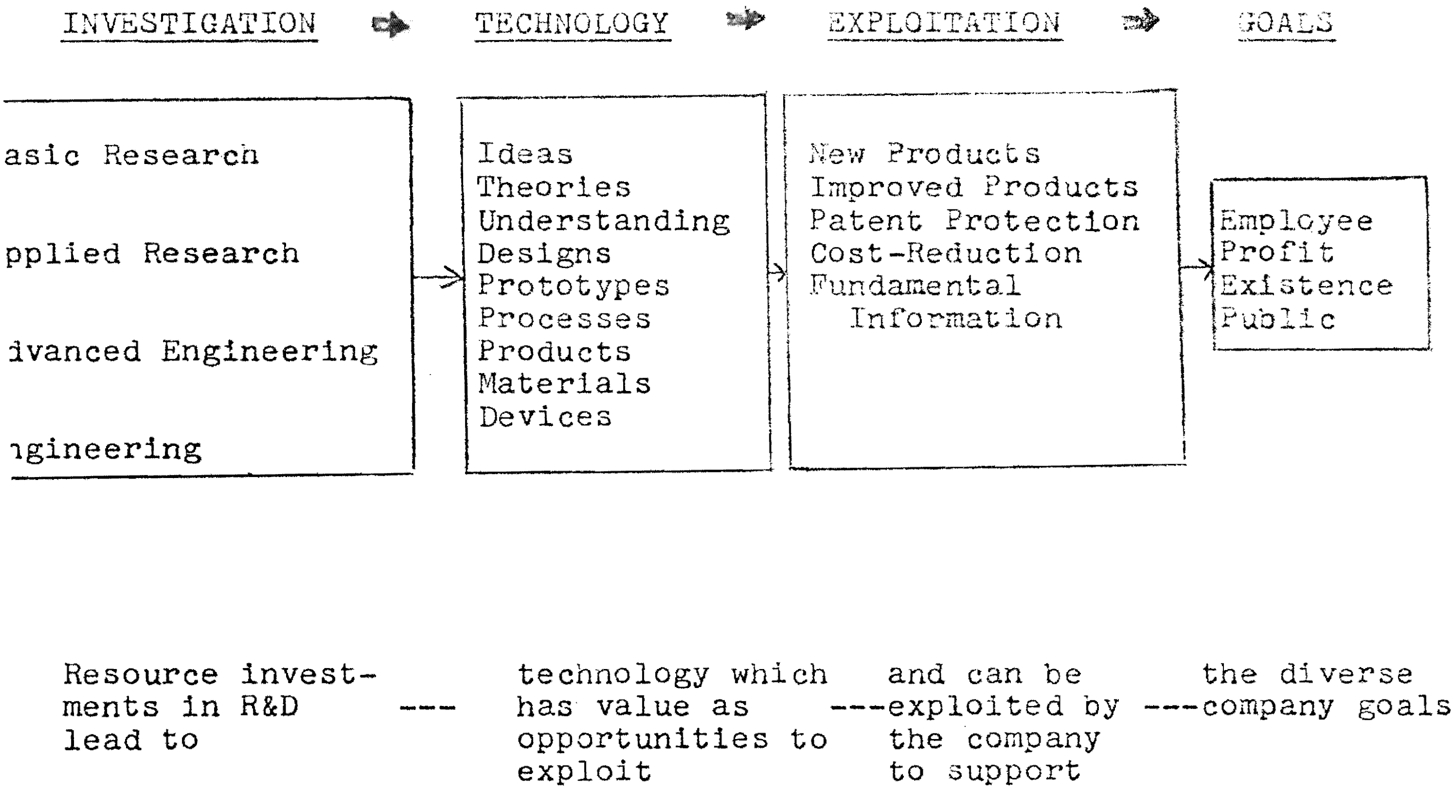


Figure 1: Work Performance Model

upon the research orientation of the laboratory in question. In a laboratory whose activities are primarily of a development or applied research orientation, technical personnel and management can be expected to rate highly those ideas which are relatively urgent and more highly predictable. Conversely, by its very nature, a laboratory with a more basic orientation should evidence evaluations and decisions which tend to favor ideas which are less urgent and less highly predictable. Thus, rather than replacing the classic distinctions, Gordon's concepts can be adapted for classification of activities and laboratories within the classic distinctions.

The notion of offensive versus defensive research and development is also common in the R&D management literature. As typically used, offensive R&D is performed in order to obtain an advantage over the competition; whereas, defensive R&D is undertaken in response to some existing or expected action by the competition. Offensive R&D, thus, is usually associated with improving the competitive situation and defensive R&D with maintaining the existing competitive situation.

In summary, there are a number of "balance" considerations which appear in the R&D literature; e.g., balance between research and development, balance between urgent/predictable and long-run/risky R&D, and balance between offensive and defensive R&D. Clearly, depending on expectations and objectives, one laboratory's balance is another laboratory's imbalance. Thus, the above definitions are useful for describing and classifying R&D and for considerations of balance. For the purposes of this paper, however, an alternative conceptual framework exists and appears to be more suitable for the purposes of structuring and analyzing the current state-of-art. Whereas the above concepts base their distinctions on characteristics of the work being performed, the alternative concept bases its distinctions on the behavior of the R&D personnel.

B. Distinctions Based on the Behavior of the R&D Personnel

A.H. Rubenstein and his colleagues in the Program of Research on the Management of R&D at Northwestern University have defined and researched a conceptualization of R&D which they have named the "idea flow process" (137,139). Drawing on Rubenstein (139), an idea is defined as "a potential proposal for undertaking new technical work which will require the commitment of significant organizational resources such as time, money, energy." The phrase "potential proposal" denotes that the idea has not been communicated to a person who has organizational authority to allocate resources (a reviewer) or who has responsibility to communicate the idea to a reviewer. A proposal is an idea which has been submitted to an organizational reviewer. A project is a proposal which has had resources allocated to it.

Figure 2 is a flow model which identifies some of the activities, linkages, and decision points which arise from consideration of how ideas are created and submitted, proposals reviewed, and projects investigated, implemented, and evaluated in R&D organizations (24). Following the primary path (solid line flow), it is clear that there are many opportunities for the R&D activity to depart from "optimal" behavior. The first two opportunities occur before R&D management has an opportunity to exert direct influence or control, namely during idea creation and submission. These two subprocesses and the impact of feedback (broken line flow) will be analyzed in detail in section III of this paper.

At the point when the idea is submitted to a reviewer, the innovator transfers authority over his idea to the R&D management. The proposal is evaluated, typically relative to other proposals existing in the R&D organization, and, if its evaluation is sufficiently high, resources are assigned to the proposal, i.e., it becomes a project. The proposal evaluation activity (25,34,39) is typically referred to as the project selection decision in the R&D literature and is reviewed in depth in section IV of this paper.

The projects are then turned over to some subset of the technical personnel, perhaps including the innovator, to be researched and developed. However, management typically maintains control and coordination responsibilities even though they have relinquished technical responsibilities. Motivation of the technical personnel and technical performance are the topics of concern in section V. The decision was made not to summarize the literature on project control and scheduling. This has been treated extensively elsewhere, and is of only marginal interest to primary reason for writing this paper.

The "idea flow" process appears to be descriptive for R&D effort whether that effort be basic or applied, urgent/predictable or long-term/risky, or offensive or defensive. For this reason, it was selected to provide the underlying flow and structure for this paper. However, before commencing the detailed analysis of the phases of the idea flow, section II will examine the exogenous considerations and provide perspective for the analyses.

Figure adapted from Baker and Freeland, Management Science, Sept. 1972. (24)

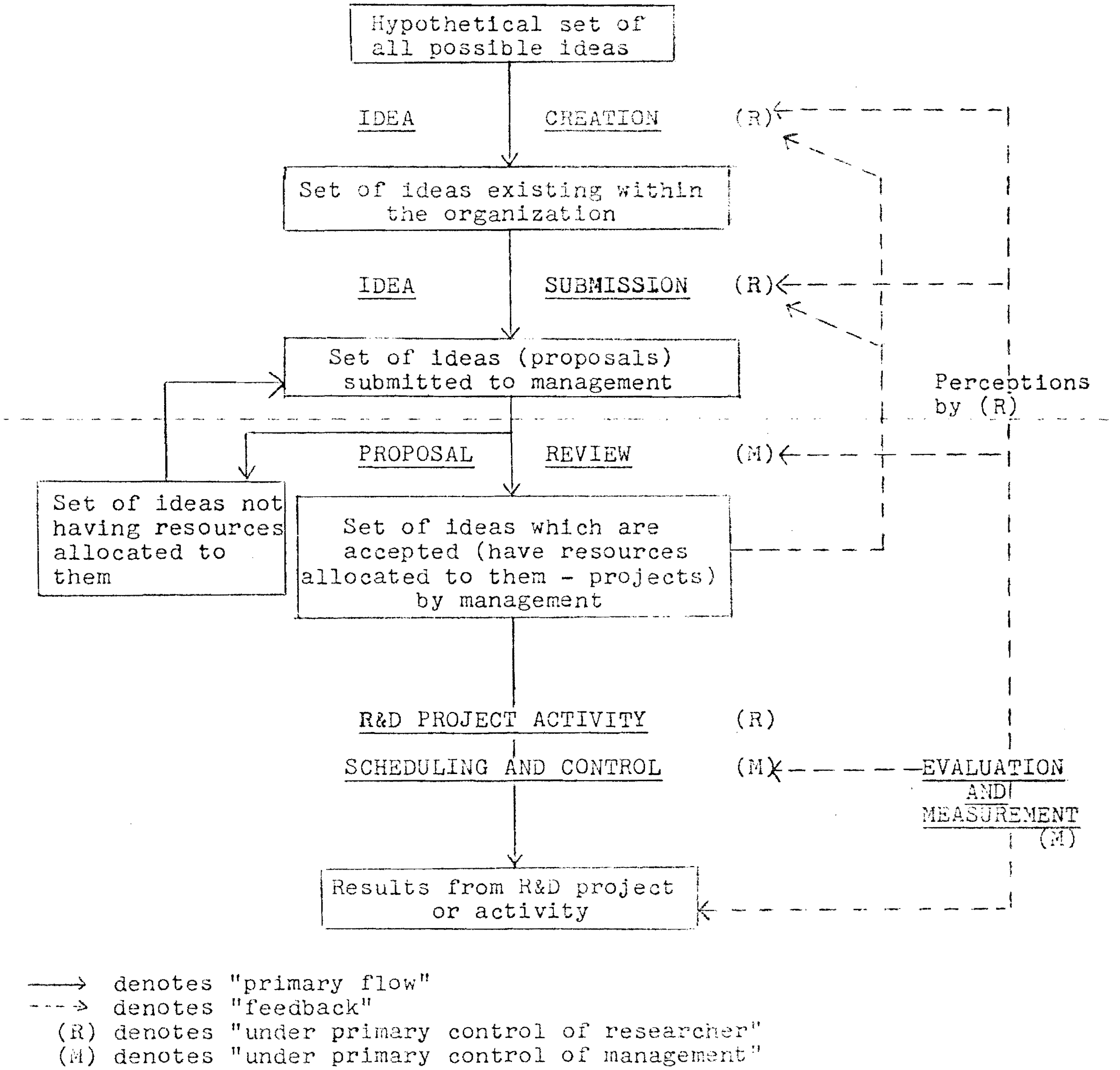


Figure 2: Behavioral Model of R&D

II. Exogenous Considerations: Impact on R&D and Contribution from R&D

The purpose of this section is to summarize the many, varied exogenous conditions, not to treat them in exhaustive depth. This statement is not meant to imply that the exogenous considerations are unimportant--indeed, they are critical. Rather, a summary is presented for two reasons: (1) the topic is treated in considerably more depth in Tarpley's paper dealing with "application" and (2) Mansfield's excellent and recent summary paper "Contribution of R&D to Economic Growth in the United States", Science, 4 February 1972, Vol. 175, No. 4021 will be included as Appendix 1 to this section (for the final draft, we must get permission from Science and from Mansfield.)

A. Some Additional Perspective

Mansfield (98) and Mansfield, et al (101) have presented and analyzed data which indicate that between 1953 and 1970, total R&D expenditures increased at an average annual rate of 10%. Rubenstein (136, pg. 95) summarized this growth differently in 1956. He noted that the 1956 edition of the Industrial Research Laboratories of the United States (136) lists 4834 R&D laboratories operated by 4,086 companies. He further stated: "A sizable proportion of these companies were not operating research programs 10 years ago, and a majority of them were not doing 15 years ago. As for the programs that did exist then, most have grown so fast that today they can hardly be recognized." (136, pg. 95). In Hamberg's words, there has been a "research explosion" and "R&D is being conducted on an unparalled scale offering the potential for unprecedented advances in productivity increases." (74).

The R&D growth in expenditures is not uniformly distributed. Mansfield (99,100,101) notes that the federal government and five industries---aircraft and missles, electrical equipment and communication, chemicals and allied products including drugs, machinery, and motor vehicles and other transportation equipment---accounted for 94% of the U.S. R&D spending in 1970 and 75% of all company-financed R&D expenditures in 1969 were concentrated in the same five industries. Given this spending distribution in 1970, it is interesting to note that in recent years, since 1965, there has been a tendency for nondefense, nonspace activities to get a greater share of federal R&D funds (99). This tendency apparently is related to a public attitude which changed in the late sixties from an attitude favorable toward science and technology to an attitude somewhat opposed to science and technology (99), especially to that related to defense and military R&D. The unfavorable attitude change apparently resulted in the tightening of federal fiscal constraints.

In 1967, Nelson, Peck, and Kalachek (121) concluded that the "invention industry" is concentrated in industrial R&D although government, university and non-profit, and private inventors do make significant contributions. Further, industrial R&D is also concentrated (1) in 400 large firms, (2) in few industries and product lines, and (3) apart from defense and space, in short-range and applied activity. Thus, the Nelson, Peck, and Kalachak literature survey suggests that R&D is concentrated in industrial R&D and that industrial R&D is iteself concentrated.

A recent book by Hamberg (74) yields considerable insight into the extent of the concentration by industry and into some of the implications of the concentration. Industrial R&D is heavily concentrated, 87%, in industries which account for a small proportion, 13%, of value added in the national economy. Further, in 1961, 60% of all industry R&D was concentrated in the aircraft and missles and the electric equipment and communication industries which are characterized as associated with missle and space programs, by notoriously expensive development costs, and by small spillover into the civilian economy (, Ch. 1). Thus, not only is industrial R&D heavily concentrated into a few industries, but it is concentrated in industries which appear to have some undesirable characteristics.

The concentration in short-range and applied activity phenomenon has also been noted by Mansfield and Hamberg. Mansfield (98,101) and Hamberg (74) state that the bulk of money spent on R&D goes for development, not research and goes toward improvements or minor changes. In studying these hypotheses in 39 firms, Mansfield (99) generated data which indicated that the bulk of the R&D projects carried out in these firms are characterized as being relatively safe from a technical point of view. The large technical advances are more likely to be sought in government-financed projects particularly in the military area. In 1970, according to estimates made by the National Science Foundation (100) about two-thirds of the total R&D expenditure went toward development and only one-third for research.

Earlier it was noted that since 1965 there has been a tendency for nondefense, nonmilitary activity to get a greater share of federal R&D funds. This does not imply that federal R&D funds have decreased in total or that the percentage of total U.S. R&D funds which come from the federal government has decreased.

Indeed, at least prior to 1965, there has been a shift to increased federal government financing of R&D, from 14% in 1930 to a 63% average in the period 1953-62 (100) and in 1970, 43% of the nation's investment in R&D was in defense and space R&D (74). As Hamberg notes, there has been no such drastic shift in the place where organized inventive activity is actually performed (74). There has been in the period 1951-1961, however, a measurable drop of approximately 10% each in the relative importance of basic research, especially in the universities, and in the relative importance of applied research, especially in industrial laboratories. Hamberg attributes this decrease at least partly to the influence of federal financing of R&D.

Based on the recent literature search underlying this paper, we apparently know very little of the realtionship, if any, between increased federal funding of R&D and decreased emphasis on basic and applied research. If, as Hamberg speculates, increased federal funding is partly responsible for the decrease and if Mansfield is correct that the large technical advances result primarily from government-financed projects in the military area and this area is currently being deemphasized, then it is important to have a more thorough investigation of this relationship.

If there is disagreement regarding causes, there is agreement that there is an increasing imbalance between basic and applied research which result in technological advance and development and engineering which result in marginal improvements and short-term payoff. The implications of this increasing imbalance are very much at the speculative stage. Some authors believe that it poses a danger to the future productivity of R&D (74,121). There is also speculation that the imbalance has a feed-forward effect because of its impact on education. The shift in the university away from basic research may have a long-term impact on the quality of our nation's R&D effort as graduate students, our future R&D technical personnel, are directed to work along conventional lines rather than encouraged to undertake new departures in basic research (74).

Chapter 2 in Hamberg's book (74) is an excellent discourse on implications for the patent system. The implications arise from two causes; namely, (1) industry typically does R&D not under the stimulus of the patent system, but under the stimulus of competition and (2) yielding patents to governmental sponsored R&D results in patent rights granted for "no risk" activity and in inherent societal costs. He offers

He offers four alternatives to the patent system:

1. abolish the patent system,
2. substitute a system of awards, but open use of the invention,
3. issue patents which have terms of protection which vary according to the risk of the R&D activity, and,
4. no patents for governmental sponsored R&D.

The implications of the research/development imbalance may be critical and systemmatic research should be undertaken to improve our understanding of them.

A number of economists have estimated that between 40 and 90 per cent of the advances in productivity have resulted from technological progress (51,74,102,148). Further, we have argued that R&D is being conducted, although somewhat in inbalance, at an unparalled scale. Given these two as premises, the U.S. should expect unprecedented advance in productivity increases. Yet, increasingly we are recognizing that the anticipated advances in productivity are not forthcoming. One explanation is that productivity increases will lag the R&D expenditure increases and that we are still in the period of the lag. In other words, be patient and the productivity increases will become evident.

While the lag hypothesis is undoubtably true, productivity increases will lag R&D expenditures, it is not the only explanation. Other possible explanations include:

1. the economists are not correct in their estimates.
2. there are factors external to the R&D organization which are counteracting the R&D expenditures.
3. the R&D activity is being mismanaged.

The remainder of this paper is addressed to summarizing and assessing the literature dealing with the above explanations.

B. Contribution of R&D to Economic Growth in The United States

Mansfield has recently published an excellent paper which has the same title as this section (100). The author and the publisher have agreed (I hope!) to permit the inclusion of Mansfield's paper as an Appendix to this document. Thus, no attempt will be made to treat this topic in length -- the interested reader is referred to Appendix 1 for Mansfield's paper. However, for the sake of completeness, the paper is summarized in this section. The reader is cautioned that what follows is a summary of Mansfield's paper and, hence, is subject to possible misinterpretation and lack of complete explanation. The reader is encouraged to read the original paper and to not be satisfied with this summary.

Mansfield (100,pg. 477) states that the primary objective of his paper is to "describe briefly what we know - or think we know - about the relationship between R&D and economic growth and productivity." He points out that by focusing on economic results he is not intending to imply that increased knowledge is unimportant nor is he implying that public policy should seek to maximize economic growth. With these cautionary statements noted, Mansfield commences his summary and assessment.

Studies by Solov (148), Abramowitz (2), and Fabricant (56) which occurred in the 1950's are cited as the pioneering studies of the relationship between technological change and economic growth (100, pg. 477). Solov concluded that about 90 per cent of the increase in output per capita during the 1909 to 1949 period was attributable to technological change. Only a minor proportion of the increase was due to increases in the amount of capital employed per worker. The early estimates of the contribution was based on a method of residues; i.e., whatever portion of output could not be explained by labor and capital inputs was attributed to technological change. In the early 1960's a number of additional studies, including a comprehensive and influential study by Denison (51), were carried out in order to refine the earlier estimates. Denison estimated that technological change was responsible for about 40% of the economic growth in the 1929-1957 period and that organized R&D contributed about 20% of the economic growth attributed to technological change (100,pg. 478.)

Mansfield (100,pg. 478) indicates that there are a number of fundamental problems associated with the manner in which the existing estimates are developed including:

1. failure to give proper credit and weight to improvements in the quality of goods and services produced,
2. failure to recognize that the returns to some input are dependent on the rate of technological change,
3. lack of a technique to translate an estimate of contribution from technological change to an estimate of contribution from R&D,
4. errors in measurement and aggregation associated with the input measures tend to inflate the residual,
5. defense and space R&D output is valued at cost and does not show up in economic growth figures directly and the indirect impacts occur only after a sizable time lag.

For the above reasons, Mansfield concludes that "at best, the available estimates are rough guidelines" (100, pg 478).

During the late 1950's important work was being conducted at the National Bureau of Economic Research which was concerned with the rate of productivity increase in various industries (100, pg. 478). The research culminated in Kendrick's book (87). As a part of this effort, Terleckyj (155) studied 1919-1953 data and concluded that an industry's rate of growth of total factor productivity was related statistically to its ratio of R&D expenditures, its rate of change of output level, and the amplitude of its cyclical fluctuations. In subsequent work, Mansfield (98) and Minasian (112) found reinforcing results which relate productivity change to the rate of growth of cumulated R&D expenditures made by the firm or industry. Brown and Conrad (36) published results in 1967 which indicated that R&D expenditures had a statistically significant effect on the rate of productivity increase and that the increase was substantially larger in durable goods industries than in non-durable goods industries.

One advantage of concentrating on an individual industry is that the effect of R&D is not derived indirectly as a residual but rather as an explicit input (100, pg. 479). However, as Mansfield points out (100, pg 479) a number of important problems remain including:

1. little is known about the characteristics of the activities that firms call R&D,
2. spurious correlations may exist; e.g., firms with high R&D expenditures may have progressive and futures-oriented management and this management may be the cause of increased productivity.
3. R&D in one industry may result in productivity increase in other industries,
4. there are a host of technical problems inherent in the economic analyses,
5. a number of the same problems list earlier for contribution to economic growth also exist in estimating productivity increases in an industry.

Despite these limitations, Mansfield (100, pg. 480) concludes "existing economic studies do provide reasonably persuasive evidence that R&D has a significant effect on the rate of productivity increase in the industries and time periods that have been studied."

Mansfield continues in his paper to summarize briefly the conclusions of a number of economists who have been concerned with the question of whether or not the R&D support that society presently gives to supplement the market mechanism is adequate in total and allocated properly (, pp. 480-482). He clearly points out the limitations of each bit of information. In total, with one exception (51), the individual conclusions seem to point in the same direction. Mansfield concludes: "In the case of those using the judgmental approach (87,121), there is considerable agreement that we may be underinvesting in particular types of R&D in the civilian sector of the economy. In the case of the econometric studies, every study of which I am aware (e.g., 72,97) indicates that the rate of return from additional R&D in the civilian sector is very high." (100, pg. 482).

Mansfield concludes his paper (100, pp. 482-485) with the identification of research needed concerning R&D, the process of technological change, and economic growth and productivity increase. These insights will be presented in the appropriate "Research Opportunities" subsections of this paper.

C. <u>Factors Influencing a Firm's R&D Activity</u>

A number of factors have been hypothesized to be related to inventive and/or R&D activity. For example, as early as 1935, Merton (110) analyzed patent statistics and concluded that inventive activity slackens in a mature industry due to exhaustion of possibilities, industrial collusion by way of a patent pool, withholding of expenditures from R&D so as to protect existing investments, and seeking of additional profit through improved management techniques rather than technological innovations. Kendrick (87) cited the short-term impact of current economic conditions especially on innovations related to cost reductions. Other authors have identified factors such as domestic and foreign competition (74,144), market structure (101), R&D expenditures as a constant ratio of sales (101), tendency to follow the leader in the industry (101), and amount of federal funding (74,101). Nelson, Peck, and Kalachek (121) concluded from their 1967 literature search that technological advance is determined by supply - e.g., stock of relevant components and materials, relevant knowledge base, and number of people possessing the relevant knowledge base - and demand - e.g., existing bottlenecks or future expectations regarding capacity, quality, price, etc. - factors.

Hamberg (74, Ch. 6) posed and investigated what is perhaps the most complex hypothesis in this area of the literature. The hypothesis includes several factors and postulates positive, an increase in the variable implies increased R&D activity, and negative, a decrease in the variable implies increased R&D activity, associations. The hypothesis is that a firm's R&D activity is influenced by: (1) current or immediate past sales with either a positive or negative association, (2) profit with either a positive or negative association, (3) liquidity position with a positive association, (4) federal government R&D contracts with either a positive or negative association, (5) gross investment in plant and equipment with a negative association, and (6) past scale of R&D with a positive association. Based on 1960 data associated with 405 firms in 21 manufacturing groups, Hamberg found: (1) even in one industry in which the federal government R&D contracts would be expected to be strongest, it had no apparent significance and in general was not significant except in a few industries; (2) the liquidity variable exerted little influence; (3) profit and sales variables are used as a guage of future profitability and operate significantly in both positive and negative associations; (4) the gross investment variable has a weak apparently negative association; and (5) the past scale of R&D has a strong positive association.

In summary of the above, a number of factors have been identified as having an influence on a firm's R&D activity. With the exception of Hamberg's study, most of the evidence is based on old data or on questionable assumptions and even Hamberg's data was collected in 1959-1960. There is a definite research need to better our understanding of the variables and relationships by statistical analysis of current data.

Size of the firm is a variable which should be conspicuous by its absence from the preceding discussion. Indeed there has been more research, speculation, and controversy about this variable than perhaps any other single issue in the R&D literature. The remainder of this subsection is devoted to a summary of the literature dealing with the size of firm variable.

Two conflicting hypotheses exist in the literature. The "bigness/fewness" hypothesis states that because of the resource requirements associated with R&D, the need to be able to withstand the risks of R&D, the benefits of diversification, and the existence of sufficient volume to profit from R&D, an industry composed of a few large firms will conduct more R&D and innovate more readily than a more competitive industry characterized by numerous comparatively small firms (62,74,85,143,159). The counter-hypothesis is that since bureaucrats in large corporations tend toward organizational security and norms and away from risk and innovation, limited numbers of firms implies limited numbers of R&D centers, the ability of new firms to enter the industry is impaired under bigness and fewness, and innovations often obsolete existing products and processes in which there may be a substantial investment, for these and other reasons it is possible that the potential capacities of the giant monopolistic firm to conduct R&D may be neutralized (74,75,83,124,142).

Mansfield (98) reported an analysis of 1945-1959 data associated with the chemical, petroleum, drug, glass, and steel industries. His findings do not, in general, support the "bigness/fewness" hypothesis. However, they strongly indicate each industry should be treated as an individual entity. The largest firms did not spend more on R&D relative to sales than did somewhat smaller firms. The chemical industry was an exception here. Where the size of the firm is held constant, the number of significant inventions carried out by a firm seems to be highly correlated to the level of R&D expenditures of that firm. In the chemical industry, increases in R&D expenditures resulted in more than proportional increases in inventive output; however, in petroleum and steel, there is no evidence of economies or diseconomies of scale within the relevant range. In most industries, the productivity of an R&D program of a given scale seems to be lower in the largest firms than in the somewhat smaller firms. Mansfield stresses the crudeness of his model regarding the number of innovations; however, the implication is that the largest firms are not making a disproportionate contribution in innovation.

Based on an analysis of the literature and of 27 major inventions during the period 1946-55, Hamberg (74, Ch. 5) concludes that "the relative share of the large industrial laboratories in minor inventions from all sources is likely to exceed the relative share of these laboratories in major inventions from all sources." He recognizes that the cumulative impact of relatively minor improvements can be substantial, but argues that the "few fundamental patents" or basic inventions maintain the broad stream of technological progress in the long run. Apparently the work of independent inventors and small- and medium-sized firms has been ignored or underestimated in our formal policies.

Mansfield, et al (101, Ch. 8) reported in 1971 a study of innovation and discovery in the ethical pharmaceutical industries. Their empirical findings included:

1. the largest firms did not carry out a disproportionately large share of the innovations.
2. the firms that contributed the most innovations, relative to their size, were not the largest firms, but somewhat smaller ones.
3. the economic impact of the innovations from the larger firms tends to be greater than from smaller firms.

In summary, then, the "bigness/fewness" hypothesis cannot be accepted for all industries. It may hold in specific industries, however. The impact of bigness appears to be mixed--the largest firms may be having a greater short-term economic impact, but less of an impact with respect to new, basic innovations. Clearly, each industry should be treated as an individual industry.

A 1973 paper by Scott (144) may result in a refocusing of the "bigness/fewness" literature and warrants detailed attention. It should be noted that his underlying assumption is that competitive pressures are still the most potent forces governing corporate evolution. Three significant hypotheses are examined:

1. the stages of corporate evolution are not small, medium, and large, but small, integrated, and diversified.
2. the companies in the diversified class can be subclassed into dominant businesses that derive 75-90% of sales from a single business or a vertically integrated chain of businesses, related business diversified into related areas, and unrelated businesses diversified such that the new businesses are not related to the old businesses.
3. hypotheses 1 and 2 above will hold for both United States and Western Europe organizations.

Scott utilizes data contained in a number of unpublished DBA dissertations conducted at the Harvard Business School during the period 1970-72 in order to examine the hypotheses.

The data analyses yield several interesting insights. There apparently is a trend in U.S. and Western Europe business toward increasing diversification and away from single or dominant businesses. The functional organization is declining and there is a dramatic rise of the form based on product divisions. The divisional structure appears to be the most effective way to manage the strategy of diversification into new profit and growth opportunities identified through their R&D capability. Thus, R&D both creates (offensive) and responds (defensive) to competitive pressure.

The following conclusions are particularly significant:

1. the conjecture that competition is a declining force in the marketplace fits many of the dominant business organizations.
2. businessmen in the highly industrialized countries of Europe and in the U.S. tend to move toward high-performance strategies and structures when competitive pressures induce them to do so.
3. public policy should work within the competitive market and reshape some of the market forces to make better use of the skill, sensitivity, and flexibility of the large corporation in its diversified, divisionalized form.

Scott's findings and conclusions suggest that it is not size of the organization or the industry within which the organization operates which are the variables important in influencing the quantity, newness, or economic impact of the firm's R&D output. Instead, his paper makes a rather persuasive argument that organizational strategy and structure are the more important variables. It is important to recognize that strategy and structure are much more under the control of the management of the firm than are size of the firm and industry. Thus, both because of the refocusing of the literature and because of the practical implications, these insights should be subjected to further systemmatic research.

D. Research Opportunities

There is apparent agreement in the literature that there is an increasing imbalance between basic and applied research which result in technological advance and development and engineering which result in marginal improvements. This imbalance apparently is occurring in the universities as well as in industry. In addition there is agreement that both total R&D and industrial R&D in the U.S. are heavily concentrated in a few industries which account for a small proportion of value added in the national economy and which can be characterized by notoriously expensive development costs. We have relatively little knowledge of what is causing the imbalance and concentration or of what the implications of the imbalance and concentration are for the future productivity and health of R&D in the U.S. There is speculation that current R&D focus impacts on graduate education and that graduate education impacts on future R&D performance. The significance of the impact and the nature of the relationships are essentially still conjectures and speculations. Much more information and work is needed to measure more accurately the "spillover" to civilian technology from government sponsored R&D (100, pg. 484). In general, there is need for better understanding of the complex interrelationships among R&D performance, imbalance and concentration in R&D, education, federal sponsorship of R&D, and the patent system.

Given that R&D is being conducted on an unprecedented scale and that economic growth and productivity increase are related to R&D activity, advances in productivity increase and economic growth should be observed at a high rate. Yet many authors have observed that the resulting economic growth and productivity increase have been disappointing. Mansfield (100, pg. 485) concludes that existing studies have not been able to estimate the contribution of R&D very accurately; however, they have certainly indicated that this contribution has been large. Among Mansfield's specific recommendations (, pp. 482-485) are the following:

1. More information is needed concerning exactly what is included in R&D in various industries.
2. Given a more detailed breakdown of R&D in various industries, it is important to disaggregate R&D in the models used to relate R&D to economic growth and productivity increase.
3. More information is needed concerning economies of scale in particular types of R&D.
4. More information is needed concerning the conditions and mechanisms leading to the application of basic science and its translation into new products and processes.
5. The measures of output on which the productivity statistics depend need to be improved and, in general, better information is needed as input to studies of productivity increase and technological change.

"Turning to the adequacy of the nation's investment in R&D, there is little evidence to support a very confident judgment as to whether or not we are underinvesting in certain types of R&D" (100, pg. 485). (Among other things Mansfield is referring to the imbalance and concentration condition in R&D which have been discussed throughout this section.) "However, practically all of the studies addressed to this question seem to conclude, with varying degrees of confidence, that we may be underinvesting in particular types of R&D in the civilian sector of the economy, and the estimated marginal rates of return from certain types of civilian R&D seem very high. Additional research is badly needed to determine more adequately the relationship of R&D to economic growth" (100, pg. 485).

A number of factors have been identified as influencing the level of R&D activity within the firm. Among the factors are industry maturity, current economic conditions, competition, market structure, current or immediate past sales, profit, "follow-the-leader" tendencies, level of technical knowledge, market demands, liquidity position, past scale of R&D, firm size, and gross investment in plant and equipment. The relationship of any of these factors to R&D activity in the firm is speculative and variable interrelationships are virtually unknown. Knowledge in this area is essential if federal policy makers are to have the ability to understand the impact of their policies prior to implementation.

The size of the firm variable has been widely discussed in the R&D literature. Two conflicting hypotheses have their support and supportors. In general, the "bigness/fewness" hypothesis cannot be accepted for all industries. It may hold in specific industries, however. The impact of bigness appears to be mixed - the largest firms may be having a greater short-term economic impact, but less of an impact with respect to new, basic innovations. Scott's (144) conjectures and findings suggest that small-medium-large is not the most useful dimension for analysis. Rather, small-integrated-diversifed may be a more insightful differentiation. The findings suggest that organizational structure and strategy are more important determinants of a firm's R&D activity than are size or industry. Because of important policy, management practice, and economic theory implications, the "bigness/fewness" and "small-integrated-diversified" relationships must be studied in depth.

III. Idea Flow in R&D

In recent years, there has been a significant increase in the amount of empirical research oriented toward the exploration of creativity, most of it being conducted by psychologists. In general, these efforts can be categorized in the following areas: (1) creativity and education, (2) description of creative people, (3) identification of creative people, (4) prediction of creative performance, (5) early experiences and creativity, and (6) creative techniques. Excellent summaries of this research area can be found in (73,125,151,154).

Several authors including Kuhn and Kaplan (90) and McPherson (107) have noted that despite the abundance of literature on the general topic of creativity, there has been little study of the environmental conditions conducive to creativity in organized R&D. Some relevant work has been reported however. These studies begin to identify organizational factors affecting creativity and to postulate relationships between these factors and creative behavior (23,30,96,147). In addition, numerous articles have been written by persons engaged in, or with the management of, industrial research (33,42,65,73,80,107).

In 1973, Utterback (156) prepared an excellent summary of the literature dealing with innovation and the diffusion of technology in industry. His bibliography includes 71 items the content of which he has summarized and integrated. A number of his conclusions are presented here to set the stage for the main topic of this section. Eight recent studies (27,38,66,91,119, 145,153,157) have indicated that R&D project ideas tend to be stimulated by either market, mission, or production needs or by technical opportunities. The largest percentage of the ideas tend to be stimulated by market, mission, or production needs, ranging from 61 to 90 per cent. Thus, it is not surprising to find that many of the ideas successfully developed and implemented by any firm came from outside the firm (91,117,119). Hence it is important to understand how information flows between and within organizations, e.g., Allen's concept of "technical gatekeepers" (5,6,7).

The literature of creativity and of idea generation and problem solving is assessed in Kelly's paper on the idea generation and problem solving phase of the innovation process. The purpose of the brief summary above is to set the background for the remainder of this section. Given that an idea exists in the organization, i.e., that an organizational participant is able to describe it, the idea typically must also become a proposal and project prior to benefit accruing to the organization. The process by which ideas flow within the R&D organization has been termed the "idea flow process" and has been extensively studied by A.H. Rubenstein and his colleagues of the Program of Research on the Management of Research and Development (POMRAD) at Northwestern University. The remainder of this section reviews the idea flow process.

A. A Model of Idea Flow in R&D*

March and Simon (102) argue that "the greater the explicit time pressure attached to an activity, the greater the propensity to engage in it." They also

*This subsection is a rewrite of pp. 3-8 of "Control Mechanisms in the R&D Idea Flow Process: Model and Behavioral Study" by J. Siegman, N.R. Baker, and A.H. Rubenstein, unpublished paper 5/69, POMRAD, Northwestern Univeristy, May 1969.

argue that "the greater the clarity of goals associated with an activity, the greater the propensity to engage in it." Recent data by Kaplan (96) and Jones and Arnold (84) tend to support these arguments and to indicate that they are applicable within an R and D environment. Specifically, their data, and other articles by research administrators and researchers, suggest:

> PROPOSITION 1: The perception of time pressures associated with the current research work encourages idea generation and development associated with the current work, but stifles idea generation and development not associated with the current work.

In their studies, Marcson (104) and Kaplan (10) both identify a commonly used technique oriented towards overcoming the idea-stifling influence of perceived time pressures. This technique can be referred to as "free time" and is often expressed in a policy statement indicating that the laboratory personnel are free to use X% (often around 10 to 20%) of their time on research of their own interest. Both authors found that effort expended on "free time" must lead to rewards for the researchers or they tend not to use it.

It may be assumed that rewards are given to a research staff by their research organization for effort expended on current work. One may further assume that at least some of these rewards are of value to the researchers (e.g., salary, security, advancement opportunities, etc.). Moreover, through such devices as progress reports and supervisor ratings, a research organization typically has a built-in operating system for reviewing achievement related to the current work. In addition, a research organization can be expected to have established policies for rewarding its research staff for such achievement, and, perhaps, for not rewarding, or even punishing them for lack of such achievement.

On the other hand, when one examines the process of idea generation and submission as a means for gaining rewards, it becomes apparent that the review mechanism is not so well defined, and reward for achievement is not so likely to occur. Further, it may be assumed that all the rewards valued by the research staff are not provided by the current work, or, that if they are provided, the researchers are not saturated with respect to all rewards. Examples of such rewards might be: recognition from peers both inside and outside the laboratory, opportunity to publish, etc.

Under these assumptions, and accepting the results of Marcson and Kaplan, the following types of rewards appear to be necessary in order for "free time" to be utilized in the generation of new ideas:

1) Rewards which are the same as those provided for effort on current work, but with which the researchers are not saturated. Because of the relative uncertainty and lack of structure, the return in rewards for effort expended on new idea generation may have to be greater than for the same effort on current work.

2) Rewards which are valued by the research staff, but which are not provided from effort expended on current work. Clearly, some consideration must be given to the relative value of the rewards and the potential cost for deciding to expend effort on idea generation not related to current work, i.e., solving assigned problems.

With respect to reward category 2, two additional points are relevant. First, the existence of "hot" or "rush" projects would tend to increase the perceived time pressures, hence the inherent risks involved in taking effort away from the current work. Second, Storer (152) and many others have suggested that there are some researchers who are rewarded primarily by being able to pursue their own research interests. If such people exist, reward category 2 might be satisfied for these researchers if they were permitted to use "free time" without reduction in the rewards they could have achieved had the "free time" effort been expended for current work. This discussion suggests:

PROPOSITION 2: In order that "free time" be used for idea generation, the associated effort must be perceived as resulting in rewards as described in category 1 and/or category 2.

In addition to supporting the general importance of attaching rewards to idea-generating effort, the Jones and Arnold data also identify "positive recognition" as an important reward. "Positive recognition of creativity and productivity from management" was ranked first out of 17 factors cited as management actions which "stimulate creative action," and recieved 48 out of 224 citations (84). Kaplan, in his questionnaire study, found that "positive and enthusiastic reception to new ideas" is a necessary reward for stimulating ideas (86). Similarly, in the research management literature, writers such as MacLaurin (95) and Williamson (163) stress prompt and positive recognition. Thus, this suggests:

PROPOSITION 3: In order for idea-generation effort to continue over time, it is indicated that previously submitted ideas be perceived as having been positively and enthusiastically received and evaluated by the organization's reviewers.

Furthermore, Houton (82) and Avery (19) argue that reviewer receptivity to previous ideas can have a trial-and-error influence on researchers' perceptions of organizational goals and needs. That researchers consider it risky to achieve these perceptions by trial and error is implied in Houton's statement that the "quickest, easiest and safest way to [perceive organizational goals and needs] is to consult someone who has already learned." In view of studies conducted within 10 industrial R and D laboratories, Avery concludes that such a technique is not only quick, easy and safe, but also common to many researchers within several different laboratories. Thus, we have:

PROPOSITION 4: The perceived reception and evaluation behavior of reviewers influences perceptions of organizational goals and needs, and,

PROPOSITION 5: Interaction with other laboratory personnel influences perceptions of organizational goals and needs.

In his discussion, Kaplan indicates that ideas must be "relevant" in order to be greeted positively and with enthusiasm (86). Apparently, organizational personnel consider an idea to be "relevant" if: they perceive it as satisfying an existing need or solving an existing problem, if it can be developed into a new project which is compatible with the organization's overall goals, and/or if it can be investigated with existing laboratory resources and facilities (20). Thus:

PROPOSITION 6: If the organizational reviewers perceive any idea to be relevant, they are more likely to receive the idea positively and enthusiastically than if they perceive it to be non-relevant.

The perceived relevance of an idea should also influence researcher behavior. A number of studies suggest that subordinates purposely screen their communications with those organizational members who control their rewards (46). Observations by Morris (115) and findings by Kornhauser (89) indicate that this finding is also valid within the industrial research environment. Therefore:

PROPOSITION 7: Prior to submission, research personnel tend to screen their ideas according to the relevance of each idea.

We have argued that perceptions of organizational goals and needs play an important role in determining which ideas are perceived as rewarding (perceptions held by management) and which ideas are submitted (perceptions held by the research staff). There is also support in the literature of the notion that these perceptions influence idea generation and development behavior.

A case is reported by Bralley in which a "significant increase" in the rate of idea generation and submission was apparently brought about by communicating information on company objectives (33). In light of the earlier conclusions, one might argue that the increase resulted from the increased clarity of goals and needs which, in turn, reduced the researcher's uncertainty regarding the potential rewards and costs associated with idea submission. However, Hillier's argument (80) and Gershinowitz's observations (65) suggest the following as an equally plausible explanation:

PROPOSITION 8: Perceptions of organizational goals and needs stimulate idea generation and development congruent with these perceptions, but stifle idea generation and development not congruent with these perceptions.

It has been hypothesized that the idea-generation behavior of industrial researchers can be explained and described at least partially by four variables:

1) Perceptions of time pressures due to current work.
2) Perceptions of rewards associated with effort on idea generation.
3) Perceptions of organizational goals and needs.
4) Interaction with other laboratory personnel.

One additional specific reward was also identified:

5) Positive and enthusiastic reception and evaluation of ideas by the organization's reviewers.

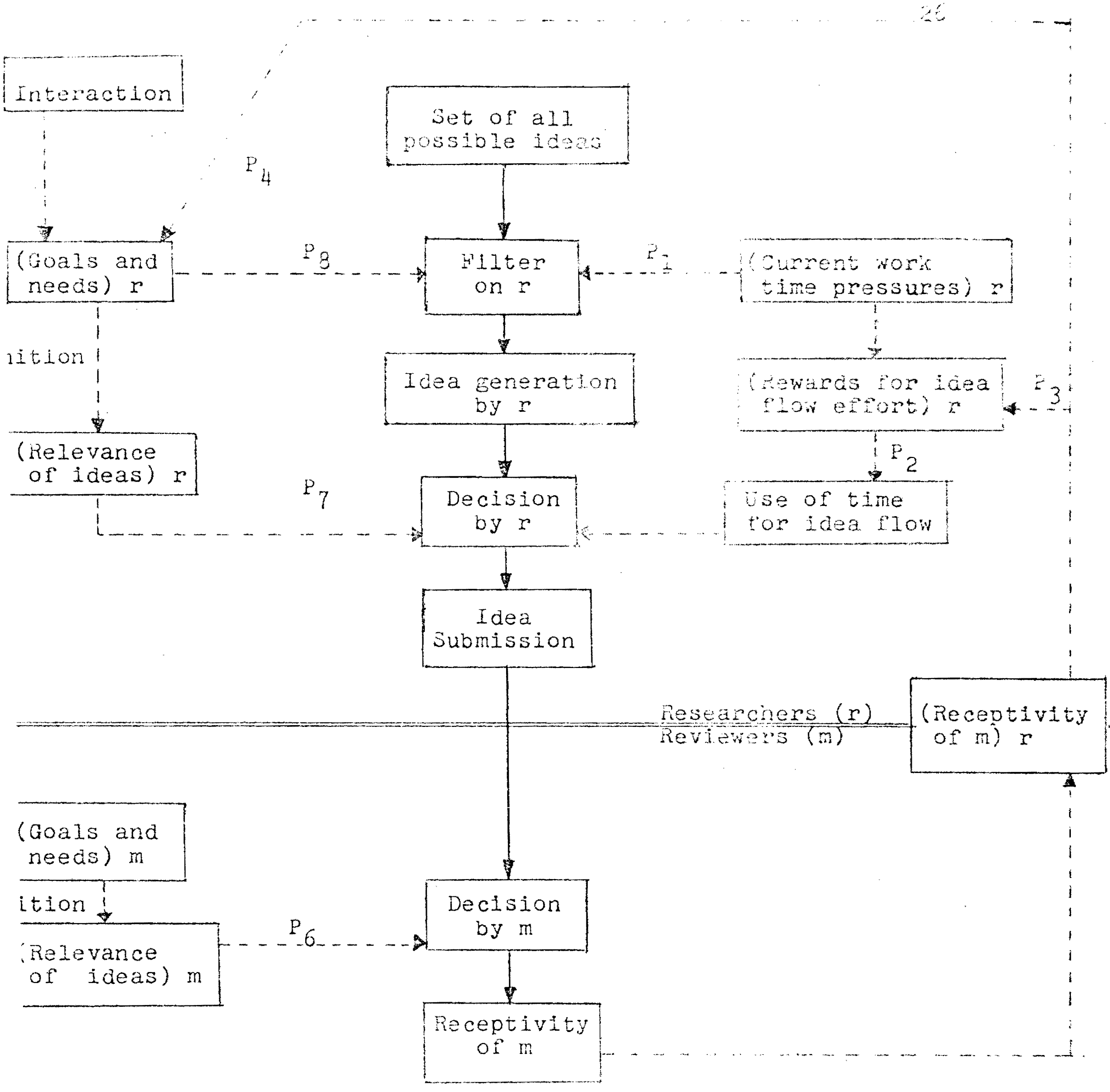
It is stressed that many other independent, dependent, and intervening variables might be considered (156). However, the variables identified in the literature search, and summarized above, provide a logical starting point.

A flow model, Figure 3, is presented in order to indicate the interrelation of the 8 propositions developed in the previous section. Directed arrows are used to indicate the dependent and independent variables.

The main concern is with the set of submitted ideas and the constraints imposed on this set by the idea flow process. From the model it is clear that two screenings precede the project selection screening. In the first screening, perceived organizational goals and needs, and time pressures due to current work, bias the set of generated ideas in the direction of ideas which are perceived to be "relevant" to the organization. The resulting set of generated ideas is again screened, with the major considerations being perceived "relevance" and perceived rewards and/or costs involved in expending the necessary effort for developing the generated ideas to the point of submission. Thus, the set of submitted ideas is prestructured according to the researchers' perceptions of time pressures due to current work, organizational goals and needs, and rewards (or costs) associated with effort expended on idea flow. Other flow models, showing the results of these successive screenings or the size of the set of ideas finally submitted for approval, are given by Rubenstein (137) and Rubenstein and Hannenberg (138).

Nearly all the normative project selection/resource allocation models start with the premise of a given set of alternatives and then attempt to structure that set in some manner most conducive to achieving firm and R&D objectives (see Section IV). The ideal operation of the screenings is that "good" ideas should be submitted, i.e., pass through the screens, but "poor" ideas should be screened prior to submission. If "good" ideas are screened, the organization misses an opportunity to reap the benefits of the subsequent project. Conversely, if "poor" ideas are not screened, the organization must expend scarce resources to evaluate them.

Recent research (22,24,28) resulted in data which indicate that, in the industrial R&D organization studied, ideas were being created which were not being submitted. Further, when compared with other ideas existing within the organization, subjective ratings of quality elicited from a panel of qualified judges reveal that the non-submitted ideas contained a significantly higher proportion of "good" ideas. A



:

denotes "proposition i"	"m"	denotes "reviewers"
is read "as perceived by"	——→	denotes "idea flow"
denotes "researchers"	- - - →	denotes "control flow"

Figure 3: Summary Flow Model of Idea Flow

mechanism was developed whereby the ideas not submitted during the normal operation of the organization became submitted and reviewed by management. The managerial review decisions also indicated a relatively high percentage of quality ideas in the set of non-submitted ideas. Perhaps the most startling finding was that 38% of the ideas achieving project status came from the non-submitted set which contained only 15% of the total ideas. In an unrelated study, Peters and Roberts (129) found somewhat similar results in university laboratories. Thus, based on rather speculative data, there appears to be a substantial loss of innovative potential due to ideas not being submitted. An analysis of the implications of the idea flow model will assist by providing insights as to how this occurs.

B. Discussion of the Idea Flow Model

In order to obtain empirical insights into idea submission, Baker (22) developed detailed case histories for 45 ideas which existed, but were not submitted, in an industrial R&D laboratory. These data verified the eight propositions in the flow model for the laboratory studied. For each idea it was possible to identify the factor which was cited as the primary reason the idea was not submitted or, for ten of the ideas, not resubmitted. The importance of time deadlines on current work and of expectations concerning the relevance as perceived by management was clearly demonstrated.

The data indicated that because of organizational review and reward mechanisms which focus attention on current project activity and because of the uncertainty inherent in the review and reward mechanisms associated with idea flow, research personnel tend not to function to their full creative potential. Ideas are generated only if market, mission, or production needs and if technical opportunities can be identified. Ideas are submitted only if the originator (individual to whom the idea is known) believes that the underlying needs and opportunities will be perceived as relevant by management and if rewards to the originator are seen as being at least equal to the cost of creation and submission, e.g., taking time from the current work. Unfortunately, relevancy is time and reviewer dependent; e.g., an idea judged not relevant by a reviewer at one point in time may be judged relevant by the same, or another, reviewer at another, or the same, point in time. Further, since expectations regarding reviewer evaluations are based primarily on reviewer actions on previous ideas, there can be a significant lag between the time reviewers change their evaluation behaviors and the time idea originators perceive the change.

The data also indicated further insights into the role of managerial behaviors and of the subsequent feedback effects. Submitted ideas often are not sufficiently developed technically and not sufficiently supported by evidence of relevancy that management can objectively evaluate them. The lack of completeness is explained by the idea originator investing minimum time on idea development because of pressures for current work accomplishment, of uncertainty regarding rewards for idea generation, and of lack of knowledge regarding relevant needs and technical opportunities. Since management is unable to evaluate the partially developed ideas, they behave in ways perceived by the idea originators as nonrewarding and costly; e.g., typical responses are "develop on your spare time", "state of art not sufficiently advanced", "too far out", or no response at all. As a consequence, expectations regarding organizational rewards for idea flow effort are modified downward and the cycle repeats. As new employees enter the organization they learn these low expectations from the veterans who have previously traversed full cycle. In such an environment it is little wonder the potentially creative employees fail to realize their potential and appear to "go dry" over time.

The above is a somewhat pessimistic characterization of the environment. However, it does illustrate the sensitivity of the innovative environment. Information of a technical and an organizational nature is required at strategic points in time. The nature and an initial specification of such a system is presented in Section VI of this paper.

C. <u>Research Opportunities</u>

It appears that innovative output is lost because ideas which exist within an organization are not submitted to the appropriate members of management. Two independent studies, one conducted in an industrial R&D laboratory (22) and the other in university laboratories (129) have identified significant numbers of non-submitted or unutilized ideas. Two studies in a limited number of organizational settings do not demonstrate that such an occurrence is common in U.S. R&D laboratories. However, they do seem to support such an hypothesis. There is a research need to investigate if the situation is widespread.

An idea flow model has been constructed and data from one organization tend to support it. The model is not complete. There are a large number of possible significant variables which have not been explicitly considered (156). These should be critically examined and a more complete model should be proposed. Empirical validation is needed for the various propositions and for the model. A scenario has been written for the model in one organization. Its general applicability is unknown and should be examined for a number of different organizations and organizational settings.

If the findings and the model, perhaps in some modified form, survive the rigorous evaluations, then they should be used for prescriptive purposes. It may be that improved information systems can provide a useful first step; however, in order to do so, they must be designed within the existing behavioral and design constraints and criteria. This suggestion is further developed in Section VI.

IV. R&D Project Selection and Resource Allocation*

Rubenstein (136) in 1957 and Brandenberg (34) in 1966 wrote remarkedly similar introductions to their respective papers. Both observed changes in the R&D environment which this author believes are still present in 1973. The post-World War II period was a period of optimism, permissiveness, and faith with respect to an industrial R&D revolution. As Rubenstein states, it was a period in which R&D "accomplishment usually came as pleasant, and often complete, surprises to others in the company" (136, pg. 95). The two authors saw this atmosphere giving way to one in which measurement, control, and evaluation were being imposed on the R&D activity. By 1964, Baker and Pound (25) could cite over 80 papers dealing directly with normative models for R&D project selection and resource allocation. In 1972, Baker and Freeland (23) updated the 1964 paper and cited over 175 references to project selection and resource allocation. However, this literature is deceptive. There is little evidence that these normative models are being utilized by the managers of industrial R&D.

The purpose of this section is to review the current state of knowledge regarding R&D project selection and resource allocation. The first subsection surveys the descriptive literature. Next the estimation problem is discussed and some of the major normative difficulties are identified. The third section is an exhaustive reivew of normative benefit measurement and resource allocation models. The section closes with a statement of current research opportunities.

*The author is indebted to Prof. J.R. Freeland of Stanford University for his collaboration on the "Normative Models" subsection.

A. Descriptive Literature: The R&D Project Selection Problem

There are numerous papers which contain speculations regarding the R&D project selection problem, for example every normative model, and some that report case studies and/or experiences, both the IEEE Transactions on Engineering Management and Research Management have published several such papers. In my opinion, Brandenberg's 1966 paper is the classic, and still most complete, descriptive paper (34). The paper reports the results of intensive investigations in 5 companies and preliminary explorations in 9 other organizations. All of the research activities were company-financed R&D in firms with formal, on-going research departments and budgets. However, the firms varied in size, product-market areas, underlying technologies, and R&D organizational configurations. Despite these differences, a consistent description emerged.

Brandenberg (34, pg. 17) reported that R&D project selection could be portrayed in terms of changes made in lists of currently active and proposed projects and of mechanisms determining the form which the changes would take. Typical changes were:

1) add or drop projects from the proposal list.
2) reprogram active projects.
3) replace active project with proposed project.
4) initiate a proposed project and reprogram a current one.
5) terminate a current project and shift resources to another.

The project selection decision can be much more realistically described as a stream of investment alternatives than as a once-a-year decision event (34, pg. 21). This characteristic still has not been adequately treated in the normative models.

Brandenberg also constructs a detailed process flow model of the project selection decision (34, pg. 18). In general the process consists of six main stages (, pp. 18-20).

1) generating and changing the inventory of project proposals.
2) reviewing the status of current and proposed projects for the purpose of deciding when to make a project selection and resource assignment decision.
3) choosing the projects on current and proposal lists which should be subjected to evaluation and comparison and the criteria, variables, and constraints which are appropriate to the analysis.

4) evaluating each resource allocation alternative.
5) comparing the alternatives and choosing among them.
6) recycling to gather additional evidence on given alternatives, to reformulate criteria, variables, and constraints, and to redefine entirely new alternatives.

At best, the existing normative models explicitly include only steps 2-5.

Brandenberg identifies the major shortcomings in normative models as their inability to treat the following characteristics of the R&D project selection problem (34, pp. 20-27):

1) the need for evaluating an intermittent stream of investment possibilities.
2) the diversity of projects along the R&D process spectrum.
3) the importance of individual scientists and engineers in providing decision information and in executing project assignments.
4) the close coupling of planning to implementation in R&D management.
5) the significance of alternative generation in relation to selecting and executing specific projects.
6) the characteristics and consequences of uncertainty in project decisions and of interrelated constraints, alternatives, and objectives.
7) the matching of project selection criteria to company objectives.

It would not be an overstatement to state that with respect to overcoming these shortcomings, the 1973 models are not further advanced than their 1964-66 counterparts. Many of these will be discussed in subsequent subsections. Special attention will be given to the interactive models which have attempted to address the inherent uncertainty through sensitivity analyses. At this point we turn to the project selection criteria. It should be noted that these criteria underly the benefit measurement methods to be discussed later.

In industrial R&D one would immediately identify profit maximization as an important criterion. Indeed, it is important, but it is not sufficient. For example, one of Mansfield's studies resulted in data which indicated that profit maximization accounted for 50% of the variation in the allocation of funds (93, Ch. 3). Conversely, Baker, Siegman, and Larson (27) found that urgency (immediacy of the need or opportunity toward which the idea is directed) and predictability (degree of certainty with which the methods and procedures for researching the idea are known) were significantly related to project selection decisions in one industrial laboratory.

Rubenstein (136, pg. 97) asked 37 laboratory directors to indicate the criteria used to judge the "results of R&D work" or "progress on R&D project or programs." The criteria and the number of companies citing the criterion are as follows:

1) Related to effect on sales volume or revenue--19.
2) Related to effect on saving in materials, labor, or other costs--17.
3) Related to effect on profits--13.
4) Related to time and cost of the technical solution--28.
5) Related to customer satisfaction--10.
6) Related to success of technical solutions--16.

By 1968, such lists had grown to include 25 or more items; however, the only differences were that criteria related to compatibility with product line and process capabilities and to the interest of the R&D personnel were included (27).

Even if the criteria are known, two significant difficulties remain. First, there is no known way to generate accurate estimates of contribution to the criteria for projects in their early stages or to combine the estimates for the individual criteria. Second, the relative importance of the criteria and, hence, of project contributions change over time as the environmental conditions, e.g., market, consumer, etc., change (136;103, Ch. 3). These difficulties will be discussed throughout this section.

The preceding comments view the project selection decision essentially as a decision event. Recent research indicates that in many organizations it must be viewed and modelled as a decision process. The process evolves from the fact that nearly all the organizational forms within which R&D is conducted are hierarchical and the R&D director is not at the highest level in the organization. Thus, the R&D director typically receives guidance in the form of budgets from the higher organizational levels and passes on guidance in the form of budgets to the managers in R&D. It is within this process at each organizational level that the project selection decision occurs. Baker, *et al*, have described this process for the R&D organization in one large federal R&D agency (26, pp. 1-4). They believe that this description in its general form applies to other agencies and to many large industrial organizations as well. The budget process is sequential in nature. Based on the guidance it has received and on its decision as to how the budget should be further apportioned, each superordinate level issues budget guidance in the form of recommended funding to its immediate subordinate levels. Thus, guidance information flows from the highest administrative level, through all intermediate levels, and on to the lowest organizational unit. In addition to budget guidance according to organizational entity, guidance is also issued according to technical areas. For example, a laboratory will receive guidance regarding its total budget and guidance indicating acceptable budgets for selected projects and groups of projects. The laboratory, in turn, will issue guidance for its subordinate organizations and for its project sub-entities.

After guidance reaches the lowest organizational level, the information flow is reversed. Each subordinate level transmits a proposed budget allocation to its immediate superordinate level in which the subordinate level details how it would allocate the guidance budgets if they were in fact to be authorized. These proposed allocations are integrated at each level and are then communicated to the next higher level. This downward-upward flow cycle may recur many times for more than one set of figures. Ultimately the highest organizational level receives a proposed budget allocation either consistent with the figures it originally issued as guidance or otherwise acceptable to it.

Eventually, the highest administrative level determines the total amount of funding which will be appropriated for the entire research organization. The appropriated allocations then flow through the organizational hierarchy in a manner analogous to the flow of the guidance information. At this point, each organizational level knows, within limits, the level of funding it can anticipate during the fiscal year and the budgetary constraints which have been imposed on its operation. Specific fiscal year budget plans are then made. Frequently, these plans must be revised during the year, since the eventual authorizations may deviate from the appropriations. Accordingly, several times during the year each organizational level is faced with determining a resource allocation which is characterized by a large number of budgetary constraints, defined both by organizational entity and by research area. Thus, in many organizations the project selection decision is sequential and hierarchical.

An alternative view of the R&D project selection decision process is offered by Connolly (47) and is also based on studies conducted with the R&D organization of a large federal agency. He argues (47, pp. 2,3) that for several reasons--the lack of an identifiable decision-maker, the extended time-period, the importance of structuring and linkage mechanisms--the planning and budgeting of expenditures in a large research laboratory is not conveniently modelled in terms of the decision event type model. It appears that it is more appropriately examined as a decision process than as a decision event.

The adjective "diffuse" is introduced in Connolly's paper to emphasize the characteristics of such processes which distinguish them strongly from decision events. He argues: (1) the processes are temporally diffuse, covering extended periods of time, with indistinct end-points; (2) they are multi-person processes, with influence (and decision-making responsibilities) diffused across a number of individuals; (3) the participants are typically separated by non-trivial physical distances, so that these processes are geographically diffuse; and (4) when found in organizational settings, the processes often cover several organizational levels, and are thus organizationally diffuse.

As one moves backwards in time from the output (decision) end of the process, it is probable that the decision problem changes, generally in the direction of less specificity and more uncertainty. In order to capture as much as possible of the decisional phenomenon of interest, one would look for the earliest possible formulation of the decision problem. In the focused decision event approach, the decision problem is typically formulated quite close to the decision itself, thus neglecting much of the process. In the approach proposed here, one aims to formulate the decision problem as early as possible, treating all activities from that point to the final decision as a single, large decision process. The analytic focus thus shifts from the individual to the process as a whole.

The extreme case of a diffuse decision, then, is a process in which many participants, over an extended period of time, generate a decision in response to some decision problem, working with alternatives which may initially be unclear or unknown, with costs and benefits not reliably estimable, with unclear and/or conflicting preferences, and with modifiable resources and constraints. Activities concerned with the clarification of objectives, predictions of future states, generation of alternatives, resolution of preference conflicts, and so on are thus treated as an integral part of the decision process rather than as external to the decision itself, the traditional approach. Essentially, this formulation suggests a shift from a micro- to a macro- level of analysis in treating complex multi-person decision phenomena. As Connolly states (47, pg. 4) it remains to be demonstrated that this approach is both feasible and useful. The hierarchical and diffuse characteristics of the R&D project selection process have not been studied and represent a potentially important area for additional research.

B. Estimation: Uncertainty, Cost, Time to Completion, Trade-offs

In an early 1958 paper, Klein and Meckling (88) argued that an efficient allocation of the development budget is a very different sort of problem than the efficient conduct of a current problem. By studying 24 post World War II military developments, they documented that uncertainty is an inherent characteristic of the projects which they examined. Marschak (103, Ch. 3) reported nearly identical conclusions in 1967. The recognition of inherent uncertainty in R&D reached the level of consensus that in 1971 Mansfield (101, Ch. 1) stated that R&D is an activity which is aimed at reducing uncertainty. Indeed the very definitions of research, development, and engineering offered in the opening section of this paper are based on the amount of uncertainty remaining.

A large number of authors have identified the various types of uncertainty which are inherent in R&D. Bright (35) discusses the various types of uncertainty in as informative and complete a fashion as any other source known to this writer. The following is a revision and expansion of Bright's list:

1) technical: scientific, engineering, production
2) market: demand, consumer response, competitive actions
3) behavioral: resistance to change, lack of receptivity by management, organizational conservatism
4) timing: obsolescence

Clearly the above list could be expanded even further. However, the key point should be established. Many different kinds of uncertainty are inherent in R&D. Further, these uncertainties result in the project selection/resource allocation decision in R&D being substantially more complex than the analogous decisions elsewhere in the organization.

The existence of uncertainty causes substantial difficulties in data estimation and normative modelling. First, there is the problem of adequately representing the uncertainty of the benefits expected to accrue. For example, Mansfield (98, Ch. 3) indicates the estimates of probability of technical success tend to be optimistic and that a criterion such as expected value of profit can only account for something like 50% of the variation in the allocation of funds. Mansfield's data (98, Ch. 3) also indicate that (1) the probability of commercialization is higher for large and medium technical advances than for smaller ones, for product improvements than for new products, and for projects aimed at familiar markets than for unfamiliar markets; (2) the probability of market success is higher for large or medium technical advances, new products, and products involving unfamiliar markets; and (3) for the sample, on the average, for overy 100 projects that were begun, 57 were technically successful, 31 commercialized successfully, and 12 were market successes. Clearly the overall uncertainty associated with industrial R&D can be substantial.

The most common approach for incorporating uncertainty into estimates of benefit is to weight the benefit estimates by a probability measure. The probability typically attempts to include technical and market uncertainties (e.g. 6,18,61,114,116). Several authors have noted the inadequacy of this approach and have attempted to include variance, as well as expected value, properties into the benefit estimate (e.g., 48,76,77). As yet no completely acceptable approach has emerged. The topic of benefit measurement is treated in depth in the next section, "Normative Models".

Uncertainty enters into the benefit estimation problem not only through technical and market uncertainties, but due to the fact that objectives for R&D and project requirements are uncertain in that they tend to change over time (136;103,Ch. 3). As a direct consequence of the inherent uncertainty, project requirements and budgets are often initially set in a most flexible fashion based on best predictions of cost, time, and technological advance (103, Ch. 3). However, also as a direct consequence of the inherent uncertainty, the initial best estimates are subject to large errors, especially at the initiation of the project (103, Ch. 3; 99, Ch. 5; 103, Ch. 4). Partly for these reasons, Hitch (81), Mansfield (101, Ch. 4), and Rosenbloom and Abernathy (1) among others have proposed and examined the strategy of parallel technological approaches to achieve project requirements and goals for high payoff and/or high uncertainty projects. Relatively little is known regarding the efficiency and effectiveness characteristics of the parallel approaches strategy.

The inherent uncertainty also results in substantial difficulty in the estimation of project cost and time to completion. The difficulty in accurately estimating cost, although just recently being widely publicized in the popular press, has been recognized by R&D managers and management scientists for a considerably longer period of time. In one of the more comprehensive empirical sutdies, Summers (103, Ch. 4) investigated cost uncertainties involved in large military development projects. Among his results are the following:

1) early estimates, made near the beginning of a development program, are particularly unreliable.
2) adjustments made for actual procurement quantities remove most of the gross inaccuracies in the estimates.
3) price-level corrections do not result in removing gross inaccuracies.
4) even when both procurement-quantity and price-level adjustments are made, the estimates are far from accurate and tend to be higher than actual costs.
5) cost estimate inaccuracies are greater for long development projects than for short and for projects which require a significant advance in technology.
6) perhaps the most important factor is change in the performance requirements.

Summers summarizes his findings by noting that as the project is conducted there are trade-offs between cost, time to completion, and performance characteristics.

Mansfield (99, Ch. 4) investigated the cost characteristics of 75 development projects in an ethical drug firm. In general he found results consistent with Summers, namely that type of project, technological advance, and development strategy were related to cost. Further, he reported (99, Ch. 5) that sizable errors are made at the beginning of the project and that cost overruns are greater for technically ambitious projects, projects with a wider spectra of activity, and projects of longer duration. There were instances when a general regression model was more accurate than estimates provided by the firm. Mansfield's data indicate that cost overruns are shorter, but time overruns longer, in the drug industry than in weapons development. However, both overruns are apparent in both settings.

Mansfield (101, Ch. 7) in the industrial environment and Glennan (103, Ch. 2) and Sherer (141) in the government R&D environment note that the time to completion of a project is a function of the rate at which resources are expended. Time to complete a project can only be decreased by increasing total cost because more errors result and the marginal returns decrease. Further, the cost to speed up a project varies from project to project depending on specific project characteristics.

Timing is also related to cost in other ways. For example, the largest percentage of the total cost of an innovation frequently occurs when tooling is designed and manufacturing facilities are designed and constructed (99, Ch. 6). Only about one-half of the cost of innovation is accounted for by R&D. In general, the cost to innovate is a function of the stage of the innovation process.

This brief section on estimation is closed by noting one other parameter interrelationship. The liklihood of technical and commercial success is thought to be a function of the total cost (3). Thus, the resource allocation decision is made even more complex. Not only is there significant uncertainty surrounding the resource and benefit estimates, but also the resource and benefit estimates are a function of the cost which is the decision variable. Hence, the dilemma, to determine a good allocation pattern, we must have estimates of time to completion, likelihood of success, manpower requirements, costs, etc.; however, all these parameters are a function of the resource allocation pattern selected.

There has been no attempt at an exhaustive search of the relevant literature dealing with estimation problems in R&D. It is a large literature and a complete survey of it is beyond this paper. However, the basic difficulties have been identified and documented. The brief discussion on estimation problems also functions to set the stage for the following discussion of normative models.

C. Normative Models*

All managers of research and development are faced with a common decision problem. In the operations research/management science literatures, this problem is referred to as a resource allocation or capital investment problem. A general description of the problem is:

> Given a set of alternatives which require common scarce resources (such as dollar budgets, manpower, and facilities), determine that allocation of the resources to the alternatives which will maximize, over all possible allocations, the value contribution of the resulting program.

Mathematical models of the resource allocation problem are comprised of two types of mathematical functions:

1. objective functions, which measure the value contribution of an allocation pattern, and

2. constraint functions, which describe the operating environment within which the decision must be made (of special importance is the availability of the scarce resources).

The purpose of this section is to assess the state of normative R&D resource allocation models.

As Souder (149) notes, the normative models can be classified into six categories: linear, nonlinear, zero-one, scoring, profitability, and utility. Linear (17, 26, 29) and nonlinear (18, 78, 135) models have linear and nonlinear objective functions, respectively, and have linear constraint functions. Zero-one models (123) use integer variables, scoring models (113, 114) use multiple criteria interval scores in place of financial measures, profitability models maximize some single measure of economic return (12, 50, 52), and utility models (48, 61) maximize the subjective utility of the investment. The mathematics involved range from simple ranking by benefit to the sophisticated algorithms of linear, non-linear, and integer programming.

Two papers have appeared which survey the literature associated with normative models of the R&D project selection and resource allocation decision; namely, one by Baker and Pound [25] in 1964 and one by Cetron, Martino and Roepcke [39] in 1967. These surveys have been periodically updated in the prefatory

*This section is a rewrite of an earlier unpublished paper by N.R. Baker and J.R. Freeland entitled "Recent Advances in R&D Benefit Measurement and Project Selection Methods," an invited paper at the 1972 National Meeting of the Operations Research Society of America in New Orleans, Louisiana.

remarks of numerous papers; however, no specific assessment of the literature has appeared since 1967. The purpose of this section is to provide a current assessment of the literature addressed to normative models of the R and D project selection and resource allocation decision.

1. A review of reviews

Baker and Pound [25] in 1964 and Cetron, Martino and Roepcke [39] in 1967 reviewed the R and D project selection and resource allocation decision model literature. The prefatory remarks in recent papers by Moore and Baker [113, 114], Alboosta and Holzman [3] and Souder [149, 150] serve to update the two reviews. Since most of these papers are accessible in the open literature, it is not necessary to treat them in deatail. Table 1 summarizes the content of the relevant portions of the cited papers. In addition, a bibliography compiled from the cited papers and a review of the recent literature is included.

The reviews suggest limitations inherent in the currently proposed normative models. An integration and summary of these limitations is:

1. inadequate treatment of risk and uncertainty
2. inadequate treatment of multiple, often interrelated, criteria
3. inadequate treatment of project interrelationships with respect both to value contribution and to resource utilization
4. no explicit recognition and incorporation of the experience and knowledge of the R and D managers
5. the inability to establish and maintain balance in the R and D program; e.g., balance between basic and applied work, between offensive and defensive activity, between product and process effort, between in-house and contracted projects, between improvement and breakthrough orientation, and between high risk - high payoff and moderate or low risk - moderate payoff opportunities
6. perceptions held by the R and D managers that the models are unnecessarily difficult to understand and use
7. inadequate treatment of the time variant property of data and criteria and the associated problem of consistency in the research program and the research staff.

Given these limitations, it is clear why few normative models of the R and D project selection and resource allocation decision have been implemented and used by R and D managers.

TABLE 1: SUMMARY OF LITERATURE RESULTS

ORS	MODEL CATEGORIES	COMPARISON/EVALUATION OF MODELS
r/Pound) [25]	Decision theory Economic Analysis Operations Research	Discussion of general descriptive nature of project selection material. Characteristics of 30 representative models. Brief description of areas of application and data requirements.
n/Martino ke) [39]	Decision theory Economic Analysis Operations Research	Features which describe input and output characteristics. Ease of use: data. Areas of applicability. Table summary for 30 models.
/Baker) 114]	Scoring models Economic models Risk analysis Constrained optimization	Brief discussion of each model type. Summary of some accepted descriptive insights. First empirical data relating output from different model forms.
sta/Holzman) [3]	Project scoring Project index Math programming Utility models Descriptive	Brief discussion of each model type. Identification of critical factors not included in most models.
?) 1,150]	Linear Non-linear Zero-one Scoring Profitability index Utility	Scoring models used to evaluate representative models from each class. Uses data from 30 actual projects to perform comparative analysis of four models designed to represent main categories.

The 1964 review [25] clearly illustrated the lack of implementation and use and identified the need for formal, hopefully empirical studies of the implementation process. With the exception of Baker's work [96] and current work at a large governmental agency [26] little has been accomplished beyond the early economic return and rating models (99, Ch. 3).

Souder [149, 150] has made a promising start in providing approaches for evaluating R and D decision models. Expanding a list of R and D model performance characteristics suggested by Cetron, Martino and Roepcke [39] and incorporating responses from a number of R and D managers and management scientists, Souder [149, 150] prepared a list of performance criteria and characteristics. This list was used as the basis for a scoring model to determine the "relative suitability" of certain classes of quantitative models. Since the measure of suitability is only a relative measure, it provides little insight into the "absolute suitability" of the models. Two applications of the scoring model have been reported [149, 150] and the results indicate some strengths and limitations of a number of quantitative R and D selection and allocation models which have appeared in the literature. The list of performance criteria and characteristics is also useful as a reference guide during model design. A slightly expanded version of the list is reproduced as Table 2.

Souder [149, 150] has also provided data which is useful for preliminary assessment of the effectiveness of four resource (budget) allocation models. The four models are referred to as a "nonlinear model", a "linear model", a "zero-one model", and a "profitability index model" and are intended to be specific representations of broader classes of models. The output from each test model is evaluated against the output from two control models; namely, a "benchmark model" which allocates the budget in a pro rata fashion, i.e. the budget is distributed proportionately to the projects based on the project's maximum annual funding level and an "ex post optimal model" which funds at their maximum annual level all projects that terminated as successes. Perhaps the most interesting result is that, for the specified set of input data, the benchmark model performed as well, and in some instances better, than the test models. In Souder's data, the estimates of the probabilities of technical success undergo significant change over the life of the projects and the benchmark model performs best when the data is least valid. It is impossible to determine whether the result that the benchmark model is as effective as the mathematical

TABLE 2: CRITERIA AND THEIR CHARACTERISTICS*

ALISM CRITERION CHARACTERISTICS

odel includes:
- Multiple objectives
- Multiple constraints
- Market risk parameter
- Technical risk parameter
- Manpower limits parameter
- Facility limits parameter
- Budget limits parameter
- Premises uncertainty parameter

2) CAPABILITY CRITERION CHARACTERISTICS

Model performs:
- Multiple time period analyses
- Optimization analyses
- Simulation analyses
- Scheduling analysis

EXIBILITY CRITERION ARACTERISTICS

del applicable to:
- Applied projects
- Basic projects
- Priority decisions
- Termination decisions
- Budget allocation applications
- Project funding applications

4) USE CRITERION CHARACTERISTICS

Model is characterized by:
- Familiar variables
- Discrete variables
- Computer not needed
- Special persons not needed
- Special interpretation not needed
- Low amount of data needed
- Easily obtainable data

ST CRITERION CHARACTERISTICS

del has:
- Low set-up costs
- Low personnel costs
- Low computer time
- Low data collection costs

6) ADDITIONAL CRITERIA

Model considers:
- Competitor efforts
- "Strategic need"
- Project dependencies (value, resources)
- Updating data
- "Flags" for potential problem areas

Souder, W.E. [150]

optimization models is an artifact of the data or is a general result which will hold for uncertain environments. Regardless, it is an interesting and critical hypothesis which should be systematically tested in subsequent research.

Several papers have been written by individuals who are not academicians, but who are management science professionals working for an organization and who are attempting to improve the organization's R and D selection and allocation decision process, e.g. [17, 18, 123, 135]. In all but possibly two situations [31, 123] there is little evidence that the models proposed by these practitioners were used, or if used, survived after the departure of a critical management sponsor and/or the model builder. Hence, these papers provide little assistance in assessing the potential effectiveness of the proposed models; however, they do suggest some level of dissatisfaction on the part of the R and D management.

Two recent studies whose results are just appearing [26, 96] report on on-site experiments designed to evaluate specifically constructed models. The results reported to date are generally optimistic, but are too incomplete and specific to provide general results. Additional on-site experiments are urgently needed in order that general conclusions can be developed.

As Table 1 illustrates, several attempts have been made at categorizing the normative models. The attempts have been only partially successful. There appears to be growing recognition that there exist two primary categories of models; namely, benefit measurement models and project selection/resource allocation models [16, 128]. The following sections discuss recent advances and research opportunities associated with each of these two categories.

2. Benefit measurement methods: an overview

All of the benefit measurement methods proposed to date require some well informed respondent, or group of respondents, to provide subjective inputs regarding characteristics of the proposals under consideration. Admittedly the level of subjectivity varies considerably between a simple rating and a detailed economic analysis; however, even the economic analysis of R and D proposals requires subjective judgement. The benefit measurement methods can be described as systematic procedures for obtaining and integrating subjective and objective benefit data. The methods can be classified, therefore, on the basis of the thought processes which are imposed on the respondents.

The first category includes such approaches as Q-sort, ranking, rating, paired comparisons, dollar metric, standard gamble, successive ratings, and successive comparisons [44, 53, 61, 94, 127, 128]. Each of the methods requires the respondent(s) to compare one proposal either to another proposal or to some subset of alternative proposals. The respondent is requested to specify which of the two entities is preferred and, in some approaches, to specify the strength of preference. A set of project benefit measures is then computed by performing specified mathematical operations on the stated preferences. Since benefit measures have meaning only relative to the set of alternative projects evaluated, any time an alternative proposal is added or deleted from the set under consideration, the entire process must be repeated. These approaches comprise a category termed comparative approaches. For example, the Cramer and Smith paper [48] suggests an especially interesting approach for treating the risk associated with R and D proposals by using certainty equivalent forms of utilities in a proposal selection model.

A second type, usually identified as scoring models, assumes that a relatively small number of decision criteria can be defined which, when properly related, can be used to specify the desirability of allocating resources to each alternative proposal [8, 63, 114, 116]. The criteria are typically related to specific project characteristics such as cost, manpower availability, scheduling feasibility, probability of technical success, etc. The respondents must determine the merit of each project with respect to each criterion. The resulting vector of scores for an alternative is useful as a diagnostic for identifying the strengths and weaknesses of each alternative. The project scores are then combined, usually by addition or multiplication, to yield an overall benefit measure. The benefit measures are relative, but alternatives can be added or deleted without affecting the benefit scores of other alternatives. This approach has received renewed emphasis since 1969.

The final class, benefit contribution models, require the respondent(s) to tie projects directly to research and development objectives or to systems requirements [21, 50, 76, 111, 122, 146]. Included in this class are the numerous economic return, cost/benefit, risk analysis, relevance tree, and assessment tree approaches. Project benefit is measured either in terms of contributions to a number of objectives or systems. The resulting measure may or may not be relative depending on the specific approach.

Alternatives may be added or deleted without influencing the benefit scores of the other alternatives.

Studies which focused on empirical investigations of various considerations of selected benefit measurement methods have appeared in the literature. The results are too preliminary to be presented as general conclusions: however, they do illustrate the potential of research in this area. A brief summary is:

1. Data from two studies (53, 128) support the hypothesis that simple linear relationships exist between the outputs generated by using different comparative approaches. The question which remains is: Are the thought processes underlying the comparative approaches sufficiently alike so that the resulting benefit measurements are closely related. Some existing results indicate that they are. However, a recent study by Goodwin (67) does not support the hypothesis and suggests, at least in his application, that weights from ratings were more valid than weights from rankings.
2. Present research in a large governmental agency is investigating the relationship between the measures generated by a comparative to those generated by a scoring model. Results to date are inconclusive in that for some respondents the measures are nearly identical whereas for other respondents the measures, and subsequent recommended resource allocations, are quite dissimilar. It is not yet possible to explain this observation.
3. A suggestion by Dean and Nishray (50) prompted Moore and Baker (113, 114) to conduct an intensive study of scoring models. Their work demonstrated that a linear (versus multiplicative) scoring model can be constructed such that its output is rank order consistent with the outputs of a profitability index model and of a linear programming model. Concepts of the effective range and discriminatory power were defined for scoring models, a theoretical foundation for scoring models was developed, and a procedure for designing scoring models was detailed. Evaluation of the design procedure in an R and D environment has not yet been accomplished.
4. In the Moore and Baker (113, 114) study, the additive model consistently produced more consistent results than did a multiplicative model. A recent paper by Goodwin (67) supports the Moore-Baker assertion that linear scoring models are better prescriptive

models than multiplicative scoring models. It is quite interesting that Einhorn's studies [55] indicate that, in a quite different environment linear models provide a satisfactory approximation to the respondents' responses to multivariate stimuli in a decision situation. There is increasing evidence that linear objective functions have validity in a multiple criteria decision situation.

5. No formal investigation of benefit contribution methods was found by the authors. Many industrial organizations use one or more of the economic return approaches, especially in the evaluation of development and engineering proposals [13,45,52]. A recent study [96] indicated that a risk analysis approach was favorably evaluated by the R and D management of one organization. Several military agencies and government contractors have apparently tried to use a relevance tree or cost/benefit type of approach [50, 122,134, 146]. It appears that most of these have been discontinued due to the excessive cost and time required by management and the research staff to initialize and update the data set.

In summary, despite the large number of benefit measurement models prposed in the literature, relatively little is known about the performance of these approaches when applied within an R and D environment. This is a critical area for future research.

3. Benefit measurement: recent advances

Once the two primary categories, benefit measurement models and project selection/resource allocation models are recognized and accepted, it is natural to think of the benefit measurement models as providing input for the objective function of a project selection/resource allocation model. For a selection model, i.e. a model which recommends acceptance or rejection but does not consider funding levels, a point estimate of value is sufficient. However, for a resource allocation model, it is necessary to have more than a point estimate of value. Some type of function, continuous or discrete, must be specified which relates benefit to the feasible funding levels associated with each proposal under consideration. The methods previously cited and discussed generate single point estimates of value.

Two independent, but remarkably similar approaches for constructing such a function have recently been reported, one proposed for a governmental agency [26] and the other for an industrial firm [3]. Both approaches are based on three alternative funding levels which are identified for each proposal by the R and D personnel in answering such questions as:

1. What is the minimum level of resources necessary to sustain a project effort with the current project objectives?
2. In your judgment, what level of resources is required to adequately pursue project objectives (in the governmental agency, this level is the level expected to be recommended by higher authorities)?
3. What is the maximal amount of resources which could be absorbed by this project if an all out program were desired?

In the governmental agency, one of the benefit measurement models is then adapted and used to generate a point estimate for each funding level. For the industrial application, a "project risk function" is estimated by a least squares approximation to the cumulative distribution function

$$f(x) = 1 - \frac{1}{(1+x^c)^k} \qquad x \geq 0, \; c,k \geq 0$$

A project benefit estimate and the project risk function are sufficient to define an expected benefit function for each proposal and each funding level. In each application, the benefit functions are used to define an objective function for a resource allocation model. Apparently the basic approach has a rather broad potential for application.

Recognizing the inherent difficulty of quantifying subjective judgments, management scientists have recently adopted an interactive approach in the design of benefit measurement and resource allocation information systems. Cochran, et. al. [45], have defined a system which easily permits the user to modify the economic characteristics and observe the impact on expected net present value and return on investment. Eventually, for each proposal, the user determines the economic characteristics and, hence the expected net present value, which he feels best describes the proposal. The selected expected net present values are then input to the objective function of a project selection model which maximizes overall expected net present value subject to a constraint on total R and D costs. The cycle can be repeated any number of times thus providing the user with an opportunity to assess the impact that changes in the underlying economic characteristics of the proposals will have on the portfolio of selected projects.

A similar sensitivity analysis approach is presently being evaluated by Baker, Shumway, Souder and Maher [26] within a governmental agency. As mentioned previously in this section of the paper, piecewise linear benefit functions are generated for each proposal. These benefit functions serve as inputs to the objective function of a constrained resource allocation model. This information system also operates in a time share, interactive computer mode. The user has the opportunity to modify the funding levels at which benefit is estimated and the estimates of benefit. Thus, the user can change the benefit function associated with any one or more of the alternative proposals. It is also possible to change any of the several constraints. Accordingly, the user can examine the impact various changes have on the portfolio of selected projects and the associated funding levels. Although independent efforts, it is important to recognize that both of the interactive approaches concentrate on the analysis of the impact on the portfolio, not on the individual project values. Perhaps there is a growing recognition that it is easier and more meaningful for R and D managers to evaluate alternative portfolios than to evaluate isolated proposals. Not only is this likely in situations with multiple criteria, but also in situations where economic return is the dominant consideration.

The two interactive approaches just discussed utilize the decision maker in the solution procedure. As the user, the decision maker determines which "what if" questions will be asked and hence what information will be generated. However, they both assume that it is possible to quantify the decision maker's overall benefit function. Although applied in an unrelated environment, Geoffrion, Dyer, and Feinberg [64] suggest an interactive approach which does not assume that the benefit function can be quantified. Their approach is specifically designed for a multiple criterion environment. It assumes that if the decision-maker could somehow specify an overall benefit function which relates the multiple criteria, then known mathematical algorithms could be used to evaluate alternative proposals. However, they never require this benefit function to be identified explicitly. Instead, only local information about the benefit function is actually needed to carry out the calculations. The approach allows the decision-maker to make iterative decisions which will improve the overall benefit function at each iteration. Starting with a set of alternatives which do not exceed the available resources, the decision maker evaluates the trade-off between criteria and determines an improved solution which does not exceed the available resources. This interaction between the decision-maker and the mathematical model is accomplished in criterion space rather than in the space of the decision variables. The application of such an approach to R and D resource allocation offers some exciting possibilities.

Another approach for handling multiple objectives is that of goal programming as illustrated by the Charnes and Stedry model [41]. In this case the multiple objectives are expressed as goals, e.g. profit, market shove, cost in dollars, cost in manpower, etc. could all be goals. If desirable goal levels can be determined, then a set of project proposals and a resource allocation plan can be found which minimizes the cost of deviation from the goal levels.

Promising new directions have been identified in the area of benefit measurement and these need to be more fully developed, extended, and applied. A methodology for constructing the functions which relate benefit to funding level has been proposed that appears to have broad applicability. In response to the inherent difficulty of quantifying subjective judgements for a multiple criteria decision problem, interactive approaches have been suggested for benefit specification. One approach goes beyond the sensitivity analysis interaction, which requires an initial benefit function as a starting point, to an approach which does not assume that the benefit function can be quantified. The interactive models allow portfolio as well as proposal evaluation. The area of benefit measurement is rich in its potential for future research.

4. Decision models: recent structural considerations

A number of selection and allocation models have been suggested. Most of these have appeared in the open literature and were surveyed in the earlier reviews (25,39). The model forms were briefly described in the introductory comments, the models are in the open literature, and, hence, they will not be further discussed at this point. Some recent developments have suggested interesting and rather unique ways of handling structural considerations frequently found in the R and D environment. The remainder of this section identifies and briefly discusses these efforts.

The Charnes and Stedry model (41) is an interesting integration of the concepts of chance-constrained programming and goal programming. The model determines the minimum expected short run and long run resource requirements necessary to achieve specified goals and includes explicit consideration of interim adjustments due to research breakthroughs. In essence, the breakthroughs represent significant advances in knowledge which lead to high priority activities whose resource requirements pre-empt resources otherwise allocated in the long-run plan. This approach is a departure from the more typical allocation model which produces an optimal plan based on forecasted developments, but does not provide for the adjustments necessary when the forecasted developments materialize and place unexpected demands on the resources. This model is an important first step in modelling crisis or breakthrough planning. Further work is required to modify the Charnes and Stedry model for use in other environments, e.g., the urban environment, and to develop alternative model forms for crisis and breakthrough planning.

Hess [78] and Rosen and Souder [135] recognized that the expected value of a proposal depends on the distribution of allocated resources over time. However, they were unable to completely consider this relationship in their models. Atkinson and Bobis [18] have formulated and solved a resource allocation model which explicitly accounts for the variation of allocated resources over time. It is the first model to offer a structure which includes the unwieldy consideration of a single criterion, expected profit, and, it is not clear that the approach can be extended to a multiple criteria situation. Additional research is justified on the general approach suggested by Atkinson and Bobis.

Decisions which impact on the allocation of resources to R and D proposals are frequently made at several different levels in a hierarchical organization. Each level makes decisions, or recommendations, based upon the information it receives from higher and lower levels. Since each level operates with some autonomy, there exists the problem of coordinating the decisions made at the various levels. The resource constraints of any constrained allocation model implicitly recognize this by identifying such factors as total available budget. The Baker et. al., model [26] is designed to incorporate constraints which are generated by two hierarchies--an organizational hierarchy of headquarters, divisions, branches, laboratories etc. and a research hierarchy of programs, projects, tasks etc. Thus, constraint intervention is one means of hierarchical coordination used in R and D resource allocation models.

At a higher level of analysis of hierarchical considerations, it is important to note that resource allocation decisions can be affected by the specific form of the hierarchy, by the nature and content of the information flow, and by the way in which coordination is attained. Until recently little opportunity existed for conducting such analyses because of the lack of appropriate mathematical methodology and model structures; e.g., the Dantzig-Wolfe decomposition approach is primarily a computational technique and is not suitable for such analyses [49]. Recent advances, however, appear to offer sufficient structure so that important general insights into hierarchical decisions can be derived [40, 60, 140, 162]. This research should continue and applications to the R and D environment should be investigated.

The research summarized above represents significant advances in the structure of R and D resource allocation models. They are important first steps in providing for the development of resource plans which anticipate research breakthroughs, the consideration of time variant data and criteria, and the study of effects of hierarchical structure. These characteristics are inherent to most R and D environments and additional research is warranted.

5. Summary and discussion

A substantial amount of progress has been accomplished in the last few years with respect to the R and D project selection and resource allocation problem. Important initial efforts have been reported which eventually should lead to overcoming many of the limitations identified in the earlier reviews. These, and associated opportunities for future research have been identified throughout the paper.

In addition to the research opportunities associated with value measurement and with resource allocation models, there is a need for additional empirical research oriented toward a better understanding of the R and D environment and of the behavioral process by which decision and information systems become adopted and implemented. Several instances exist in which resource allocation models apparently were adopted, but subsequently were discontinued when the model builder and/or a sympathetic management sponsor left the R and D organization, e.g. [18, 135]. This is hardly a satisfactory long-term condition and research is required to explain why this has occurred and to suggest alternative adoption processes and behaviors.

The trend in application appears to be away from "decision models" and toward "decision information systems". Two legitimate reasons can be suggested for this trend. First, the existing models are incomplete in the sense that they do not include all the important, relevant aspects of the R and D environment. As a result, the manager is forced to adjust the recommended allocations in order to account for the often numerous, environmental conditions not included in the model. The second reason is that the decision problem is characterized by multiple criteria many of which are not easily quantified. The typical approach is to quantify preferences or subjective estimates of benefit with methodologies which are far from satisfactory. As a result, managers are highly skeptical of the validity of the estimates and of the subsequent allocation recommendations. The rather simple models of Cochran, et. al. [45], Baker, et. al. [26] and Geoffrion, et. al. [64] operating in an interactive mode appear to be most promising as information systems for utilization during the decision process.

The more complex models, such as those of Charnes and Stedry [41], Atkinson and Bobis [18], and Ruefli [140] probably will not find use during the decision process. Their benefit will more likely accrue from higher order analyses of broad structural and policy decisions, e.g., see Hess [78]. Extensions in this area offer the opportunity to study such diverse considerations as:

1. the impact of scientific breakthroughs on resource allocations over time.
2. the identification of good funding patterns under various environmental conditions.
3. the impact of hierarchical structure and coordination on resulting resource allocations.

These and related extensions should yield rule-of-thumb guidance for interpreting information generated during a specific decision analysis and should provide data useful for the design of better organizational structures and processes.

D. Research Opportunities

There are numerous research opportunities in the R&D project selection and resource allocation decision area. Many of these have been identified previously in this section. The purpose now is to summarize and integrate the ones identified. The presentation will occur in three areas: design of better normative models, abstract analyses of the diffuse and hierarchical characteristics of the R&D decision processes, and empirical studies and analyses.

One of the major limitations of the current normative models is their inability to adequately handle the inherent uncertainty. The uncertainty causes estimation inaccuracies in both the input data and the benefit measures. Various approaches have been suggested to work around the inaccuracies including chance constraints (41), distribution estimates (76,77), certainty equivalent estimates which consider variance (48), and interactive systems (26,31,64). These approaches help, but they are not sufficient. Thus, 1) existing approaches to treating uncertainty need to be extended and new, improved approaches need to be developed.

A second major limitation deals with the area of benefit estimation. The second research opportunity is: 2) improved methods of estimating the benefit of a single alternative are needed especially for benefit as a function of the allocation level; there are no acceptable methods for combining the individual estimates to produce an estimate of benefit for a program or portfolio, especially when the project benefits are interrelated or when current and proposed alternatives are being evaluated simultaneously; and the recently proposed interactive benefit measurement approaches (64) need to be designed specifically for R&D project selection and resource allocation. Finally, 3) existing normative models need to be extended to include such characteristics as a stream of intermittent investment alternatives; reprogramming in response to a crisis or breakthrough (48); the criticality of the assignment, or non-assignment, of specific R&D personnel, and the time variant property of data and benefit estimates.

Another major area of research opportunity is abstract analyses of the hierarchical and diffuse characteristics of the R&D decision process. Recent extensions (60,140) to decomposition approaches in mathematical programming appear to offer the required mathematical structure. Both extensions in methodology and specific analyses are required. In summary, 4) conduct abstract analyses of the hierarchical and diffuse characteristics of the R&D decision process in order to recommend improved organizational structures, control and coordination policies, and management information flows.

The final research area is empirical studies and analyses. It is critical to design more useful R&D project selection and resource allocation information systems. To do this, it is critical 5) to evaluate existing models in a field setting, i.e., in R&D activities, to determine why models which apparently were being utilized were subsequently discontinued (135), and to better understand how the use of a normative model impacts on individual and group behavior, type and network of information flows, and quality of decision output. In the area of benefit measurement, 6) empirical studies are needed to better describe the decision processes of individual decision-makers, to compare the output of alternative benefit measurement approaches, to clarify the implications of goal programming as an approach in multiple criteria situations, and to evaluate the recent interactive approaches. Finally, 7) it is important to develop better functional representations of the cost/time, benefit/allocation, allocation/probability of success, and other relationships in order that these functions can be used in the normative models (18).

V. Motivation and Performance in R&D

The previous section surveyed the current literature with respect to R&D project selection and resource allocation. The focus in this section is on the activity performed on the funded projects. Specifically, this section is concerned with the performance of scientists, researchers, and technicians in R&D laboratories and with the variables which influence how well they will perform on assigned tasks, as contrasted with idea generation and problem solving. At literally the same time the first draft of this paper was being prepared, Martino (106) published a most relevant paper in the August 1973 issue of the IEEE Transactions on Engineering Management. The section begins with a summary of the Martino paper.

A. <u>The Variables</u>

Martino (106) surveyed the behavioral science literature related to the performance of R&D scientists and engineers. He identified 53 input variables which he classified into eight categories (106, pp. 69, 70):

1. variables under the direct control of management which affect the climate or atmosphere in which the researcher works.
2. variables under the direct control of management which enable the researcher to carry out his work.
3. variables representing the consequences of external conditions, or factors inherited from prior decisions, which are beyond the control of the R&D manager.
4. variables which are inherent psychological characteristics of the researcher.
5. variables representing characteristics acquired by the researcher.
6. variables descriptive of the researcher or his job.
7. variables which link other variables.
8. variables representing activities carried out by the researcher in performing his work.

In addition, he identified 13 output variables.

After identifying the variables, Martino constructed a cross-impact matrix showing interactions between the variables (106, pg. 71) and also summarized the interactions by identifying the impacting variable, the impacted variable, and the relevant literature citations for each relationship (106, pp. 72,73). Five of Martino's output variables are of direct concern to this section. The impacting variables, impacted variables, and references will be presented for this output variables. All variables will retain Martino's number. The first digit in the variable number identifies the variable class with the output variables designated as class 9.

The relevant input, or impacting, variables (106, pp. 69, 70) are:

1.1) Leadership style: Supervisor's manner of exercising control over a researcher: "<u>laissez-faire</u>," "participatory," or "directive."
1.4) Percent of leader's time on project: Percentage of leader's time actually allocated to project-related work.
1.5) Diversity: Number of different types of work for which a researcher is responsible.
1.6) Total workload: Extent to which a researcher's time is taken up with prescribed tasks (and thus unavailable for "thinking").

2.1) Equipment: The level of research equipment and facilities available.
2.2) Support staff: Number of technicians and administrative workers supporting the researcher.
2.3) Funding: Dollar level of financial support available to a researcher.
2.4) Project staff size: Number of people working on a specific research project.
2.6) Project length: Calendar time allowed for completion of a specific research project.
2.7) Group size: Number of researchers in a laboratory working on similar or related activities.
3.1) Laboratory size: Total number of people in a laboratory.
3.2) Time in organization: Length of time a researcher has been employed by his present organization.
3.5) Type of organization: Institutional setting of laboratory, as government, industry, etc.
4.1) RAT score: Score on the Remote Associates Test.
4.2) Differentiation: Ability of researcher to differentiate among his co-workers on the basis of their performance (this characteristic is measured by a standard test).
4.3) Guilford score: Score on the Guilford Battery of psychological tests.
6.1) Age: Calendar age of the researcher.
6.2) Time since degree: Years since the researcher received his highest academic degree.
7.1) Awareness of user needs: Extent to which the researcher is aware of the actual needs of the customers of his laboratory.
8.2) Consult internally: To seek advice or help regarding a research effort from someone within the organization.
8.3) Consult with outside colleagues: To seek advice or help regarding a research effort from persons doing similar work but outside the organization.
8.6) Use hired consultant: To seek advice or help regarding a research effort from an outsider hired for the purpose by laboratory management.
8.11) Serve as internal consultant: To provide help or advice with respect to a specific problem at the request of someone in the organization.
8.13) Technical discussion: To talk with one or more other researchers about technical matters not directly related to current work.

The relevant output, or impacted, variables (106, pg. 71) are:

9.3) Usefulness: Peer judgment of a researcher's usefulness to his organization.
9.4) Contribution: Peer judgment of a researcher's contribution to his discipline or field.
9.5) Unpublished reports: Number of reports prepared for private circulation within an organization.
9.6) Research productivity: Magnitude of research output of an individual or group (total count of papers or reports).
9.7) Research quality: Judged or measured degree of originality or innovativeness in the work of an individual or organization.

Table 3 is a summary of the relationships reported by Martino. The scarcity of relationships and supporting literature clearly illustrates that considerable work remains prior to a systematic, integrated body of knowledge in this area.

B. Performance of Scientists in Organizations

By far the key initial assessment of this area is contained in the 1966 book by Pelz and Andrews (126). The book presents data from a study conducted in 1959 involving 1311 scientists and engineers from 11 laboratories in industry, university, and government. It is specifically concerned with identifying, and understanding the relationships of, several organizational factors and a number of output measures such as 9.3, 9.4, 9.5, and 9.6 of Martino's set of output variables. Many of the results appeared in papers published prior to the book, but these were integrated into the book. Hence, no attempt is made to cite or reference these papers. The interested reader is referred to the book for this information. The findings are summarized in two ways: 1) an overall summary is presented in the text and 2) specific findings are listed in Table 4.

It is possible, therefore, to use the Pelz-Andrews results to construct a scenario which is descriptive of effective scientists. Effective scientists are self-directed by their own ideas and value and seek the freedom to pursue their own ideas. Coordination is provided by self-direction and by allowing several others a voice in shaping their directions. They interact vigorously with their colleagues. The work of the effective scientists is diversified; that is,

cting Variable	Impacted Variable (References)
1.1	9.3(57);9.4(9,57);9.5(57);9.6(73,108,109,160);9.7(69,71)
1.4	9.7(69,70)
1.5	9.5(57)
1.6	9.7(69)
2.1	9.6(109); 9.7(69,71)
2.2	9.7(69)
2.3	9.6(108,109); 9.7(69,70)
2.4	9.7(69)
2.6	9.7(70)
2.7	9.7(118,161)
3.1	9.6(109)
3.2	9.3(9); 9.4(9)
3.5	9.6(109); 9.7(71)
4.1	9.3(9); 9.4(9); 9.5(9)
4.2	9.6(69); 9.7(69)
4.3	9.6(118)
6.1	9.6(54,93,133); 9.7(92,93,133)
6.2	9.6(54,133); 9.7(54,133)
7.1	9.7(71)
8.2	9.7(4,105)
8.3	9.7(4,69,105)
8.6	9.7(105)
8.11	9.3(57)
8.13	9.3(57)

Tabel 1: Relevant Relationships from Martino's Summary (106)

A. Freedom (126, Ch. 2)

1) A combination of freedom and coordination with several other people is both feasible and helpful.
2) This involvement keeps the researcher informed on major goals of the organization, provides stimulation from people showing interest in his work, and exposes him to a diversity of viewpoints.
3) An individual can exert more influence in a flat organizational structure with fewer levels.
4) Continued direction by a chief will stunt initiative and independence.

B. Communication (126, Ch. 3)

1) Contacts with colleagues increased performance.
2) Contacts were most useful if originated by the persons concerned (the man or his colleagues.)
3) Frequent contacts with many colleagues were more beneficial than frequent contacts with just a few colleagues.
4) Many colleagues inside and outside one's own group seemed better than having many colleagues in one place and just a few in the other.
5) Contacts can be useful even to the relatively unsocial scientist.

C. Diversity (126, Ch. 4)

1) Diversity is essential.
2) Scientists may benefit from exposure to administrative duties.
3) Younger scientists should be stimulated to develop several specialized areas.
4) The number of skills and specialties a scientist uses is more important than the number of projects he is assigned.

D. Dedication (126, Ch. 5)

1) Intensity of motivation (morale) was generally stronger among people who were self-directed.
2) Feeling of intense involvement was consistently found among high performers.
3) Measurement of morale is difficult and only an approach was demonstrated.

Summary of Findings from Pelz and Andrews ( )

Table 4

E. Motivations (126, Ch. 6)

1) People interested in furthering their career in a science also showed slightly more performance than average, as opposed to people who are status seekers within an organization.

2) Scientists who relied on their own ideas as a source of motivation were highly effective, whereas those who relied on supervisors for stimulation were below par.

3) Among the high performers, the approach of "broad mapping of new areas" in work was clearly an aid, whereas the desire to "probe deeply in a narrow area" tended to handicap.

F. Satisfactions (126, Ch. 7)

1) Suggestions for rewarding achievement:
   (a) intrinsic to work - new challenge, education, self-direction
   (b) extrinsic to work - good salary, more responsibilities, association with top executives

2) Intrinsic rewards cannot be relied on to motivate achievement, but when achievement occurs, the extrinsic rewards should be consistent; this may possibly stimulate more achievement (dissonance theory).

G. Similarity of Colleagues (126, Ch. 8)

1) Scientists tended to perform better if they named as colleagues individuals from who they differed in strategy of tackling technical problems, and in the style of approach to the work.

2) Similarity or dissimilarity to the immediate chief did not matter.

3) Differences between the scientist and his colleagues provide intellectual jostling, where similarities may supply security.

H. Creativity (126, Ch. 9)

1) Creative ability is not related to performance unless the scientist is in the situation where he can use it. These situations seem to be (a) specializing in an area for a short period of time, (b) being part of a team where coordination was not too high and an opportunity to influence important decision-makers, and (c) having good facilities for communicating new ideas.

<u>Table 2, Cont.</u>

I. Age (126, Ch. 10)

1) Performance declines because of reduced motivation among older scientists.
2) Systematic attention to renewing and broadening one's technical skills is a way of prolonging creativity.

J. Age and Climate (126, Ch. 11)

1) Achievement after age 40 requires self-reliance and willingness to risk the uncertain.
2) The changes in performance of an individual over his life span can vary with the kinds of research climate.

K. Coordination (126, Ch. 12)

1) The looser the situation, the more strongly high levels of motivation (both internal and external in source) accompanied high performance.
2) The individual's autonomy and influence were most effective in situations of only moderate looseness.
3) Maximum autonomy in a very loose setting may isolate the individual from stimulation.

L. Groups (126, Ch. 13)

1) A general decline in scientific contribution is noted as group age increases, and a curvilinear effect for usefulness, peaking at 4 to 5 years.
2) Older groups are less communicative, less competitive, less secretive, and more specialized in their interests.
3) Older groups retain their vitality if they maintained vigorous interaction and "intellectual interests."

they do not restrict their effort to the world of application or of pure science, but maintain an interest in each. Because of these characteristics, effective scientists are not fully in agreement with their organization in terms of their interests. What effective scientists personally enjoy does not necessarily help them to advance in the organizational structure. Effective scientists tend to be motivated by the same kinds of things as their colleagues, but they tend to differ in their work styles and the strategies with which they approach their work. The scenario perhaps a rather complete, consistent picture. The reader should not overlook that the data on which the scenario is based is questionnaire survey collected in 1959. Thus, the findings require validation in both a methodological and a timeliness sense.

One further dimension can be added to the scenario. Andrews and Farris (10) studied the relationship of time pressure and scientific performance. Their data strongly that a sense of time pressure can enhance several qualities of scientific performance. In addition to experiencing the most time pressure, the effective scientists also tended to want relatively large amounts of pressure. The critical aspect appeared to be that desired and actual time pressure should be consistent. Either excess or too little time pressure tended to detract from performance. The time pressures came not only from the project work, but also from active communication with colleagues, and from administrative duties of a limited nature.

Despite the careful analyses, the consistent results, and the apparent internal consistency of the resulting scenario, Pelz and Andrews had data collected at one point in time and had correlation analysis as the only statistical methodology. Hence, it was not possible for them to deduce causality, except through persuasion and "face validity", and all the results were of the "occur together" form. Further, there was reason to suspect that in fact performance preceded the organizational variable for at least some of the relationships. For example, Houton (82) had rather persuasively argued in his 1963 paper for the proposition that 1) getting a "good" work assignment is contingent upon demonstrated competence and 2) demonstrating competence is contingent upon having a "good" work assignment; thus, he suggested causality in both directions.

Fortunately it is not necessary to blindly speculate on the question of the direction of causality. In 1965, Farris (57,58) returned to three of the laboratories, all within the electronics industry, to obtain new information from 151 engineers who had participated in the Pelz-Andrews study. As a result, measures now exist of two points in time and the direction of causality can be examined. Farris concentrated on four measures of output: 1) contribution to the scientific discipline (variable 9.4); 2) usefulness to the organization (variable 9.3); number of patents; and number of reports (variables 9.5 and 9.6). The organizational conditions considered by Farris as occurring with performance were involvement in work, contact with a relatively large number of colleagues, high influence on work goals, diversity of work activities, a high salary, and a large number of subordinates (57, pg. 9).

The empirical findings reported Farris are as follows (57, pp. 13, 14):

1) Engineers who are more involved in their work produce more patents subsequently but, more than that, engineers who are seen as useful and produce more patents, become more involved in their work.

2) Higher performing engineers subsequently received more influence on their work goals. Greater influence on work goals was not followed by increased subsequent performance.

3) Engineers who have greater contact tend to (weak finding) perform better subsequently, and high-performing engineers subsequently come into frequent contact with their colleagues.

4) Greater diversity is followed by higher performance, and higher performance is followed by greater diversity of work activities.

5) Engineers who perform well subsequently are paid more, but there is no evidence that those who get paid more, subsequently perform better.

6) Engineers with more subordinates subsequently perform better but, more than that, engineers who perform well subsequently receive more subordinates.

In summary, the general pattern is that for the six organizational factors and four measures of performance, the predominant sequence is that the factor is followed by performance.

The predominance of the performance-followed-by-factor relationship was stronger than Farris had initially predicted. Research should be directed toward determining more precisely the ways in which a person's performance affects his social-psychological working environment. The consequences of performance should be explicitly considered in subsequent theories and performance must be treated as both an end and a cause of changes in the social-psychological working environment. Further, the implications of this viewpoint for designing and implementing the laboratory reward system should be studied.

C. The Role of Supervisory and Managerial Behaviors

The role of supervisory and managerial behaviors in idea flow was summarized in Section III. Andrews and Farris (10,59) in their 1972 paper provided an initial framework for understanding the impact of supervisory behavior on group performance in R&D. In the paper, performance is a group composite measure made up of input measure how well each individual in the group increased knowledge in his field, extended or refined existing knowledge in his field, contributed to general knowledge in his field, and had been useful in helping his R&D organization carry out its responsibilities. The study was conducted in a NASA research center and focused on 94 non-supervisory scientists who comprised 21 small teams.

The results can be summarized in terms of colleague roles and information flow among group members, between group members and the supervisor, and with persons outside the group. High performing groups tend to be characterized by: technical information and help in thinking occuring among group members; help in thinking, critical evaluations, and administrative help from the supervisor to the group and technical information, help in thinking, critical evaluations, and original ideas from the group to the supervisor; and original ideas from the outside coming to the supervisor and the supervisor going outside the group for administrative help. Conversely, in low performing groups, the following was observed: organizational information flowed among group members; organizational information and original ideas were passed from the supervisor to the group members; the group members went outside the group for help in thinking and administrative help; and the supervisor sought help in thinking and original ideas outside the group and received help in thinking and organizational thinking.

Andrews and Farris note two surprises in their results--so-called because they tended to not be consistent with other organizational literature. First, none of the several measures of supervisory skill in the human relations area related to group performance. Second, markedly negative relationships were found between the supervisor's performance of administrative functions; e.g., planning and scheduling, and his subordinate's performance measures. These surprises as well as the descriptions of roles and information flow are a particularly rich base for additional empirical research.

Drawing on the findings in his earlier book (14), Argyris has analyzed the R&D phase of the innovation process and presented findings regarding how varying degrees of interpersonal competence among top managers influence their, and their organization's, innovatetiveness, willingness to take risks, and problem-solving effectiveness (15). Interpersonal competence is the individual's ability to produce intended effects in such a way that he can continue to do so. Included among the assumptions drawn from the earlier book are: 1) the lower (higher) one goes in the organizational hierarchy, the greater (less) the probability that behavior is controlled by technology, organizational structure, and managerial controls; 2) changes in behavior in organizations must begin with top management; 3) all behavior either adds to, or/and detracts from, interpersonal competence; 4) all behavior can be classified as at either the ideational (intellectual) or feeling (emotional) level; and 5) pyramidal organizations imply a strategy of effective human relationships wherein a) the important human relationships are related to achieving organizational objectives, b) effectiveness increases with rationality and decreases with emotionality, and c) participants can have their energies canalized in the organization's interest by direction, control, and appropriate rewards and penalties. With this as the initial set, three organizations are studied--two R&D organizations and one management consulting firm--by questionnaire, interview, and observation.

Argyris reports some empirical findings which provide insight into organizational behavior in innovative organizations. In one of the R&D organizations, the superiors did not seem to be aware of relevant interpersonal problems and, when made aware of them were unable to solve them or solved them in such a way as to detract from the effectiveness of the problem-solving processes. Low interpersonal competence led to overemphasis on the need for autonomy, equipment, position, space, technicians, meetings, and communication. Also, top management's perceptions of cohesiveness, decision-making effectiveness, openness, and risk taking proved to be wrong--they were over positive. Finally, Argyris reported that subordinates' behaviors tended to be similar to those about which they objected in the superiors.

The most interesting content in Argyris' book is his detailed model (15, pg. 236), and the associated discussion, of R&D deterioration. The deterioration occurs because of low interpersonal competence and despite high technical competence. The model illustrates many feedbacks showing how interpersonal competence is decreased over time. In essence, low interpersonal competence leads to behaviors resulting in lower R&D effectiveness. But, lower R&D effectiveness leads to strong control and directive behaviors by management including getting the R&D client and the customer involved in control and direction. Client and customer involvement tends to deemphasize long-range, risky research and to emphasize short-term customer needs. This emphasis further decreases the effectiveness of the R&D personnel which leads to increased management control and direction. Thus, once the cycle is initiated it feeds itself and leads to increasing deterioration over time.

Argyris' R&D deterioration model sounds plausible and, if true, sounds a pessimistic future for industrial R&D. It is essential that the model be subjected to systemmatic, empirical test. If the model, or some modification thereof, is validated, strategies for breaking the cycle should be proposed and evaluated.

D. Research Opportunities

The variables and relationships cited by Martino are drawn from numerous, typically unrelated studies. His table, as well as Table 1, indicate the unevenness of the literature; i.e., some of the variables have been studied in depth, others have been hardly studied. There is an opportunity to study some of these variables further in order to obtain more even coverage. More importantly, there is a need to integrate the existing literature in order to better understand variable interrelationships.

A scenario of effective scientists was constructed from the Pelz-Andrews findings. A subsequent study by Farris, while it does support the scenario, also raises significant questions regarding the directions of causality. In fact, stronger results are found for the hypothesis that performance improvement results in improved organizational conditions than for the other direction, i.e., the improved organizational conditions result in performance improvement. Replications of the Farris study would be beneficial. The consequences of improved performance, as well as of improved organizational conditions, must be explicitly included in subsequent theories. Such theories should be developed and tested empirically.

The scientific group research of Andrews and Farris and the R&D deterioration model of Argyris are both in the early stages of development and verification. Both have potentially significant implications for enhancing, or stopping the deterioration of, R&D performance. This is a critical area and should be given a high priority.

VI. Transfer of R&D Output Within the Firm

Research and development output can have little benefit in the firm until it has been applied and utilized. In industrial R&D this means that the R&D output must be coupled with sales, marketing, and production. The bridging of the gap between R&D and these other organizational functions may be the single most critical problem in organized innovation. For example, Glennan (103, Ch. 2) states that, with respect to federal development projects, many of the problems surrounding a development appear only during the initial phases of production and many of the most important questions in the selection of development policies revolve around the creation of the production equipment and processes. Mansfield (101, Ch. 6) conducted interviews in 15 chemical, machinery, and electronics firms regarding 38 product-innovations and concluded: 1) the largest percentage of the total cost of innovation generally occurs when tooling is designed and constructed and 2) the stage of the innovation process that generally goes on for the longest period of time is prototype testing or pilot-plan examination. Thus there is some evidence that the transfer of R&D output within the firm is a critical problem.

Unfortunately there is a dearth of literature which addresses this part of the innovation process. After an extensive literature search which includes all issues of IEEE Transactions on Engineering Management and the NASA Management of R&D Literature Search this author was able to find one paper which did more than identify that a problem exists. The paper is a 1963 Harvard Business Review paper by Quinn and Mueller (132). The next three paragraphs summarize that paper.

The insights in this article are based on interviews with over 200 top operating and research executives in the United States. The underlying assumption is that the key problem in research management today (1963) is getting research results effectively transferred into operations. Secondary assumptions include 1) management must understand what interfaces exist and what is involved in transfer across each interface, 2) the R&D program must be targeted to goals and technological needs, 3) top management action can establish a progressive outlook toward technological change, and 4) the exploitation process can be planned and controlled.

Although not formally stated, a number of hypotheses, which could be tested empirically, are implied in the Quinn-Mueller paper. Examples of these hypotheses are.

1. such pressures as rising wages, increasingly complex technologies, and difficulty in obtaining protected market positions are forcing R&D management to improve their management tecaniques.
2. the place where managements fail most drastically in transfer is in the handling of technologies which do not fit existing operating units.
3. technology transfer requires the transfer of information about the technology, enthusiasm for the technology, and authority to use the technology.
4. some of the factors which inhibit the transfer are beyond the control of management.
5. long-range planning provides information useful in targeting research to organizational goals and in smoothing the way for anticipated technological change in operations.
6. management policies set the organizational outlook toward technological change, especially policies related to short-term controls.
7. the most difficult technologies to transfer are those which are totally new to the company.

The interview data tend to support the above hypotheses or result in lists related either to the assumptions or the hypotheses.

It is not possible to state all the possible insights contained in the Quinn-Mueller without approaching the length of the original paper. It does appear that there is no single technology transfer system which is optimal for all companies. Quinn and Mueller conclude that management can drastically improve the flow of technical innovation from R&D into operations by utilizing the following four-step program: 1) examine technological transfer points, 2) provide information to target research, 3) foster a positive motivational environment, and 4) plan and control exploitation of R&D results. Details for implementing such a program are given in the paper. The empirical results and conclusions presented in the paper are based on interview data and must be viewed as insights. However, they are potentially very important and should be researched and tested.

Baker and Freeland (24) also recognized the role that could be played by long-range planning in helping to target R&D to organizational goals. Based on the idea flow model analysis (see Section III) they concluded that there is a requirement for an information system which:

1. results in more consistency in evaluations performed by different reviewers.
2. can be easily updated as the environment and the information base change.
3. has researcher involvement in both input and output.
4. decreases the lag in feedback to idea originators.
5. generates timely need and technical opportunity information.

They conjecture that the type of information which can be generated by long-range planning can be used to initiate such an information system. Figure 4 is a summary of their ideas. Long-range planning inputs specific technical barrier problems, needs, and technical opportunities to the R&D personnel to assist it in idea generation and submission. In addition, long-range planning provides R&D management with long-range objectives which they can convert into decision criteria for R&D project selection and resource allocation. This possible information system should be developed and tested.

While attempting to summarize my frustrations regarding the lack of literature in this area I was reminded of some recent statements by Mansfield (100, pg. 483). His writing so aptly summarized my feelings that I have taken the liberty to close this section with the following quote. "Systematic, in-depth studies of the problems in this area - and the ways in which industry has attempted to solve these problems - would be of considerable use. It is high time to build this aspect of the R&D process into models relating R&D expenditures to productivity increase and economic growth. Also, it should be recognized that a large part of the riskiness of industrial R&D is due to commercial, not technical, uncertainty." "If this is indeed the case, it raises questions concerning the extent to which there is proper coordination between the R&D people, on the one hand, and the marketing and production people, on the other. Detailed and intensive studies should be carried out to shed light on this question, which has received limited - and often superficial - treatment in the past."

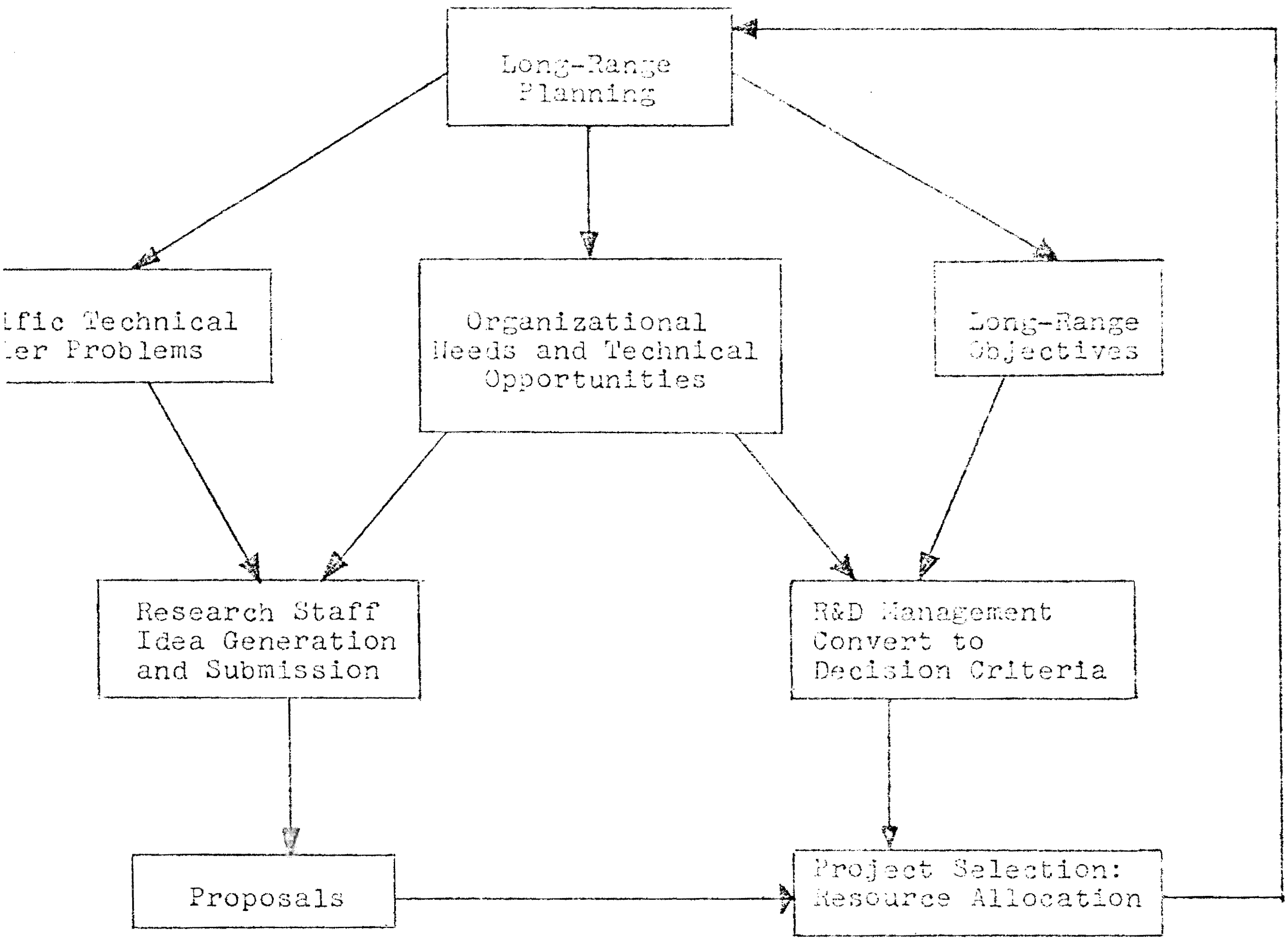


Figure 4: Summary Model of Information System

VII. Research Areas: Knowledge Gaps and Extension Opportunities

A number of research opportunities have been identified in the preceding sections. Included among these opportunities are the following:

A. Section II

1. How is the nation investing its R&D resources?
   Is there an imbalance between basic and applied research?
   What are the implications of such an imbalance, and of the concentration of industrial R&D in a few industries, on such national issues as graduate education, federal sponsorship of R&D, the patent system, etc.?
2. What exogenous factors influence the amount of R&D, the innovativeness of the R&D, and the effectiveness and efficiency of the performance of R&D in the individual firm?
   What are the specific relationships and interrelationships among such factors and with the firm's R&D activity?
3. Is "small-integrated-diversified" a more useful focus than the "bigness/fewness" hypothesis?
   What is the relationship between size of firm and R&D?
   Are internal structure and strategy more important variables than size of the firm?

B. Section III

1. Are significant innovation opportunities lost because R&D personnel are not communicating their ideas to management?
   What organizational factors influence the flow of ideas?
   What structural, strategic, policy, attitudinal, or behavioral changes would help improve the flow?
2. What is the role of information in idea flow?
   Can information systems be designed such as to enhance idea flow?

C. Section IV

1. How do R&D managers and supervisors deal with uncertainty, multiple criteria, crises or breakthroughs, intermittent stream of alternatives, the criticality of specific personnel, limited resources, time-variant R&D and organizational objectives, etc.?
   How do the proposed normative models treat these factors?
   Can improved descriptive knowledge lead to improved normative models?

2. What is the impact of the use of normative models, or of improved information systems, for R&D project selection on the decisions made and on managerial, supervisory, and research behavior?
   Why have many of the implemented R&D project selection models been discontinued?
3. Would a better historical data base lead to more accurate project parameter estimation?
   Can parameter and project interrelationships be quantified and used to improve R&D decisions?
4. Is R&D project selection better modelled as a decision event or a decision process?
   Can the hierarchical decision models be utilized to model the R&D project selection process as a diffuse decision process?
   What implications can be drawn from analysis of the hierarchical models?
5. What extensions can be made to the existing interactive models?
   Will they function well as information systems?
   Can they be used in enhancing the validity of estimates of project benefit?

D. <u>Section V</u>

1. Is the literature of the behavioral science contributions to laboratory management sufficiently advanced to provide an integrated understanding?
   If not, what additional studies are required?
   What structural and policy recommendations will enhance R&D performance?
2. Will replications of the Pelz-Andrews results support their insights regarding the impact of organizational factors on scientific performance?
   What is the direction of causality in specific relationships?
   What is the impact of supervisory behaviors on scientific performance?
3. Is the R&D deterioration model proposed by Argyris descriptive and accurate?
   What can be done to stop the deterioration if the model, or a revision thereof, is proven accurate?

E. <u>Section VI</u>

1. Is there an existing literature which has been overlooked?
2. Are the Quinn-Mueller recommendations still timely?
3. What structural and policy recommendations would aid in the transfer of R&D to the other organizational functions?

Thus, just a listing of the research opportunities identified in each section results in a large number of research opportunities.

In addition to the above, there are research opportunities which cut across the areas covered in the sections of this paper. The methods for evaluation, prior or post, are still inadequate. Section II notes the difficulties in evaluating the contribution of R&D at the national, industry, and firm levels of analysis. Similarly Section IV notes the shortcomings in measuring R&D benefit at the project level. Very little is known regarding how to combine estimates at the project level into estimates for the firm. Empirical studies are essential in order to describe how such estimates are accomplished by R&D managers and to test or evaluate proposed approaches. The primary output of research, although not necessarily development, is information, yet there are few measures which work directly with information as an output. Thus, the whole area of evaluation of R&D is a critical area for continued research.

The normative recommendations in the idea flow, project selection, and transfer of R&D results to other organizational functions sections tend to focus on control and coordination primarily by improved information related to better defined objectives. However, the behavioral studies of Pelz-Andrews, Farris, and Argyris stress the freedom and autonomy are important for improved performance. Are these literatures contradictory? Yes, unless the information systems are designed and utilized with the behavioral results in mind. This implies that the information systems should provide interaction characteristics analogous to those which the behavioral scientists have found to be beneficial. Thus, it is critical that the behavioral knowledge be extended and validated so that it can be utilized in the design of information systems and that first generation information systems be designed and evaluated.

Project selection decisions have an impact on both idea flow and performance behavior in R&D. However, the relationships are not well known. Further, normative models have not explicitly recognized these relationships. Also, the direct impact on managerial behavior or decisions is not known. With all the pressure to improve R&D management and with the increased apparent utilization by industry of the normative models, this is also a critical area of study.

A valid summary of the R&D literature is that there is a great deal of literature providing speculation and normative recommendation, but little providing empirical knowledge. What empirical knowledge exists is generally non-integrating, i.e., it does not build and integrate the previous literature. Research is essential which provides integration.

B I B L I O G R A P H Y

1. Abernathy, W.J. and R.S. Rosenbloom, "Parallel and Sequential R&D Strategies: Application of a Simple Model," IEEE Trans. on Engin. Mgmt., Vol. EM-15, No. 1, March 1968, pp. 2-10.

2. Abramowitz, M., Amer. Econ. Rev. 46, 5 (May 1956).

3. Alboosta, Chester A. and Holzman, Albert G., "Optimal Funding of an R and D Project Portfolio", presented at 11th Institute of Management Science Meeting, Los Angeles, Cal., (Oct. 1970).

4. Allen, T.J., "The diffusion of technological information," in Proc., 20th Nat. Conf. Administration of Research (Denver Res. Inst., Univ. Denver, Denver, Colo.), pp. 111-120, 1967.

5. Allen, T.J., Technol. Rev. 70, 1 (1967); ____ and S.I. Cohen, Adm. Sci. Quart. 14, 1 (1969).

6. Allen, T.J., in Annual Review of Information, Science, and Technology, C.A. Cuadra, Ed. (Encyclopedia Britannica, Chicago, 1969), p. 3.

7. Allen, T.J., R & D Manage. 1, 1 (1971).

8. Andrew, G.H.L., "Assessing Priorities for Technical Effort", Operational Research Quart., Vol. 5, pp. 67-80, (Sept. 1954).

9. Andrews, F.M., "Factors affecting the manifestation of creative ability by scientists," J. Personal., vol. 33, pp. 140-152.

10. Andrews, F.M., and G.F. Farris, "Time Pressure and Performance of Scientists and Engineers: A Five Year Panel Study", Organizational Behavior and Human Performance, Vol. 8, No. 2, Oct. 1972, pp. 185-200.

11. "Annual Report, 1963-64," Program of Research on the Management of Research and Development, Department of Industrial Engineering and Management Sciences, Northwestern University, Evanston, Illinois, July 1964.

12. Ansoff, H.I., "Evaluation of Applied Research in a Business Firm", Technological Planning on the Corporate Level, J.R. Bright, Ed., Proc. Conf. at Harvard Business School, Harvard University, Cambridge, Mass., pp. 209-224, (1962).

13. Anthony, Robert N., Planning and Control Systems, Harvard University Press, 1965.

14. Argyris, Chris, Integrating the Individual and the Organization, John Wiley & Sons, Inc., N.Y., N.Y., 1964.

15. Argyris, Chris, Organization and Innovation, Richard D. Irwin and the Dorsey Press, Homewood, Illinois, 1965.

16. Army Materiel Command Seminar on Value Measurement, AMC Value Methodology Experiments; Session at Airlie House, Department of Industrial Engineering and Management Sciences, Northwestern University, Evanston, Ill. (Sept. 1970).

17. Asher, D.I., "A Linear Programming Model for the Allocation of R and D efforts, IRE Transactions on Engineering Management, Vol. EM-9, pp. 154-157, (Dec. 1962).

18. Atkinson, Anthony C. and Bobis, Arthur H., "A Mathematical Basis for the Selection of Research Projects", IEEE Transactions on Engineering Management, Vol. EM-16, No. 1, (Feb. 1969).

19. Avery, R.W., "Technical Objectives and the Production of Ideas," Presented at MIT Industrial Liaison Program Seminar on the Organization of R and D in Decentralized Companies, 1959, p. 22.

20. Avery, R.W., "Enculturation in Industrial Research," IRE Transactions on Engineering Management, EM-7 (1960), No. 1, pp. 20-24.

21. Ayers, Robert U., Technological Forecasting and Long-Range Planning, McGraw-Hill Book Company, New York, 1969.

22. Baker, N.R., "The influence of several organizational factors on the idea generation and submission behavior of industrial researchers and technicians," Ph.D. dissertation. Dept. Industrial Engineering and Management Sciences, Northwestern University, Evanston, Ill., June 1965.

23. Baker, N.R., and J.R. Freeland, "Recent Advances in R&D Value Measurement," invited paper at 41st National Meeting of ORSA, New Orleans, La., April, 1972.

24. Baker, N.R. and J.R. Freeland, "Structuring Information Flow to Enhance Innovation", Management Science, Vol. 19, No. 1, Sept. 1972, pp. 105-115.

25. Baker, N.R. and W.H. Pound, "R and D Project Selection: Where We Stand", IEEE Transactions on Engineering Management, Vol. EM-11, pp. 124-134 (Dec. 1964).

26. Baker, N.R., Shumway, C.R., Maher, P.M., Souder, W.E., and A.H. Rubenstein, "An R and D Project Selection and Budget Allocation Model for Large, Decentralized Organizations", Program of Research of the Management of R and D, Northwestern University, (January 1972).

27. Baker, N.R., Siegman, J. and J. Larson, "The Relationship Between Certain Characteristics of Industrial Research Proposals and Their Subsequent Disposition", IEEE Trans. on Engin. Mgmt., Vol. EM-18, No. 4, Nov. 1971, pp. 118-123.

28. Baker, N., Siegman, J. and A.H. Rubenstein, "The Effects of Perceived Needs and Means on the Generation of Ideas for Industrial R&D Projects", IEEE Trans. on Engin. Mgmt., Vol. EM-14, No. 4, Dec. 1967, pp. 156-163.

29. Beged-Dov, A.G., "Optimal Assignment of R and D Projects in a Large Company Using an Integer Programming Model." IEEE Transactions on Engineering Management, Vol. EM-12, pp. 138-142, (December 1965).

30. Bell, D., Chilcott, J. Read, A. and R. Salway, Linear Programming for Resource Allocation, Report RD/H/R2-3, Central Electricity Generating Board, London, England (September 1967).

31. Bender, A.D. and Edmund B. Pyle, "Planning, Control, and Resource Allocation Models in R and D, A Performance Review By a Pharmaceutical Manufacturer", XIX International Meeting of TIMS, Houston, Texas, (April, 1972).

32. Blau, P.M., The Dynamics of Bureaucracy, University of Chicago Press, 1963, especially pp. 127-130.

33. Bralley, J.A., "Environment for Creativity," Research Management, Vol. 3, (1960), No. 2.

34. Brandenberg, R.G., "Project Selection in Industrial R&D: Problems and Decision Processes", in M.C. Yovits (ed.), Research Program Effectiveness, Gordon and Breach, 1966.

35. Bright, J.R., "On the Appraisal of Risks in Technological Innovation", in *Research, Development and Technological Innovation*, R.D. Irwin, Inc., Homewood, Illinois, 1964.

36. Brown, M. and A. Conrad, in *The Theory and Empirical Analysis of Production*, M. Brown, ed. (Columbia Univ. Press, New York, 1967), pp. 341-371.

37. Capron, W., *Amer. Econ. Rev.* 56, 508 (May 1966).

38. Carter, C.F. and B.R. Williams, *Industry and Technical Progress: Factors Governing the Speed of Application of Science* (Oxford University Press, London, 1957).

39. Cetron, M.J., Martino, J. and L. Roepcke, "The Selection of R and D Program Content - Survey of Quantitative Methods," *IEEE Transactions on Engineering Management*, Vol. EM-14 (March 1967), pp. 4-13.

40. Charnes, A., Clower, R.W. and Kortanek, K.O., "Effective Control Through Coherent Decentralization with Preemptive Goals", *Econometrica*, Vol. 35, No. 2 (April, 1967).

41. Charnes, A. and A.C. Stedry, "Chance-Constrained Model for Real-Time Control in Research and Development Management," *Management Science*, Vol. 12, No. 8, pp. B353-B362, (July 1964).

42. Chollar, R.G. *et al*., "Creativity techniques in action," *Research Management*, vol. 1, no. 1, pp. 5-22, 1958.

43. Churchman, C.W. and R.L. Ackoff, "An Approximate Measure of Value", *Operations Research*, Vol. 2 (1954), pp. 172-180.

44. Churchman, C.W., Ackoff, R.L., and E.L. Arnoff, *Introduction to Operations Research*, John Wiley, (1957).

45. Cochran, Michael A., Pyle, Edmund B. III, Greene, Leon C., Clymer, H.A. and Douglas Bender, "Investment Model for R and D Project Evaluation and Selection", *IEEE Transactions on Engineering Management*, Volume EM-18, No. 3, (August 1971).

46. Cohen, A.R., "Upward Communication in Experimentally Created Hierarchies," *Human Relations*, Vol. II (1958), No. 1, pp. 41-53.

47. Connolly, T., "The Diffuse Decision: An Integrating Focus", invited paper at Joint TIMS/ORSA/AIIE Meeting, Atlantic City, N.J., Nov. 1972.

48. Cramer, R.H., and B.E. Smith, "Decision Models for the Selection of Research Projects," The Engineering Economist, Vol. 9, pp. 1-20, (January-February 1964).

49. Dantzig, George B. and P. Wolfe, "The Decomposition Algorithm for Linear Programming," Econometrica, Vol. 29, No. 4, (Oct. 1961).

50. Dean, B.V. and M.J. Nishry, "Scoring and Profitability Models for Evaluating and Selecting Engineering Projects," Operations Research, Vol. 13, No. 4, pp. 550-570 (July-Aug. 1965).

51. Denison, E.F., The Sources of Economic Growth in the United States (Committee for Economic Development, New York, 1962).

52. Disman, S., "Selecting R & D Projects for Profit," Chem. Engrg., Vol. 69, pp. 87-90 (December 1962).

53. Eckenrode, R.T., "Weighting Multiple Criteria," Management Science, Vol. 12, No. 3 (November 1965).

54. Eiduson, B.T., "Productivity rate in research scientists," Amer. Sci., vol. 64, no. 1, pp. 57-63, 1966.

55. Einhorn, Hillel J., "The Use of Non-Linear, Noncompensatory Models in Decision Making," Psychological Bulletin, Vol. 73, No. 3, (1970).

56. Fabricant, S., Economic Progress and Economic Change (34th annual report of the National Bureau of Economic Research, Princeton Univ. Press, Princeton, N.J., 1954).

57. Farris, G.F., "Some antecedents and consequences of scientific performance," IEEE Trans. Eng. Manag., vol. EM-16, pp. 9-16, Feb. 1969.

58. Farris, George F., "Organizational Influence and Individual Performance: A Longitudinal Study," Journal of Applied Psychology, Vol. 53, No. 2, Part 1, April 1969, pp. 87-92.

59. Farris, G.F., "The Effect of Individual Roles on Performance in Innovative Groups", R&D Management, Vol. 3, No. 1, 1972, pp. 23-28.

60. Freeland, J.R., and W.R. Baker, "Mathematical Models of Resource Allocation Decision Making in Hierarchical Organizations", invited paper at Joint ORSA/TIMS/AIIE Meeting, Atlantic City, N.J., Nov. 1972.

61. Fishburn, P.C., Decision and Value Theory. New York: Wiley, (1964).

62. Galbraith, J.K., American Capitalism: The Concept of Countervailing Power, Houghton Mifflin, Boston, 1952.

63. Gargiulo, G.R., Hannoch, J., Hertz, D.B., and Zang, T. "Developing Systematic Procedures for Directing Research Programs," IRE Transactions on Engineering Management, Vol. EM-8, pp. 24-29, (March 1961).

64. Geoffrion, A.M., Dyer, J.S., Feinberg, A., "An Interactive Approach for Multi-Criterion Optimization With an Application to the Operation of an Academic Department," Working Paper No. 176 Western Management Science Institute, UCLA, (1971).

65. Gershinowitz, H., "Sustaining Creativity Against Organizational Pressures," Research Management, Vol. 3 (1960), No. 1.

66. Goldhar, J.D., An Exploratory Study of Technological Innovation (Doctoral dissertation, George Washington University, Washington, D.C., 1970).

67. Goodwin, Paul G., "A Method for Evaluation of Subsystem Alternative Designs," IEEE Transactions on Engineering Management, Vol. EM-19, No. 1 (February 1972).

68. Gordon, G., "The Organization Designed to Produce Change", presented at the Seminar on Innovative Organization, Univ. Chicago, Chicago, Illinois, Apr. 1964.

69. Gordon, G. "A paradox of research administration," in Proc., 20th Nat. Conf. Administration of Research (Denver Research Inst., Univ. Denver, Denver, Colo.), pp. 15-25, 1967.

70. Gordon, G., and S. Marquis, "Resources and scientific innovation," Presented at the Annu. Meet. American Sociological Association, Montreal, Que., Canada, Aug. 1964.

71. Gordon, G. and S. Marquis, "Freedom, visibility of consequences, and scientific innovation," Amer. J. Sociol, vol. 72, pp. 195-202.

72. Griliches, Z., Amer. Econ. Rev. 54, 961 (December 1964).

73. Haefle, J.W., Creativity and Innovation, New York: Reinhold, 1962.

74. Hamberg, D., R&D: Essays on the Economics of Research and Development, Random House, Inc., New York, 1966.

75. Hennipmann, P., "Monopoly: Impediment or Stimulus to Economic Progress?", in E.H. Chamberlain (ed.), Monopoly and Competition and their Regulation, MacMillan and Co., London, 1954.

76. Hertz, D.B., "Risk Analysis in Capital Investment," Harvard Business Review, Vol. 42 (1964) , pp. 95-106.

77. Hespos, R.E., and P.A. Strassman, "Stochastic Decision Trees for the Analysis of Investment Decisions," Management Science, Vol. 11, No. 10, Aug. 1965, pp. 244-259.

78. Hess, S.W., "A Dynamic Programming Approach to R & D Budgeting and Project Selection," IRE Transactions on Engineering Management, Vol. EM-9, pp. 170-179 (December 1962).

79. Hill, S.C., "A natural experiment on the influence of leadership behavior patterns on scientific productivity," IEEE Trans. Eng. Manag., vol. EM-17, pp. 10-20, Feb. 1970.

80. Hillier, J., "A Theory of Communication in a Research Laboratory" Research Management, Vol. 3 (1960), No. 4, pp. 255-270.

81. Hitch, C., "The Character of Research and Development in a Competitive Economy," RAND Corporation Paper P-1247-1309, May 1958.

82. Houton, F.W., "Work Assignment and Interpersonal Relations in a Research Organization: Some Participant Observations," Administrative Science Quarterly, Vol. 7 (1963), No. 4, pp. 502-521.

83. Jewkes, J., D. Sauers, and R. Stillerman, The Sources of Invention, MacMillan and Co., London, 1958.

84. Jones, S.L., and J.E. Arnold, "The Creative Individual in Industrial Research," IRE Transactions on Engineering Management, EM-9 (1962), No. 2, pp. 51-55.

85. Kaplan, A.D.H., Big Enterprise in a Competitive System, Brookings Institution, Washington, 1954.

86. Kaplan, N., "Some Organizational Factors Affecting Creativity," IRE Transactions on Engineering Management, EM-7 (1960), No. 1, pp. 24-30.

87. Kendrick, J. Productivity Trends in the United States (National Bureau of Economic Research, Princeton Univ. Press, Princeton, N.J., 1961).

88. Klein, B., and W. Meckling, "Application of Operations Research to Development Decisions," Operations Research, May 1958.

89. Kornhauser, W., Scientists in Industry: Conflict and Accomodation, University of California Press, Berkeley and Los Angeles, 1962, p. 64.

90. Kuhn, T.S. and N. Kaplan, "Environmental conditions affecting creativity," in C.W. Taylor, The Third (1959) University of Utah Research Conference on the Identification of Creative Scientific Talent. Salt Lake City, Utah: University of Utah Press, 1959, pp. 313-316.

91. Langrish, S., R & D Manage. 1, 3 (1971).

92. Lehman, H.C., "The chemist's most creative years," Science, vol. 127, no. 3308, pp. 1213-1222.

93. Lehman, H.C., Age and Chievement. Princeton, N.J.: Princeton University Press, 1953.

94. Luce, R. Duncan and Howard Raiffa, Games and Decisions, Wiley, New York, (1957).

95. MacLaurin, W.R., "New Products Innovation and Introduction: Their Broad Implications," in A.H. Rubenstein (editor), Coordination, Control, and Financing of Industrial Research, King Crown Press, New York, 1955, pp. 34-41.

96. Maher, P.M., "Results From an Experiment with a Computer Based R & D Project Selection Technique," XIX International Meeting of TIMS, Houston, Texas, (April 1972).

97. Mansfield, E., The Economics of Technological Change, W.W. Norton Co., Inc., New York, 1968.

98. Mansfield, E., Industrial Research & Technological Innovation, W.W. Norton & Co., Inc., New York, 1968.

99. Mansfield, E., Technological Change, W.W. Norton Company, Inc., New York, 1970.

100. Mansfield, E., "Contribution of R&D to Economic Growth in the United States", Science, Vol. 175, No. 5021, Feb. 1972, pp. 477-486.

101. Mansfield, E., J. Rapoport, J. Schnee, S. Wagner, and M. Hamburger, Research and Innovation in the Modern Corporation, W.W. Norton & Company, Inc., New York, 1971.

102. March, J.G., and H.A. Simon, Organizations, John Wiley and Sons, Inc., New York, 1958, pp. 154, 185.

103. Marschak, T., T.K. Glennan, and R. Summers, Strategy for R&D: Studies in Microeconomics of Development, Springer-Verlag Inc., New York, 1967.

104. Marcson, S., The Scientist in American Industry, Harper and Brothers, New York, 1960, p. 113.

105. Marquis, D.G., and T.J. Allen, "Communication patterns in applied technology," Amer. Psychol., vol. 21, pp. 1052-1060.

106. Martino, J.P., "A Survey of Behavioral Science Contributions to Laboratory Management," IEEE Trans. on Engin. Mgmt., Vol. EM-20, No. 3, pp. 68-75.

107. McPherson, J.H., "How to manage creative engineers," Mechanical Engineering, pp. 32-36, February 1965.

108. Meltzer, L., "Scientific productivity in organizational settings," J. Soc. Issues, vol. 12, no. 2, pp. 32-40.

109. Meltzer, L., and J. Salter, "Organizational structure and the performance and job satisfaction of physiologists," Amer. Sociol. Rev., vol. 27, pp. 351-362.

110. Merton, R.K., "Fluctuations in the Rate of Industrial Invention", Quarterly Journal of Economics, Vol. 49, May 1935, pg. 454.

111. Miller, James R., Professional Decision-Making, Praeger Publishers, (1970).

112. Minasian, J., Amer. Econ. Rev. 59, 30 (May 1969).

113. Moore, J.R. and N.R. Baker, "An Analytical Approach to Scoring Model Design - Application to Research And Development Project Selection," IEEE Transactions on Engineering Management, Vol. EM-14, No. 1, (March 1967).

114. Moore, J.R. and N.R. Baker, "Computational Analysis of Scoring Models for R&D Project Selection," Management Science, Vol. 16, (December 1969), pp. B212-B232.

115. Morris, J.M., "Administration of Research in Industry," Research Management, Vol. 5 (1962), No. 4.

116. Motley, C.M., and R.D. Newton, "The Selection of Projects for Industrial Research," Operations Research, Vol. 7, pp. 740-751, (November-December 1959).

117. Mueller, W.F., in The Rate and Direction of Inventive Activity: Economic and Social Factors, R.R. Nelson, Ed. (Princeton University Press, Princeton, 1962), p. 323.

118. Mullins, G.J., "Prediction of creativity in a sample of research scientists," IEEE Trans. Eng. Manag., vol. EM-10, pp. 52-57, June 1963.

119. Myers, S., and D.G. Marquis, "Successful Commercial Innovations," National Science Foundation NSF 69-17 (1969).

120. National Science Foundation, Research and Development in Industry, 1968 (Government Printing Office, Washington, D.C. 1970), p. 9.

121. Nelson, R.R., M.J. Peck, E.D. Kalachek, Technology, Economic Growth, and Public Policy (Brookings Institution, Washington, D.C., 1967).

122. Nutt, A.B., "An Approach to Research and Development Effectiveness", IEEE Transactions on Engineering Management, pp. 103-112, (September 1965).

123. Nutt, A.B., "Some Considerations in Implementing an R & D Resources Allocation System," XIX International TIMS Meeting, Houston, Texas, (April 1972).

124. Nutter, G.W., "Monopoly, Bigness, and Progress," Journal of Political Economy, Vol. 64, Dec. 1956, pp. 520-527.

125. Parnes, S.J. and H.F. Harding, A Source Book for Creative Engineering. New York: Scribner, 1962.

126. Pelz, D.C., and F.M. Andrews, Scientists in Organizations. New York: Wiley, 1966.

127. Pessemier, E.A. and R. Teach, "A Single Subject Scaling Model Using Judged Distances Between Pairs of Stimuli," Institute for Research in Behavioral, Economic and Management Sciences, Paper No. 143 and 282, Krannert Graduate School of Industrial Administration, Purdue University (1966).

128. Pessemier, E.A. and N.R. Baker, "Project and Program Decisions in Research and Development," R&D Management, Vol. 2, No. 1, (October, 1971).

129. Peters, D.H. and E.B. Roberts, "Unutilized Ideas in University Laboratories," unpublished paper, MIT Sloan School, Sept. 1967.

130. Quinn, J.B., Yardsticks for Industrial Research, The Ronald Press, New York, 1959.

131. Quinn, J.B., Harvard Business Review, March-April 1960, pg. 70.

132. Quinn, J.B., and J.A. Mueller, "Transforming Research Results to Operation", Harvard Business Review, Jan.-Feb. 1963, pp. 49-66.

133. Roe, A., "The psychology of scientists," in The Management of Scientists, Karl Hill, Ed., Boston, Mass.: Beacon, 1964.

134. Roepcke, L.A., Rafert, W.E. and R.P. Benedict, "Army Materiel Command Planning: Principles and Philosophy," IEEE Transactions on Engineering Management, (December, 1968).

135. Rosen, E.M., and W.E. Souder, "A Method for Allocating R & D Expenditures," IEEE Transactions on Engineering Management, Vol. EM-12, pp. 87-93 (September, 1965).

136. Rubenstein, A.H., "Setting Criteria for R & D," Harvard Business Review, pp. 95-104 (January-February, 1957).

137. Rubenstein, Albert H. and Richard Hannenberg, "Idea Flow and Project Selection in Several Industrial Research and Development Laboratories," Proceedings of the Conference on Economic and Social Factors in Technological Research and Development, Ohio State University, 1962.

138. Rubenstein, Albert H., "Studies of Idea Flow in Research and Development," Presented to the New York Chapter of the Institute of Management Sciences, November 1963.

139. Rubenstein, Albert H., "Organizational Factors Affecting Research and Development Decision-Making in Large Decentralized Companies," Management Science, July 1964.

140. Ruefli, T.W., "A Generalized Goals Decomposition Model," Management Science, (April, 1971).

141. Scherer, F.M., "Government Research and Development Programs", in R. Dorfman (ed.), Measuring Benefits of Government Investments. Nov. 1963.

142. Schmookler, J., "Bigness, Fewness, and Research," Journal of Political Economy, Vol. 67, Dec. 1959, pp. 628-635.

143. Schumpeter, J.A., Capitalism, Socialism, and Democracy. Harper and Brothers, New York, 1942.

144. Scott, B.R., "The Industrial State: Old Myths and New Realities", Harvard Business Review, March-April 1973, pp. 133-148.

145. Sherwin, C.W., and R.S. Isenson, Science 156, 3782 (1967).

146. Sigford, J.V. and R.H. Parvin, "Project PATTERN: A Methodology for Determining Relevance in Complex Decision Making," IEEE Transactions on Engineering Management, EM-12, (March, 1965).

147. Smith, W.R., "Favorable and unfavorable working conditions reported by scientists at two research centers," in C.W. Taylor, The Third (1959) University of Utah Research Conference on the Identification of Creative Scientific Talent. Salt Lake City, Utah: University of Utah Press, 1959.

148. Sokov, R., Rev. Econ. Statist. 39, 312 (August 1957).

149. Souder, W.E., "Comparative Analysis of R&D Investment Models", AIIE Transactions, Vol. 4, No. 1, March 1972, pp. 57-64.

150. Souder, W.E., "A Scoring Methodology for Assessing the Suitability of Management Science Models," Management Science, Vol. 18, No. 10, June 1972, pp. B526-B543.

151. Stein, M.I. and S.J. Heinze, Creativity and the Individual. Glencoe, Ill: Free Press, 1960.

152. Storer, N.W., "Research Orientations and Attitudes Toward Teamwork," IRE Transactions on Engineering Management, EM-9 (1962), no. 1, pp. 29-33.

153. Tannenbaum, M., et. al., "Report of the Ad Hoc Committee on Principles of Research - Engineering Interaction," Nat. Acad. Sci. - Nat. Res. Council Materials Advisory Board, MAB 222-M (1966).

154. Taylor, C.W., Ed. Creativity: Progress and Potential. New York: McGraw-Hill, 1964.

155. Terleckyj, N., thesis, Columbia University (1959).

156. Utterbach, J., "Summary of Variables and Propositions Concerning Idea Flow," Working Paper, Department of Industrial Engineering and Management Sciences, Northwestern University, Evanston, Illinois, June, 1964.

157. Utterback, J.M., The Process of Innovation in Instrument Firms (Doctoral dissertation, MIT, Sloan School of Management, Cambridge, 1969).

158. Utterback, J.M., "Innovation in Industry and the Diffusion of Technology", Working Paper, Graduate School of Business, Indiana University, Bloomington, Indiana, June 1973.

159. Villard, H.H., "Competition, Oligopoly, and Research," Journal of Political Economy, Vol. 66, Dec. 1958, pp. 483-97.

160. Vollmer, H.M., "Application of the behavioral sciences to research management," Stanford Res. Inst., Stanford, Conn., Rep. AFOSR-64-2555, 1964.

161. Wallmark, J.T., and B. Sellerberg, "Efficiency vs. size of research teams," IEEE Trans. Eng. Manag., vol. EM-13, pp. 137-142, Sept. 1966.

162. Weitzman, M., "Iterative Multilevel Planning with Production Targets," Econometrica, Volume 38, No. 1 (January, 1970).

163. Williamson, M., "Role of the Technical Staff in Product Innovations," Research/Development, Vol. II (1960), No. 9.

G-43-602

October 17, 1973

MEMORANDUM

TO: Mary Ellen Mogee and Project Consultants

FROM: Patrick Kelly

This is the first draft of Kelly's paper dealing with the initial process phase, "Problem Definition and Idea Generation." You should have previously received the first drafts of papers by Kranzberg, Rossini and Baker. Tarpley on "First Commercial Application" and Mitzner on "Diffusion" are now being typed and will be in the mail to you shortly.

As before, be critical. I look forward to receiving your reactions.

Thanks,

The Process of Technological Innovation

Phase I

Problem Definition and Idea Generation

F I R S T D R A F T

Patrick Kelly

Department of Social Sciences
Georgia Tech

Phase I: Problem Definition and Idea Generation

I. Introduction

There is a pedestrian tunnel under a busy street near the campus which is so famous for the richness and diversity of the inscriptions on its walls that the students have dubbed it "Graffiti 101". Recently however it was painted a glistening white. An environment for creativity to be sure. And these white walls were cristened with the following small and carefully labelled inscription:

> And God said, "Let there be White."
> And it was done.
> And God said, "Let there be Ink to
> divide the white and give it meaning.
> And it was done.
> And God saw these things, that they were good.
>
> Amen
> (Anonymous)

A problem defined, an idea generated. But what has this to do with the process of technological innovation? Very little really, and yet quite a lot. First, it is a reminder that creativity (which we shall take as synonomous with problem definition and idea generation) within the process of technological innovation is but a sub-set of creativity as a much broader and more pervasive human phenomenon. Thus the clues to its understanding are where you find them; a reminder that, as we shall see, is needed to correct a perhaps overly narrow empirical focus. Such a restricted focus is often characteristic of an emerging research interest, which the study of creativity is, both within and without the technological context. But the understanding of any phenomenon requires more than the accumulation of data points. It requires also theory, and as we shall see to date there is little.

Thus while our main concern will certainly be the manner in which problems come to be defined and ideas generated within the process of technological innovation, we shall be drawing on a wider range of sources to shed light upon this topic.

Secondly the above example of creativity in graffiti is a heuristic device in a double sense. Not only does it serve as a lead-in to several introductory points, it also illustrates one of them. Namely, that this essay will make use of several such heuristic mechanisms. Such mechanisms are important for several reasons. First there is the disparate nature of the relevant scholarly materials as has already been alluded to. Tying these together in a logical fashion is a difficult matter. Secondly there is the paucity of theories or models which would, themselves, provide such structure. Thirdly the situation is further complicated by the complex interplay of variables from several environmental levels, including the individual researcher, his primary social and work groups, his laboratory, firm and industry, and the broad contexts formed by existing technology and level of social usage and demand. To take these as separate foci is a static approach to a dynamic situation. One is therefore torn between the impulse to force these complexities into some speciously logical procrustean bed on the one hand, and to simply catalogue and comment on each piece of data on the other. Neither approach would serve us well. Rather, we shall attempt to create a series of nexus or clusters that will relate various empirical studies with such theories and models as exist and both of these with the several levels of environmental variables. Heuristic devices, as the one offered above, will be of little value in themselves, and very often will relate the obvious. But, then, it is often the obvious that needs to be said. Hopefully, however, they will also prove suggestive in the manner in which they tie certain insights together. In any case, it seems appropriate to utilize heuristic devices as support for the structural elements of an essay primarily concerned with heuristics.

The third point implicit in our piece of creative graffiti concerns the relative contributions of the creative individual and the enabling environment to the creative activity. If one takes the anthropologist's triad of opportunity, need, and genius (1) as the necessary factors, the score would seem to be 2 to 1 in favor of the environment. Though hardly on this basis, the primary emphasis in this essay will be on the environmental or contextual factors. There are several reasons for this decision. First, though it is doubtless true that dispositions, personality traits, inherited capacity, and other stable, long-term factors are important, the more immediate rather these more remote determinants of creative performance are obviously more susceptible to alteration and control. In addition, as Ray Hyman has pointed out, "Only after we know the range within which we can change performance by these more immediate inputs can we adequately study the contributions of the more remote factors." (2) Secondly, and this is rather closely related to the first point, we are concerned in this essay with the possibility of induced changes in the level of creative behavior. That is, we are concerned primarily with creative behavior as a phenomenon which can change or be changed, rather than as something which individuals possess in varying degrees as a fixed property.

Finally, this essay will be biased in the direction of environmental factors as a result of the thrust of the project of which it is a part. The forms of creative behavior with which we are concerned are those that occur in the context of the process of technological innovation. And while more than half of the inventions studied by Jewkes et al (3) could be identified with a particular individual, and thus "It does undeniably still make sense in the twentieth century to talk about 'the independent inventor',....(the term) 'the independent innovator' is almost a contradiction in terms." (4) As Langrish et al go on to point out:

> For technological innovation to occur, there must be some interaction between a set of ideas and an institution; the ideas must be interpreted in terms of a need of the institution and put into effect by it. Innovation is almost by definition a corporate and collaborative effort, and it is correspondingly difficult to disentangle the roles played by particular individuals (5).

For these reasons our major concern will be with those environmental influences, at various levels, that impinge upon the creative process. The range of these variables is sufficiently wide, however, to necessitate the introduction of a number of psychological and social psychological considerations.

The above points exhaust the immediately relevant consideration to be drawn from our first heuristic device, the anonymous bit of graffiti. We might remark in passing, however, that scrawled beneath it was a creative addition prompted by the first. It read: "And on the seventh day, God had a picnic." Perhaps such a celebration of creativity is as appropriate as rest.

The next cluster or nexus of preliminary considerations is to be found in the following quote:

> The historians of the future may well select the development of deliberate creativeness as the most important development of this century. We have passed through the age of random creativeness and are entering an age of deliberate creativeness. (6)

Whatever the particular nature of the creative act, technological or otherwise, it is a truism that it does not take place in a vacuum, but is always embedded in a context with particular and describable characteristics. This obvious fact leads to an equally obvious question; "Might not some environments be more conducive to creative effort than others.?" Once this question is posed, there can begin a process of specification with speculative, empirical, theoretical, and quite pragmatic elements. If some environments are more conducive to creative effort than others, what are the relevant variables? How do these variables individually and in concert facilitate or inhibit creative tendencies? How may these variables be altered? What are the consequences of such alterations? Etc. In short, once the environment of creativity was taken seriously, and the implications of attempting to modify and control it were seen, the notion of deliberate creativeness was born. The date of birth is not agreed on. In the quote above the 20th century is suggested. Alfred North Whitehead places it earlier when he says, "the greatest invention of the nineteenth century was the invention of the method of invention." (7) Langrish _et al_ would place it, at least in its speculative form, much earlier in the vision of Salomon's House in Bacon's _New Atlantis_.

> Here was a national research institute in prototype, lavishly equipped by Bacon's imagination with all the equipment and facilities he could think of that might conceivably be of use. Among the thirty-six fellows of the foundation, there was well-defined division of labor and allocation of tasks. The program was clearly intended to be a corporate one. All the principal inventors were, it is true to be commemorated by statues - 'some of iron, some of silver, some of gold'; but Bacon does seem to have placed his trust more in his system than in exceptional individuals. The lame man who keeps to the right road, he pointed out, outstrips the runner who takes a wrong one. He even went on to claim that 'the course I propose for the discovery of sciences is such as leaves but little to the acuteness and strength of wits but places all wits and understandings nearly on a level'. (8)

Bacon thus dreamed of a movement beyond the fortuitous and seemingly random acts of individual creative genius.

But the large-scale effort to actualize this vision of deliberate creativeness was to await the development of the R and D lab. And even now, with some years of experience behind us it is clearly a transition still in progress. And even our view of the progress achieved may be distorted by the nature of case studies, which continues to be the dominant empirical approach. "The retrospective nature of...(these) sources probably means that the process has been viewed as much more rational and well-ordered than it is in fact." (9) We will return to this point in a later section.

That there is still a significant black box, i.e., that the transition from random to deliberate creativeness is far from complete, or completely understood, will be documented in that which follows. But that is only a part of our task. We will also be examining and assessing the base of knowledge that makes the process more deliberate and more subject to rational control. And in this examination and assessment we will be tough-minded and careful but not rigid. That is, we will not limit our considerations to those items the research community seems sure of. On the contrary, we will also examine points that can only properly be classified as folklore, speculation, and unexamined assumptions. In fact, some of these items may prove to be quite fecund in their implications for further research. To be as fair as possible, to the reader and the demands of the assessment task, the nature of each data point will be made as explicit as possible, as will the evidence or support it enjoys.

The assessment of the state-of-the-art understanding of deliberate creativeness, or problem definition and idea generation as this process phase is commonly called in the context of technological innovation, will be conducted in terms of the following questions:

1. What is the current state-of-the-art?

2. How good is our current understanding?

3. What are the major gaps and weaknesses in our knowledge, and what further research is needed to fill them?

4. How adequate are the various commonly employed research methodologies to the needs of future research?

The operational significance of all but the second of these questions should be clear. Question Two is understood to include the following dimensions:

1. What is the state of our theoretical understanding?

   (1) What is the extent of theoretical agreement and disagreement?
   (2) What are the major gaps and weaknesses in our theoretical understanding?
   (3) What are the major hypotheses that have been advanced?
   (4) What major recommendations for research and/or policy consideration are to be found in the theoretical literature?

2. What is the state of empirical knowledge concerning the innovation process?

   (1) What are the areas of empirical agreement and disagreement?
   What are the bases for the disagreement?

(2) What are the major gaps and weaknesses in our empirical knowledge?
(3) What are the major empirical findings?
(4) What are the major conclusions drawn from these findings?
(5) What major recommendations for either further research or for policy consideration are to be found in the empirical literature?

3. What is the extent of agreement between empirical studies and theory?

(1) Are there significant theoretical hypothesis which have not been tested?
(2) Are there major empirical findings that are disconfirming instances of a major theory?

These questions summarize the objectives of this as well as all the subsequent essays in this volume.

In addition to these review and assessment objectives, and as an example of how the information generated by this project might be utilized by policy makers and future researchers, each essay will also include a consideration of the following practical considerations:

1. Is our current understanding of the innovation process good enough to make statements about the potential leverage of federal policy at various points in the process?

2. If so, is it also possible to anticipate the consequences of exercising such leverage?

3. If not, what additional do we need to know to be able to make such statements, and what research is required to reach this level of knowledge?

Each contributor to this volume will, of course, be responding to both sets of questions from the perspective of the particular process phase or topic with which he is dealing.

The only remaining preliminary consideration concerns the definition and functional boundaries of the problem definition and idea generation phase of the innovation process. The "deliberate creativeness" quote which has provided the nexus for this cluster of considerations again serves to remind us of the obvious here. While we have chosen to refer to the _initial_ phase of the process of technological innovation as "problem definition and idea generation," it is clear that problems are defined and ideas generated in _every_ phase. In this essay, however, we will confine our attention to those activities that occur within the initial phase. Patterns of deliberate creativeness occuring in subsequent phases will be treated either in the essays dealing with these phases or in the discussion of phase linkage patterns in the summary essay. Of particular interest here will be the linkages of problem definition and idea generation with diffusion.

The phrase, "problem definition and idea generation" was chosen to describe the first phase of the innovation process because it points to the two informational components involved, on the one hand, and the product of a creative synthesis on the other. The two kinds of information required for the definition of a problem are described by Baker and Freeland as follows:

> 1. Recognition of an organizational need, problem, or opportunity which is perceived to be relevant to organizational objectives, and
> 2. Recognition of a means or technique by which to satisfy the need, solve the problem, or capitalize on the opportunity. (10)

Thus among the necessary conditions for generating an idea is information about both a need (problem or opportunity) and a means (potential technological capability) for meeting the need.

One question which arises in connection with the above concerns the identification of the "need" component as a need (problem or opportunity) of the organization. Would not an "organizational need, problem, or opportunity" be, by definition, "relevant to organizational objectives?" The point here is not to introduce a semantic quibble. Nor is it to deny that upon occasion the event which stimulated the process to begin was the recognition of a need within the organization. But it would seem likely that for the most part the stimulating event would be the recognition of a need that lies outside the organization, i.e., in the market. Though Utterback does not deal with this distinction explicitly, he clearly seems to take the outside or market need as primary. For instance he says in a summary passage dealing with environmental impingments upon the firm:

> One can hypothesize that an economic environment in which needs are clearly defined, as opposed to being heterogeneous or diffuse, will tend to stimulate technical innovation in firms. Similarly, mechanisms which communicate needs or change the perception of needs by firms, i.e., the use of outside consultants, contacts with customers and competitors, and efforts toward product planning and need assessment, will stimulate innovation. (11)

In another place he says, "Barriers to flows of people and information, between the firm and its environment will limit its knowledge of social and market needs... and thus limit the potential for innovation as seen by the firm." (12)

While there is substantial overlap between this view and that presented by Baker above, this is nevertheless an important distinction, and one which has received little treatment in the literature. Our suggestion here is that the flow of information about needs is a two step flow. That is, information about social and market needs flows into the firm through various channels. Unless such channels are unusually anemic, the total set of such communicated needs should far exceed the firm's capacity to respond. This set is then filtered in terms of the firm's capacity (both economic and technical), its corporate strategy and objectives, its agressiveness and morale etc. The much smaller subset of recognized needs that pass this filter are then subject to the next step which is their coupling with potential technological capacities for meeting them. This view of a two-step flow of information about needs also seems to be supported by Langrish et. al. (see quote on p. 5 above)

What Baker has identified as an organizational need, problem, or opportunity would seem (unless it is truly "internal"), to be that much smaller set of market needs which has survived the filter process sketched above and is thus compatible with the prevailing characteristics of the firm. It is important to recognize, however, that this set of "organizational needs", or perhaps more accurately "firm-compatible market needs" is the product of a filtering process. Otherwise our conceptual structure will not be sufficiently fine-grained to guide empirical investigations on several important questions. The details of this point will be explored in a later context.

Let us turn briefly now to the second information component of problem definition, namely data concerning means, that is, a potential technological capability for meeting a perceived need. As we shall see, a two-step flow of technical-means information has been well documented and there is a well developed body of empirical literature concerning its characteristics. The behavior of researchers, individually and in groups in the gathering of technical-means information is, by and large, also well understood. One significant gap in this knowledge will be explored at a later point.

The third, or creative synthesis, component of this initial process phase is herein to be labelled "idea generation". The product here is the concept of a product, process or device in which a technological capability, existing or potential, is related to a perceived need. As indicated, our concern with this creative act itself will for the most part be an environmental one. The reasons for this "contextual" bias have been offered previously. Hopefully these reasons have been reinforced by the above discussion of deliberate creativeness in which the emphasis is on the cultivation of maximally facilitating conditions for creativity.

On the basis of these considerations, let us return now to the task of defining and specifying the functional boundaries of the problem definition and idea generation phase. As we have indicated problem definition is to be viewed as the informational aspect, requiring data about both needs and technical means for meeting them. Idea generation is the synthesis of this information which results in the concept of a technological product, device or process. To put those elements together in a single definition, the problem definition and idea generation phase is to be defined as:

> The recognition of an existing or potential technological capability which may be related to a perceived need, problem or opportunity, and the concept of a product, process or device, which is a creative synthesis of such means and needs.

The key elements of this definition are indicated on the flow model provided in Figure 1. below.

On the above definition the initial phase of the innovation process ends with the creation of an idea. There is an additional consideration, however, which suggests that the functional boundaries of this process phase be extended somewhat. This point was made by Rubenstein who defines an idea as, "a potential proposal for undertaking new technical work which will require the commitment of significant organizational resources such as time, money, energy." (13) The significance of the phrase "potential proposal" is that until an idea has actually been submitted to the approoriate reviewer for a possible allocation of resources, there exists no linkage between this and later process phases. Further, until such a proposal actually has resources committed to it, and thus achieves "project" status, such linkage is only potential. Factors influencing the submission or non-submission of ideas and their subsequent disposition by management will be considered later. At this point it should simply be noted while

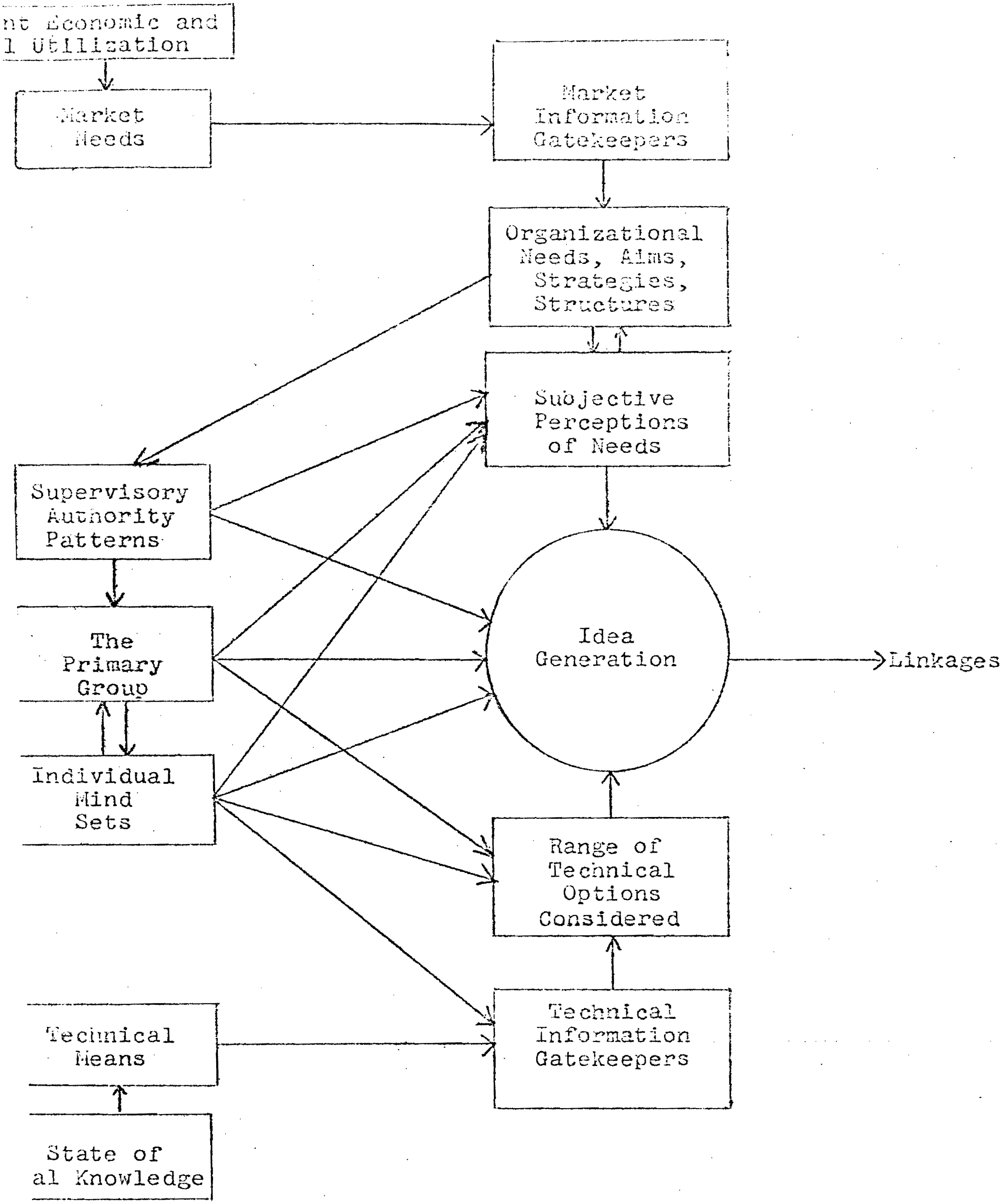


Figure 1. Information and Influence Flow Model
for Problem Definition/Idea Generation Phase

in a logical sense this process phase ends with the generation of an idea, we will extend its functional boundaries to include proposal submission and disposition which link this phase to the next.

Section II: Supervisory Authority Patterns

Perhaps by definition, but certainly in its dominant organizational forms, the transition from random to deliberate creativeness involves an increase in the extent to which the individual researcher's activities are subjected to control. By becoming a part of an organized R&D effort, he is less free to choose his research topics, to approach these as he sees fit, and to unexpectedly change direction in order to follow up on a new clue or insight. To be sure, increased control of the individual researcher's behavior is not the only implication of organized or deliberate creativeness. As has been pointed out, it is increasingly the case that:

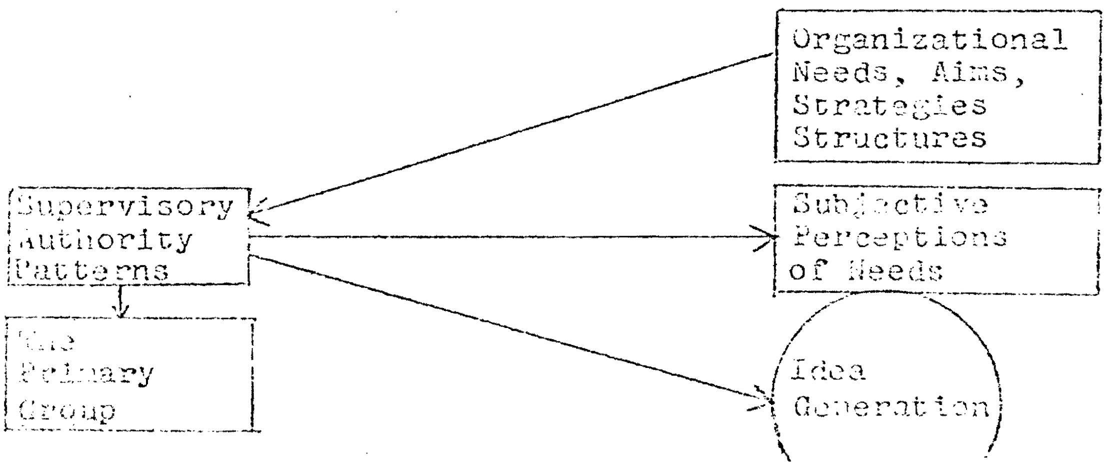
> ...problems to be attacked are too big for one man. They require the approaches of several types of specialists, of men from several disciplines. They demand more knowledge than one man may possess or readily acquire. They cannot be answered without laboratories and computers and other types of equipment which are expensive and often not available to one investigator. (14)

Thus there are numerous advantages in the organizational forms that have been developed for controlling the process of technological creativity. It may even be the case that, given the scale and complexity of needs in a technologically advanced society, such creative activities would in fact not be possible without these organized R&D efforts. But if this is so, it only serves to increase the importance of understanding the implications of decreasing the individual researcher's freedom by the imposition of controls, however necessary, upon the conduct of his professional activities.

The folklore surrounding this point is that freedom not only enhances creative accomplishment, but is in fact a necessary condition. If this is true, and if it is also true as suggested above that deliberate creativeness with its organizational structures and inherent controls on individual behavior is also necessary, then the need to understand this issue is an urgent one. Unfortunately, there are few empirical studies which bear on this topic. This situation reflects, in large measure, the conceptual difficulties inherent in terms like "freedom" and "control". Such terms are operationally unclear, and thus point in so many research directions, though with lack of specificity, that it is not surprising to find that so little has been said about them outside the "wisdom" literature.

It should be noted, however, that one of the directions in which this freedom/control issue points is to the role and "style" of the R&D managers. This individual personifies, for better or worse, the "control" factor in the enterprise of deliberate creativeness. It is the manner in which he exercises his authority in the decision to explore certain research areas and not others, to try certain approaches rather than others, and to follow only certain leads which involve a change of direction, which determines both the actual limits on the researcher's freedom and, often more importantly, his subjective perception of these limits. This observation leads one to posit that different patterns of the exercise of supervisory authority could differ in their impact on the researcher's perception of his own freedom and thus upon the frequency and quality of his creative accomplishments.

Thus one of elements in the flow model presented above (Figure 1.) was labelled "Supervisory Authority Patterns". Those elements that influence or are influenced by such patterns of supervisory authority were indicated as follows:



In examining this element of the model and other elements that influence or are influenced by it, it should again be stressed that the unit or level of analysis with which we are concerned is that of the environmental impingements upon the activity of the individual researcher as he engages in the creative act of generating ideas. Indirect influences upon this activity are taken into account on a very selective basis, and then only when they have their origin in some other element in the model. For instance, the influence of organizational needs, aims, etc. on patterns of supervisory authority is included. Other influences, such as the supervisor's educational background and experience, the mind-sets to which he is subject, the role and influence of his primary group, the networks of technical information flow of which he is a part, etc.; these influences upon his behavior are not accounted for by the model. It is not that they are considered unimportant, but simply that the model is not designed to array them. It is intended to display only those direct influences upon the individual researcher in his idea generating activity.

Let us turn now to the influence of alternative supervisory patterns on idea generating activity. In a 1965 study (15) Gordon and Marquis examined the authority patterns employed in four types of research settings: a university, a medical school, a hospital, and a health agency, in order to investigate the relationship between freedom and creative accomplishment. The results of research projects conducted in each of these four settings were assessed by independent evaluators in terms of criteria designed to reveal their quality and degree of innovation.

This comparison of innovative activities in an academic and three quasi-academic or marginal settings will be summarized only very briefly since it is tangential to our major concern. It provides the basis, however, for conclusions regarding the role and types of administrative influence on research activities, and in particular on the influence that the "visibility" of research consequences seems to have. In this study the research conducted in the academic context was judged to be clearly less innovative than in the other three contexts. The question then becomes, "How and in what manner do marginal or quasi-academic settings stimulate innovation?" (16)

Gordon and Marquis answer this question in terms of the greater visibility of research consequences in the more practical and mission-oriented settings. The visibility of the consequences of one's research is a function of two important considerations. First, the visibility of research consequences is obviously related to the clarity or obscurity of the organizational goals in terms of which such consequences are assessed.

> In an organizational setting where the owner of an organization or his representative can accurately evaluate the findings of a project in terms of organizational goals, he can encourage the researcher who shows high probability of solving such problems. As a consequence, the researcher is motivated to seek solutions to difficult but "relevant" problems in preference to less relevant but easier problems. In seeking a solution to the difficult problems, the researcher at times must abandon traditional methods and thinking. This would appear to be as true for the academic as for the non-academic researcher. Kuhn, for instance, has observed that "The novel theory seems a direct response to crises." (17)

This point is directly relevant to the line of influence in our model (see Figure 3) which extends from the element labelled "organizational needs, aims, etc." to that labelled "supervisory authority patterns". We shall return to this point in a moment. First, we need to link this point about the visibility of research consequences to a second consideration.

Regardless of the ease with which research consequences may be assessed in a particular research setting, they are not visible until someone assesses them. This raises directly the question of the pattern or "style" of research management and its influence on creative accomplishment. To get at this question, the research projects employed in this study were divided into three groups.

> 1. Projects in which the project directors either stated that they had no administrative superior or that they did not discuss their research with their administrative superior. (Low visibility of consequences + freedom)
>
> 2. Projects in which project directors had freedom to specify their research procedures and they discussed their research with their administrative superior. (High visibility of consequences + freedom)
>
> 3. Projects in which the project directors stated that they had an administrative superior with whom they had discussions and who consistently influenced procedures. (High visibility + limited freedom) (18)

On the hypothesis that both high visibility of consequences and research freedom are important to creative activity, the second of these three types of authority patterns should be expected to maximize such behavior while the first and third would minimize it. This hypothesis was confirmed. It was found that the percentage of research projects judged as highly innovative that occurred under the ideal authority pattern was two and one-half times that under the non-ideal conditions. These results are summarized as follows:

> In sum, it is not possible to make a blanket statement relating maximal freedom to innovation, but rather maximal freedom is conducive to innovation only when there is an impetus to innovate. It further appears that the institutional settings in which research is conducted---in particular the visibility of the consequences of the research in relation to the goals of the owners of the institution---has a significant effect on inducing innovation. (19)

Thus it would seem that creative behavior is more likely to occur where the consequences of such behavior are visible, i.e., where the organizational criteria for assessing research results are clear and where the supervisor keeps in touch with what the researcher is doing and how his work is going; but where at the same time the researcher has freedom, i.e., he is not dominated by his superior.

The above study was chosen for detailed examination because of the linkage it provides between not only creative behavior and different patterns of authority or leadership, but also between these elements and the clarity of organizational goals. Though there are obviously other informational sources within the environment that also contribute to shaping the researcher's subjective perception of the organization's needs and goals, the primary one is the immediate supervisor. This he does explicitly through informal discussion and more formal meetings, and implicitly by the decisions he makes concerning ideas submitted. The latter will be discussed in more detail in the context of the "disposition decision" as one of the linkages between this initial phase of the innovation process and the next.

Setting this additional refinement about the assessment of research consequences aside for the moment, the authority patterns identified by Gordon and Marquis and their correlation with innovative behavior finds support in a number of other studies. In summarizing the literature dealing with "leadership styles" or "forms of leadership", Hill reports that:

> Most commonly, three general patterns of a supervisor's leadership behavior have been described, though the terminology applied to these patterns has differed somewhat between authors:
> (1) nondirective, permissive, a laissez-faire, accomodative or abdicative style where the leader relinquishes only influence in setting group goals to the group;
> (2) democratic, a participatory, group-centered, subordinate-centered, employee-centered, human-relations-oriented style where the supervisor allows and encourages a mutual relationship with subordinates;
> (3) autocratic, authoritarian, boss-centered, task centered, production centered, close and punitive style where the supervisor allows his subordinates little or no influence in the setting up of work procedures, while primarily concentrating on achieving task goals. (20)

While the cluster of words associated with each of these three leadership patterns reflects differences of detail and emphasis, there is, "...a high concurrence of findings about general leadership patterns across a range of situations." (21) These three leadership patterns and their correlation with more or less creative behavior strengthens our confidence in the work of Gordon and Marquis. A study by Pelz (22) indicates that the creative performance of researchers in a large medical organization was highest when frequent contact with the supervisor was combined with a fair measure of independence in the conduct of their research.

The work of Ronken and Lawrence (23) also indicates that the democratic leadership pattern (High visibility of consequences + freedom in Gordon and Marquis' terms) is more effective in stimulating creative behavior than a leadership style that is either too permissive or overly directive. One of the problems of a style that is too permissive is that the researcher may interpret being left alone with only minimal contact as a lack of interest in what he is doing and thus a devaluation of his world. Hill also notes this possible interpretation of a leadership style that is overly permissive, and sees it as a "demotivating" influence. (23)

In commenting on the disfunctional effects of an autocratic style (high visibility + low freedom), Pelz and Andrews state that, "...continued direction by the chief will stunt initiative and independence and these are qualities basic to scientific achievement." (24) And in a perhaps unexpected result, Hill (25) found that while researchers perform best under participatory or democratic leadership, that when the leadership pattern was strongly autocratic or overly directive they responded by seeking even greater direction.

Finally, the studies of Andrews and Farris (25) also support the conclusions offered above concerning the need to "keep in touch", or provide visibility for the consequences of a researcher's work, if freedom is to be effective in stimulating creativity. They found that freedom was unrelated to innovation if the supervisor did not consult with them prior to making decisions concerning their projects. Where freedom was combined with consultation, however, a substantial increase in innovative behavior was observed. A key factor in the effectiveness of such consultation would seem to be the supervisor's own technical competence. In drawing some general conclusions from their research Andrews and Farris say:

> Greatest innovation occurred under supervisors who knew the technical details of their subordinates' work, who could critically evaluate that work, and who could influence work goals. Thus the widespread practice of including technical competence among the criteria for choosing supervisors seems to be sound. This does not mean that a supervisor should constantly "meddle" in his subordinates' activities. But he should be available, competent in the current "state of the art," actively interested in the project, and informed about it...
>
> What if this kind of structure is not possible, or if a supervisor's technical competence has become obsolete? Again the data were clear: provide substantial freedom for subordinates. Freedom acted as a partial substitute for skilled supervision. But even where subordinates have freedom, the supervisor still makes some kinds of decisions. For freedom to be effective, the data showed that the supervisor must consult with his subordinates before making these decisions. (26)

Thus the visibility of research consequences, as achieved by an available and competent supervisor who knows what his subordinates are doing is the intervening variable, activating the potential of professional freedom to increase creative accomplishment. The influence of this intervening variable appears dependent, however, on the technical competence of the supervisor.

Let us turn briefly at this point to the influence of major patterns of supervisory authority on the primary group. In an important study (27), to be examined in detail later, Allen and Cohen discuss the role of the primary group in establishing and/or reinforcing commitments to certain technical approaches and downgrading the value of others. The adverse consequences of such individual and group biases lead the authors to offer the R&D manager the following advice:

> Suffice it to say that engineers and scientists should be forewarned to consciously seek out contradictory opinions and attitudes concerning their work and to recognize the value of cultivating contrary-minded colleagues. R&D managers can of course take this situation into account in forming their work groups.(28)

The role of the primary group in attitude formation and the influence of such group attitudes on the innovative process will be discussed in a later section. The point to be made here concerns the influence that the supervisor can exert to minimize the possible dysfunctional consequences of such group mind-sets. This point is also closely related to evidence to be presented later that diversity or heterogeneity of work assignments and settings has a positive effect on the level of creative accomplishment.

The last point to be mentioned in connection with the "control" figure or supervisor in the R&D context concerns his influence on the researcher's perception of the needs of the firm. Illustrative of this point is an experience related by Hyman.

> I was once talking with the manager of engineering whose company had just lost several million dollars because of marketing a defective machine which later had to be withdrawn from the market. As a result, the company was under tremendous pressure to recover its previous position in a highly competitive market. The manager was worried because in this market, if you do not look ahead and keep generating new patents you cannot survive very long. Yet his men, eighteen design engineers, had not turned out a patent in the past year or so. Like everyone else in the company, their major concern was with current pressures to keep the business out of the red. The manager, in order to change this lack of new patents, first thought in terms of his selection policies. Maybe he had chosen the wrong men. Maybe he should fire some of his present staff and hire new men. One day the thought occurred to him:

> "Why not first call the men in and tell them what I want?" He called them to a meeting and told them, "Look, men, we need patents, or else we die." The next month his men presented him with several patent applications. And they have been continuing at that rate ever since. One gets the impression that essentially, the men just looked at each other and said, "Well if that's what he wants, why didn't he say so?" (29)

This overly simple and somewhat dramatic little story contains several themes which are recurrent themes in the research literature. Among these are: the dominance of oral communication throughout the process, the phenomenon of the "unsubmitted idea", as well as the role of the supervisor in shaping researchers' subjective perceptions of organizational needs. The former points will be treated later. The latter but extends the inventory of ways in which problem definition and idea generation within the process of technological innovation is strongly influenced by the first-line control figure. It is ironic perhaps that one can get the impression from many literature sources that R&D supervisors are but passive functionaries whose role deserves even less mention than that of the "product champion" or "top person". But if the evidence to be presented above and subsequently is to be believed, their role is a big one and their influence substantial.

One is reminded of the situation in which the question is asked, "How good is the school that your children attend?" While certain shaky generalizations about "the school" are possible, the appropriate response is that the question is misdirected in that it does not identify the central or "atomic" unit. The child's school experience is hardly better or worse than the particular teacher with whom the child spends his day. That person is the primary facilitating or inhibiting influence, not "the school". The same holds by and large for creative accomplishment within the R&D context. Here the supervisor looms large as a major facilitating or inhibiting factor, though apparently with less recognition than the teacher receives.

There are, of course, other influences on both sides of this analogy. One is the role and influence of the primary group. This is even less well understood than that of the immediate supervisor/teacher, though it is probably equally important to the success or failure of the ventures in these respective contexts. It is to this influence that we shall turn next.

Section III. The Primary Group

Those elements of our flow model that influence or are influenced by the primary group were as follows:

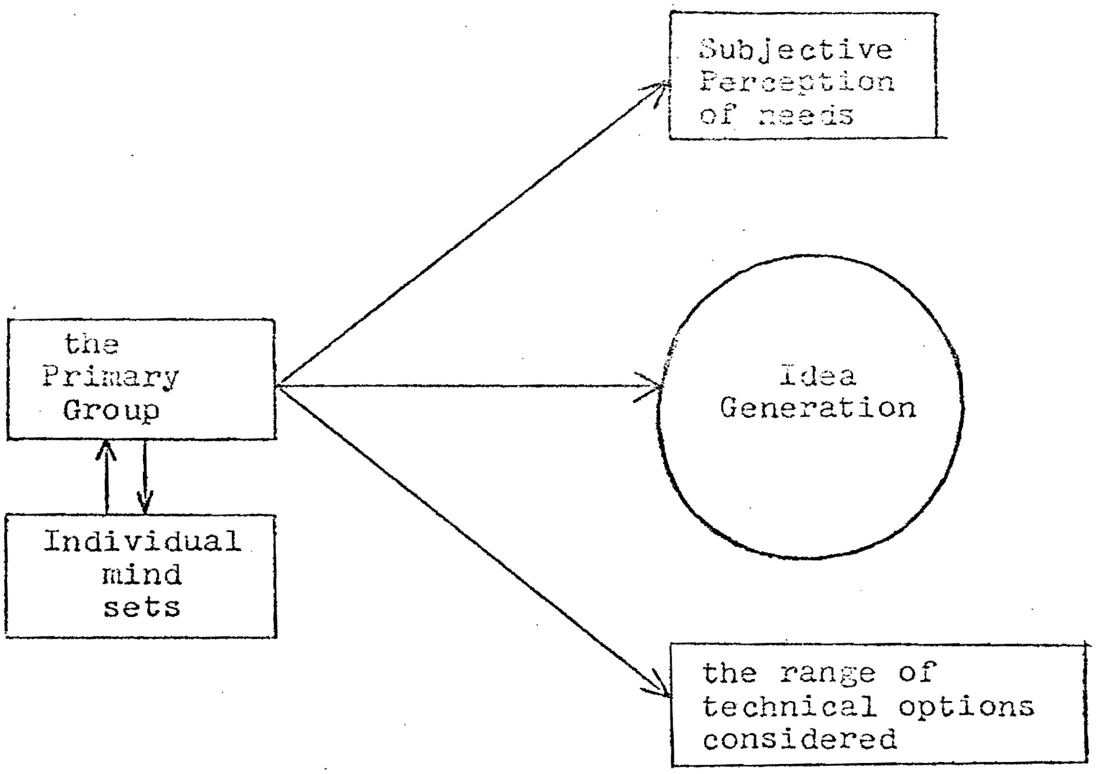


Figure 3.

Let us begin our review of the influence of the primary group with an examination of a very important article by Allen and Cohen.(30) They were concerned with the mediating influence of the primary group on information flow in a small R&D laboratory.

Two of their findings merit special attention. First they found a very high correlation between the individuals within the lab with whom one socializes and those chosen for technical communication. Thus the social structure of the lab was an important influence in the transfer of technical information. To be sure, there is almost constant technical discussion within the project work group. However, when asked which individuals supplied information which was a "critical incident" in the course of the respondent's last completed project, it was found that this information came exclusively from people outside his immediate work group. Thus while the formation of work groups does serve to channel technical communication within the lab in obvious ways, this flow is also strongly, and perhaps more crucially, influenced by the informal patterns of socialization. Thus the authors conclude, "communication patterns will tend to follow the structure of both the formal work group structures and the informal social relationships in the laboratory." (31)

In another facet of the same study, the respondents were asked to indicate their attitudes on each of three rather uncertain/technological questions confronting the laboratory. The purpose of this question was to test the following hypothesis:

> "Technological attitudes, attitudes toward such things as feasibility of particular approaches which are not yet physically testable, will be strongly influenced by the attitudes held by other members of the primary groups to which the engineer belongs."(32)

Credit for the formation of this hypothesis was given to the work of Kurt Lewin and others, who had suggested that, "When an opinion or attitude cannot be tested directly against 'physical reality' that the individual will resort to a test against 'social reality'. In other words, he will look to his peers for confirmation or disconfirmation and react accordingly." (33) The hypothesis was supported by the data. This result has obvious and important implications for the process of problem definition and idea generation. The significance of this finding is increased when coupled with the rather strong evidence that "engineers, once they have become committed to a particular technical approach, tend to discount very strongly information which would disconfirm their attitude." (34) If these two pieces of data are then connected with the conclusion presented above, that researchers tend to restrict their technical discussion to those who are members of their primary group and thus share their attitudes, the result is indeed significant.

In fact, it is difficult to overestimate its significance for problem definition and idea generation. The flow of technical information would seem to be subject to a double screen. First, as we shall examine in more detail in the next section, the individual researcher on the basis of his own past experience is biased towards a particular approach and away from others. Then, in addition, it would seem that there are social forces operating within his primary group that reinforce this particular bias or mind set. They are reinforced by technical discussions with individuals who share a similar technical orientation. As a counterbalance to these predispositional filters and reinforcements, Allen and Cohen suggest that engineers and scientists should be urged to "consciously seek out contradictory opinions and attitudes concerning their work and to recognize the value of cultivating contrary-minded colleagues." (35) In addition, laboratory managers should be aware of this phenomenon in forming work groups. "Merely introducing a single individual with conflicting attitudes should produce sufficient jitter to keep the group aware of other points of view" (36).

While the above data provide strong evidence of the role of the primary social group in reinforcing the individual's already existing predispositions, and thus in narrowing the range of technical approaches considered, the form of this data was not such as to permit conclusions regarding the role of the primary group in attitude formation. But the overwhelming body of evidence from social psychology indicates that this "formative" role is also a very strong one. This literature, while not "the-flow-of-technical-information specific" or even "technological innovation specific", is strong enough in its prima focie implications for these areas to be suggestive of future research directions. That is, it is reasonable to hypothesize that primary social groups exert considerable influence on the formation of individual and laboratory attitudes towards the feasibility of various technical approaches, and that in turn the flow of technical information is thus subject to quite significant, though "invisible" screens or filters. More work is obviously needed on this topic.

Assuming for the moment that the primary group does influence the formation of certain attitudes as well as reinforce already existing ones, a further caution could be offered to the R&D manager. "The possibility that the causal direction is such that interaction leads to agreement, implies that management should periodically rotate their devil's advocates to prevent their capture by the prevailing group attitude." (37) Perhaps however, a more powerful mechanism is available to guard against counter-productive primary group influences in attitude formation. This mechanism is, in fact, quite commonly employed throughout the R&D world and its benefits are often noted in the literature. We have reference to the introduction of diversity into the laboratory setting by means of the use of consultants, and by the diversification of work assignments. Utterback (38) reports that outside consultants played a crucial role in the generation of ideas for sixteen of the thirty-two new instruments he studied. Likewise, Peters (39) in exploring the relationships among consulting, diversity of work assignments, and idea generation in interviews with faculty in four M.I.T. departments, found that 55% of those reporting new ideas engaged in consulting as opposed to 55% of those not reporting ideas. Such enhancement of idea generation by means of outside consultation is also reported by Gordon and Morse (40). Finally, Peters, in the work cited above also found evidence that diversity in work assignments increases the probability of idea generation; specifically, 70% of those reporting ideas also reported that their work was mixed between research and development, as opposed to 28% of those not reporting ideas. Utterback (41) explains these findings in terms of the need to synthesize information in idea generation. While this should not be discounted, perhaps a deeper explanation lies in the function of both consulting and varied work assignments in providing alternative technical perspectives and attitudes to those of the researcher's primary group.

If the lead provided by social psychology, as to the role of the primary group in attitude formation, should prove suggestive enough to be pursued by future researchers, let us suggest an additional refinement. Perhaps the members of a primary group do not all play an equal role in the development of the group's attitudes. As in other areas of life there are <u>opinion leaders</u> whose personal and informational characteristics are such as to make their contribution disproportionate, even decisive. The suggestion here is that perhaps there exists within the laboratory context certain key individuals who function on the level of attitude formation in a fashion analogous to the function of "technical information gatekeepers" (the well documented role of such gatekeepers will be discussed later). Such opinion leaders or "technical attitude gatekeepers" may exist, and operate in either a facilitating or inhibiting way to bias the problem definition and idea generation process towards certain technical alternatives and away from others. We shall later consider the possibility that the same individuals perform both gatekeeper roles. But in any case, the possibility that such individuals exist should be recognized and investigated, since their impact upon the process would be quite significant.

On the assumption that a state-of-the-art assessment should be at least as concerned with what is <u>not yet</u> understood as with what is understood already, let us extend our hopefully fruitful speculation a bit further. The implications of the work by Allen and Cohen also need to be explored for organizational levels other than the primary group in the laboratory. For instance, there is the widely held folk-wisdom that in any industry some firms are innovation leaders while others are followers. If there is substance to such distinctions, might it not reflect the influence of the dominant attitudes of the primary group at or near the top of such firms? Examples of key attitudes that might be formed or reinforced by such groups would be those taken towards risk-taking, the exploitation of new technological capabilities, newly recognized market needs, and the like. Such prevailing paradigms or mind-sets of top management would be articulated in both broad-guage corporate goals strategies and the disposition decisions regarding particular ideas that are generated in the laboratory. Perhaps related to this point in negative fashion is the role of the "product champion" or "top man" as noted by Langrish et al (41). Such individuals by their own persuasive skill and dogged determination overcome the firm's negative bias towards their idea. What is overcome in these instances, or as we are suggesting, are the attitudes of a primary group which are antithetical to the idea being presented.

Let us turn now to the influence of the primary group in shaping the subjective perception of needs. We have already mentioned the role of the supervisor in the formation of such perceptions. They are also sensitive to more subtle, implicit and often unintended "communications" from management. The environment which picks up these messages and clues, interprets them rightly or wrongly, and by subsequent behavior gives them substance, are the primary social groups within the lab. Baker and Freeland point to one negative manifestation of such group perceptions; in this case triggered by inadequate management responses to new ideas that have been submitted in the past.

> Thus, expectations regarding organizational rewards for idea flow effort are modified downward and the cycle is ready to repeat. As new employees enter the organization they learn these low expectations from the veterans who have traversed full cycle. In such an environment it is little wonder that potentially creative employees fail to realize their potential and appear to "go dry" over time. (42)

Since the recognition of a need is the precipitating event for most problem definition and idea generation sequences, the influence of the primary group in shaping the researcher's perception of such needs is crucial. As has been the case throughout this section, this too is a little understood phenomenon which merits careful investigation.

In concluding this section we should perhaps offer a balancing note to what has been a largely negative thrust. The influences of the primary group, and the attitudes they serve to form and/or reinforce are, of course, by no means wholly counterproductive to corporate objectives. Whether the directions in which they lead are appropriate or not depends upon many variables in the total environment. Perhaps the influences of primary groups can best be summed up by paraphrasing the line from the nursery rhyme that goes, "When they are good, they are very very good, and when they are bad they are horrid." Whether "good" or "horrid", such influence is substantial and badly neglected to date.

Section IV. Individual Mind-Sets

The concept of "mind-set" is used in the literature to refer to the biasing influence of past experience that an individual brings to his present problem solving activities. The term "biasing set" is also frequently used to refer to this influence. Allen and Marquis introduce this concept in the following way:

> It is known that the likelihood of finding a solution to a problem may be raised or lowered because the problem solver is set to respond in certain predetermined ways. Prior experience with tools or approaches used in solving similar problems in a certain way may result in a "set" which biases the problem solver and can divert him from consideration of alternative solutions. (43)

The individual may thus be "set" to transfer information or an approach that he has used successfully in the past to a present problem perceived as similar. Such mind-sets may have either positive or negative effects on the achievement of a solution. If the transferability of past experience is appropriate to the new situation, it will be a positive factor. But if the transfer is inappropriate it may block or delay the discovery of a different and superior solution.

A paradigm illustration of the existence and influence of mind-sets has been provided in an experiment conducted by Birch and Rabinowitz (44). This experiment is so intriguing in both its structure and results that we feel a lengthy quote is justified.

> In this problem the S is required to tie together the free ends of two cords which are suspended from the ceiling to the floor of a corridor. The distance between the two cords is such that the S cannot reach one cord if the other is held. In our arrangement the problem could be solved only if the S would tie a weight to the end of one of the strings and thus convert it into a pendulum which could be set swinging and then be caught on its upswing while the stationary cord was held. The two cords could then be tied together and the problem solved. In our situation only two obejcts could be utilized as weights. The first of these objects was an electrical switch and the second, an electrical relay. The conditions of pretest training involved the acquisition of differential prior experience with these objects by our Ss. The pretest training was conducted as follows:
>
> Group S contained 9 Ss who were given the pretest task of completing an electrical circuit on a "bread-board" by using a switch, which had to be installed if the circuit were to be completed and controllable.
>
> Group R consisted of 10 Ss who received pretest training in the completion of an identical circuit by the use of a relay, which is essentially a switch.
>
> Group C, the control group, consisted of 6 engineering students with a wide variety of electrical experience. These Ss were given no pretraining. The Ss in groups R and S had had little or no experience with electrical wiring.
>
> Shortly after having completed the pretesting tasks, the Ss were presented with the two-cord problem and asked to solve it by using the objects lying before them on a table. Only two objects were present, a switch and a relay, each identical with the ones used in the pretraining period.
>
> All Ss were individually tested. Upon completing the two-cord problem, the Ss were asked why they had chosen either the switch or the relay as the pendulum weight. (45)

As would be expected the control group, who were equally familiar with both switches and relays, chose equally between them as pendulum weights in solving the two-cord problem. Their prior experience was not heavily weighted in favor of one or the other in terms of their utility in a new context in which their function was quite different from the normal.

The behavior of the subjects who had received prior training or experience with either the switch or the relay was strikingly different, however. Of those who had experience with completing the electrical circuit with a relay, none of them used this object as the pendulum weight (i.e., ten of ten used the switch.) On the other hand, the subjects who had been trained to use a switch in completing the circuit preponderantly chose the relay as a pendulum (seven of nine). Combining the results of both experimental groups, 17 of the 19 subjects used that object with which they had had no prior experience as the problem-solving tool. Thus there is strong evidence that the nature of the subjects' previous specific experience was influential in determining their problem-solving behavior. Post-experiment interviews reinforced this conclusion.

This study was reported in detail in part because of its intrinsic value as a paradigm, but also in part to illustrate that the biasing effect of past experience may be negative as well as positive. That is, while some biases or mind-sets may be positive in the sense of enhancing the value of an object, idea, or approach as a problem solving tool, others may be negative, i.e., they may prevent such transfers to the present problem solving situation. Prior experience colors the perceived characteristics or properties of an idea, object, or approach, by emphasizing some but not others, in ways that inhibit or enhance their subsequent utilization. The old saying that experience is the best teacher is only a half truth.

In the information and influence flow model presented earlier, those lines of influence indicated for the mind-sets to which an individual researcher is subject were as follows:

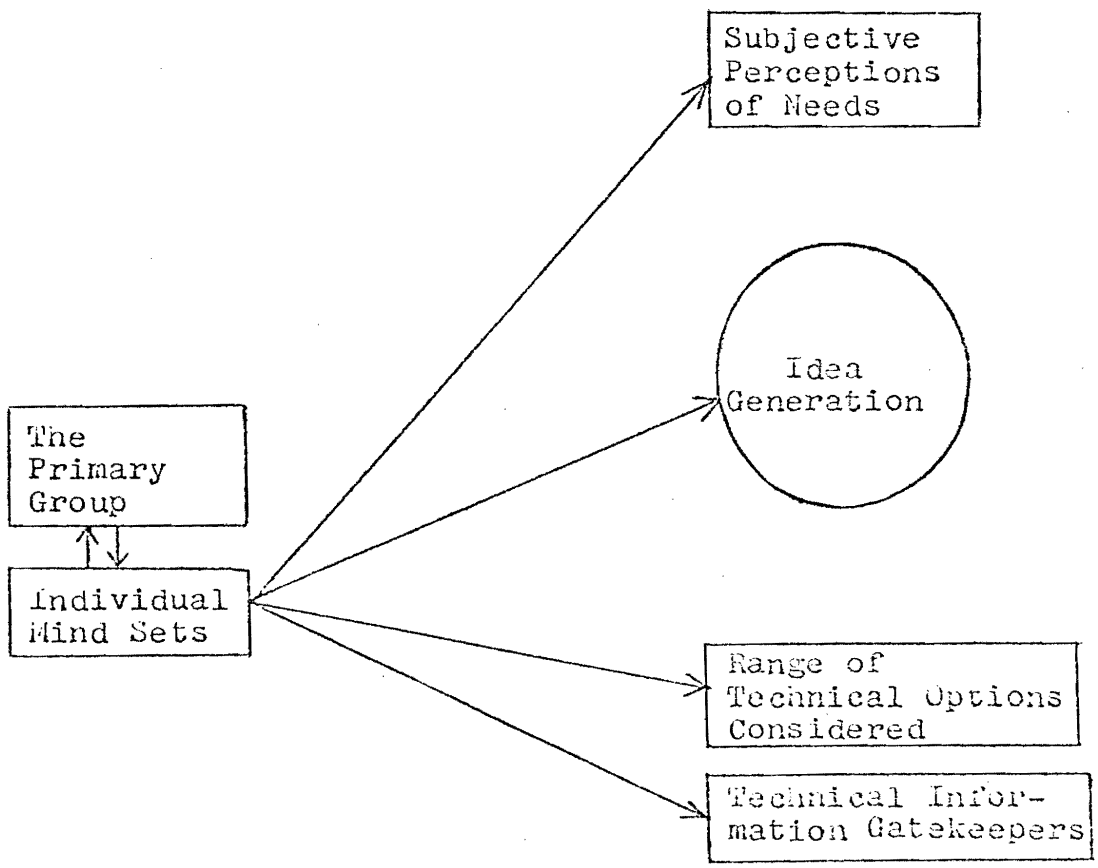


Figure 4.

For some of these lines of influence there are empirical studies, for others we would offer research suggestions.

Allen and Marquis compared the behavior of eight laboratories in two R & D proposal competitions. They found that mindsets resulting from prior experience do not, by themselves, result in a higher or lower probability of achieving a correct solution. While this result may speciously violate a commonsense feeling about the value of experience, it is hardly surprising when coupled with the reminder that, the crucial point about prior experience is not its existence per se but rather the appropriateness or inappropriateness of its transfer to a present situation. In other words, when prior experience is appropriate to the present problem the probability of achieving a successful solution is increased. Thus the biasing set was positive. On the other hand, when the prior experience is not appropriate, i.e. the biasing set is negative, the probability of success is lowered. Prior knowledge was found to have much the same effect as prior experience.

Earlier studies (46, 47) have shown that success in overcoming the effect of a negative bias is a function of the number of alternatives considered in the problem solving process. Allen and Marquis also found this to be the case.

> Of the eight instances in which the laboratory had prior experience with a technique which would be unsuccessful if applied to the present problem, four considered no alternative approaches and all four submitted solutions which were evaluated as unsuccessful. In four other instances the laboratory considered two, three, or more alternative approaches, and half of them achieved a successful solution. The additional effort required to search for and compare several alternative approaches is justified by the decreased susceptibility to negative biasing set. (43)

If one knew a priori when an individual's mind set constituted a positive bias and when it was negative, there, of course, would be no problem. One would just look for alternatives in the negative cases. However, an individual is typically not that self-conscious about the nature of his own biases, nor does he know before hand which approach will prove success-ful.

Given these constraints, the prescription to always consider several alternatives would seem to be sound. A somewhat subtle complication should be noted, however. If the individual researcher's normal approach to problem solving situations is to consider several alternatives, then by definition the influence of a particular biasing set is not a problem for him in the first place. On the other hand, if he is strongly influenced by a particular set then this fact will lessen the effect of what amounts to contrary advice. Perhaps the point of such advice needs to be reinforced by certain approaches available to management. Several points made earlier about the influence of the primary group should be recalled in this regard. While primary social groups are not open to being restructured, the primary work group is. The individual with a particularly strong mind-set might well acquire more flexibility over time if he were a part of a particularly heterogeneous work group, or at least a group that included a "devil's advocate." One might also consider the beneficial effects that have been shown to accrue from a diversity of work assignments.

It is also important in this connection to be aware of experimental work in social psychology on specific problem solving techniques. Let us mention two. The first involves a comparison of problem solving behavior under deferred-judgment instructions with such behavior under concurrent-judgment instructions. Deferred judgment simply involves the articulation of potential solutions to a problem without evaluation or critical analysis of their quality until a number of alternatives have been posed. Concurrent judgment conditions, on the other hand, involve instructions which require only solutions of "good" quality.

The deferred judgment approach to problem solving is commonly referred to as "brainstorming", and received considerable popular attention and some serious investigation in the late '50s and early '60s. Conclusions as to its merit are frankly mixed, due primarily to an absence of agreement on standardization of experimental conditions. Parnes and Meadow report, "Significantly more good solutions were produced under the deferred-judgment instructions than under the concurrent-judgment instructions." (49) Others report much less impressive or mixed results. (50) While the experimental evidence concerning the deferred judgment technique is, therefore, not clear-cut, one can safely say that it is of value in particular cases in helping the individual or group overcome the effects of biasing sets.

A related technique which should be of even more value in this context is that of "extended effort". A mind set leads one to a familiar approach or idea which is transferred to the present situation and the search stops. There is experimental evidence, however, that, "Extended effort in idea production will lead to an increasing proportion of good ideas with increased production." (51) If the conditions are such that one is forced beyond his initial production of ideas with which he is familiar, he will begin to grope for less obvious ones, the quality of which may be higher. The advantage of extended effort instructions in overcoming individual and group biases should be obvious.

Let us conclude this section with an observation and a suggestion for further research. The observation concerns the tenacity with which a bias or mind-set is held. This would seem to be a function not only of one's past experience with certain technical approaches and information and not others, but also of more generalized or content-independent characteristics which psychologists call traits. Among these traits would be the need for approval, the need to manipulate the conditions of one's environment, and the need for security. These traits are often lumped together under the label "self-confidence". And the point here is that while these, too, are the product of one's past experience, at no point are they completely fixed and unchangeable. New experiences, such as those faced when a particular mind-set does not serve well in a new situation, also contribute to the shaping of such traits. And if such experiences, in which the familiar fails and one must deal with newness and uncertainty, can be managed successfully in a context that is supportive, then personal and professional growth can take place. Thus the need for premature closure, which is at the base of mind-sets, can weaken, and in its place can emerge more flexibility in the process of idea generation.

And finally the research suggestion: In light of the above it is probably a mistake to view technical information gate-keepers as passive conduits in the flow network. Relative to their own mind-sets and the tenacity with which they are held, they also filter such information. The nature, extent and directions of such will of course vary with the individual. But to assume that such a filtering function does not exist is *prima focie* questionable. Since the flow of technical information in the R & D process is highly dependent upon such gate-keepers we need to know much more than is now known about their role. It is a wide open area for investigation.

Section V: The Flow of Information Concerning Needs

In the introductory section we dealt briefly with the pattern of information flow which characterizes problem definition and idea generation. We also introduced a distinction between market needs to which a firm may choose to respond, and its own internal needs to which it *must* respond. We shall now trace in more detail the pattern of information flow about such needs.

In our model the principal elements were represented as follows:

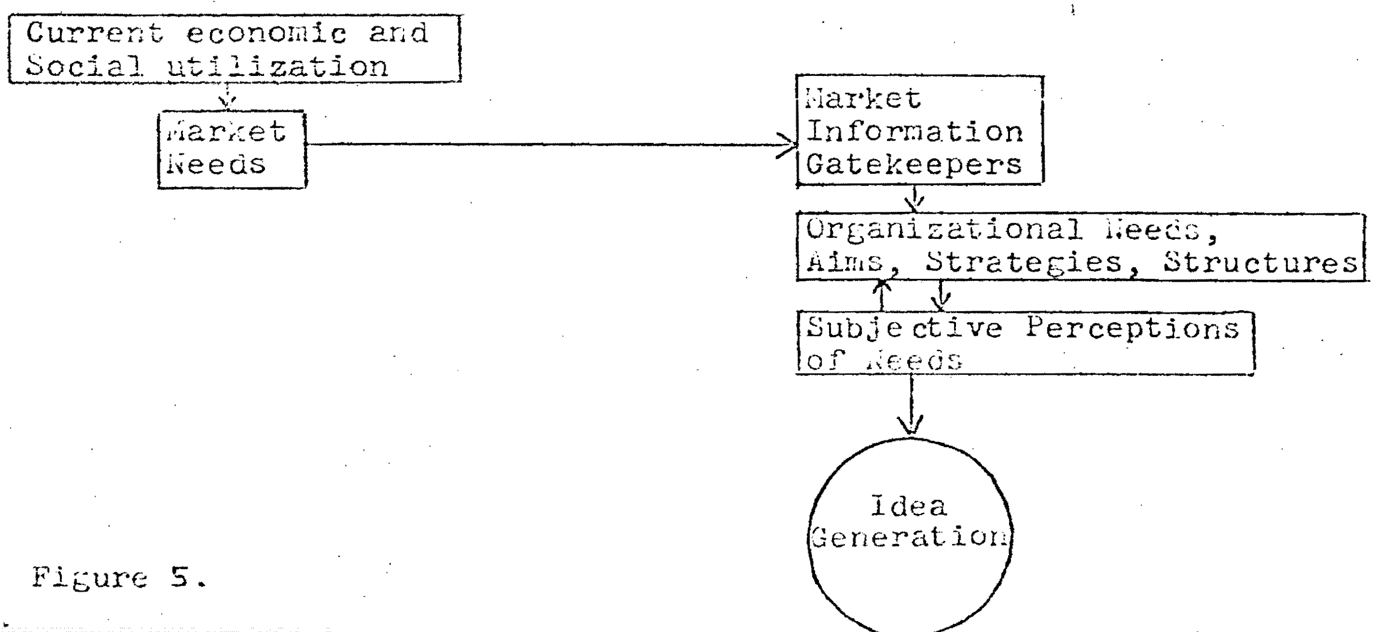


Figure 5.

This segment of our model is the first we have considered in which the flow of information rather than influence is primary. As will become apparent to the reader the informational aspect has received much more attention, and thus the empirical data base is much better established.

Following Utterback (52) we have stressed two elements of the environment in which technological innovation takes place as being of crucial importance. These are the degree of economic and social utilization of existing products and processes and the current state of technical knowledge. Together they provide both the limits or boundaries of the process and the external environmental inputs which feed it. The current economic and social utilization is taken here to include those factors which lead to the recognition of needs and desires for new products and processes.

Utterback reports that, "There have been few controlled studies of the effects of differing environments and environmental parameters on the process of innovation within firms." (53) Studies by Schmookler (54) and Enos (55) do indicate however an increase in the frequency of innovation when the market for a particular kind of product is expanding. Also, when the costs of a particular component or input increases, innovations designed to reduce the use of that component in producing the product can be expected to increase.

As we have suggested above, however, such factors are probably not the most critical ones in determining the extent and quality of a firm's innovative activity. As Utterback suggests these lie closer to home. "The primary limitations on a firm's effectiveness in innovation appear to be its ability and perhaps aggressiveness in recognizing needs and demands in its external environment." (56) At this point the state-of-the-art understanding becomes surprisingly uneven. Some aspects of the flow of market-need information are understood rather well, while others are quite opaque.

That which seems to have been studied most extensively, and about which there is broad agreement, is the event which triggers the innovation process. Sixty to eighty percent of the innovations examined by a number of researchers find their starting point in the recognition of a need. In the remaining cases the stimulating event has been new scientific or technical advances for which an application is then sought. This impressive community of agreement has been summarized by Utterback in the following table:

TABLE 1

A COMPARISON OF STUDIES OF THE PROPORTIONS OF INNOVATIONS STIMULATED BY MARKET NEEDS AND TECHNOLOGICAL OPPORTUNITIES

	Proportion from Market, Mission or Production Needs (percent)	Proportion from Technical Opportunities (percent)	Sample Size
er, et al. (57)	77	23	303*
ter and Williams (58)	73	27	137
dnar (59)	69	31	108
rwin and Isenson (60)	61	34	710+
grisn (61)	66	34	84
rs and Marquis (62)	78	22	439
nenbaum, et al. (63)	90	10	10
erback (64)	75	25	32

*Ideas for new products and processes.

+Research events used in 20 developments.

Thus the "need-means" pattern is dominant. That is, in the majority of cases a need first comes to be recognized and stimulates the search for a technical capability which will satisfy it.

Information about market needs seems to come primarily through oral and informal discussion with contacts <u>outside</u> the firm (9, 52, 65, 37, 61, 62). Further, such communication about a need seems to be most often initiated by someone other than the individual who ultimately generates the idea for an innovation. These outside sources of market need information are most often an existing or potential customer (52). The search for market need information (if, indeed, it may be called a search in light of the passivity revealed in the above summary) seems much less structured and deliberate than the subsequent search for matching technical capabilities.

This sketch of the flow pattern for market need information occasions a number of observations and/or questions. First, if this is even a reasonably complete picture of how a firm comes to know of market needs (which I rather doubt) one must be shaken by its impoverished and haphazard nature. Informal contacts with customers or potential customers is certainly an important channel to the outside world. But if, as is pictured here, most of these contacts that matter are with a researcher in the lab, then one must suspect that there are quite rich contacts at other levels within the firm that are not being utilized. Salesmen, for instance, would have much more extensive contact with customers than the researcher and thus should be a better source of market need information. We have not been able to locate any literature, however, which discusses this as a function performed by salesmen. Either salesmen do not provide such inputs to the lab, or their role in this regard has not been investigated. We suspect that the latter is the case.

Secondly, a great many firms have a technical services group which works closely with their customers, assisting with the installation, training of personnel and operations of equipment purchased. Such technical or trouble-shooting groups thus have a close working relation with customers and should provide a rich source of market need information. Again, however, we have not been able to find any research literature which deals with their input.

Thirdly, it would seem that at least some market research groups within firms, in addition to assessing the market for new products already in the pipeline, would also be concerned with analyses of existing market conditions that would reveal new needs and opportunities. As before, we have thus far not been able to locate any research literature which addresses itself to the interaction of such groups with the R&D lab. The same applies to other units within the firm, such as those with long-range planning, technological forecasting, or even corporate strategy responsibilities, whose contributions to the flow of market need information would be non-negligible. Certainly these channels must exist, but they do not appear in the research literature.

What is being suggested here is that information about market needs in fact enters the firm via a number of channels that exist at various levels within the corporate structure. For those channels that feed directly from the external environment to the researcher in the R&D lab the pattern is relatively simple and has been described above. The only caution that should be added at this point is that the researcher himself and the primary group to which he belongs provide some filter-effect in terms of both their technical receptivity to certain needs rather than others, and their subjective perceptions of corporate needs, goals and strategies. Were this the only channel of market-need flow, as one could conclude from the research literature, this would constitute a serious barrier to innovation, especially in light of the fact that most innovations are need-induced.

The dearth of empirical data to the contrary notwithstanding, however, we feel that a number of other channels must be operative. These channels would, of course, involve a two-step flow, from the outside source to a transfer agent within the firm, and then to the researcher in the lab. This would involve an additional filter or screen which not only is not understood at this point, but has not even been identified. That such transfer agents or mechanisms do exist, however, is _prima facie_ plausible in view of unlikelihood of the single-channel alternative.

If such additional channels of information about market needs do exist, this raises another possibility that merits investigation. Perhaps there are key individuals at various levels within the firm that, owing to the richness of their external contacts, perform a gate-keeper function. That is, analogous to the technical information gatekeeper perhaps there are need information gatekeepers. This too would be a quite specialized role, requiring a greater than normal "cosmopolitan" orientation, an extensive network of information sources, and unusually broad experience to detect the market-need signals from the background noise.

The last of these suggested characteristics of our hypothesized market-need gatekeeper clashes in an interesting way with the first. It implies that he must have a fairly strong "local" orientation, since the distinction between market "signals" and market "noise" is largely a function of the condition, policies and needs of the firm. That is, since the needs and opportunities of the market are always far greater than any firm can begin to respond to, the information that is relevant to a firm is that which "fits" its current profile. The rest is noise. The market-need gatekeeper must, therefore, be "local" enough to make these distinctions of relevancy while at the same time being "cosmopolitan" enough to "keep on top of" the external environment. This consideration alone is enough to warrant investigation of this hypothesized function. Market needs mis-guaged, whether by an impoverished network or by inadequate measures of relevancy, means ideas and thus innovative opportunities lost. There is probably no other facet of the problem definition and idea generation phase so badly neglected and thus so poorly understood. And yet most innovations begin here.

Let us assume, now, that by whatever channels are operative a firm has access to the raw data about market needs. We then need to address explicitly the issue of "relevant" data which was introduced above. At least two frames of reference must be distinguished here; relevance from the corporate perspective as determined by top management and articulated in the decisions of the R&D manager, and relevance in terms of the subjective perceptions of the researcher in the laboratory. We will comment on these perspectives only briefly since they will receive detailed treatment in the concluding section.

Galbraith has argued (66) that in dealing with a given external environment and a given set of internal constraints, that a firm faces not one but a rich multiplicity of possible strategies. Whatever the innovation strategy adopted by a firm, it becomes the measure of relevance for market need data. To take but one example, if in a highly competitive market a firm assumes a basically "defensive" posture reacting to the advances of others by small incremental changes in its product line, the market data considered "relevant" will have certain rather predictable characteristics. If, on the other hand, it adopts a more aggressive, "offensive" stance, seeking to gain a competitive edge rather than just holding its own, that which constitutes relevant market data will be rather different. Therefore, the corporate profile, determined partially by the environment within which it operates, partially by its own internal circumstances, and partially by the aims, strategies and structures it chooses, constitutes a major filter for the market need data which flows in (or is sought) by the various channels suggested above. The roles played by individual biasing sets, primary groups, and particular leadership styles, as these operate at the higher management levels and influence the nature of this filter---these can only be guessed at.

The individual researcher's perception of this corporate profile constitutes another significant filter to which information concerning market needs and opportunities is subject before it comes to be an element in the creative process of idea generation. At this point it is more accurate, perhaps, to follow Baker and Freeland (10) and speak of "organizational" rather than "market" needs, since the two have by this point become one and the same. The researcher's subjective perception of these needs is constructed from his own experience, and that of his primary group, with management's reception of ideas that have been proposed in the past. As we shall see in the concluding section, there is impressive evidence to the effect that these perceptions exert a considerable influence upon idea generation behavior.

Section VI. The Flow of Technical Information

The segment of our model representing the key elements of tecnnical information flow is as follows:

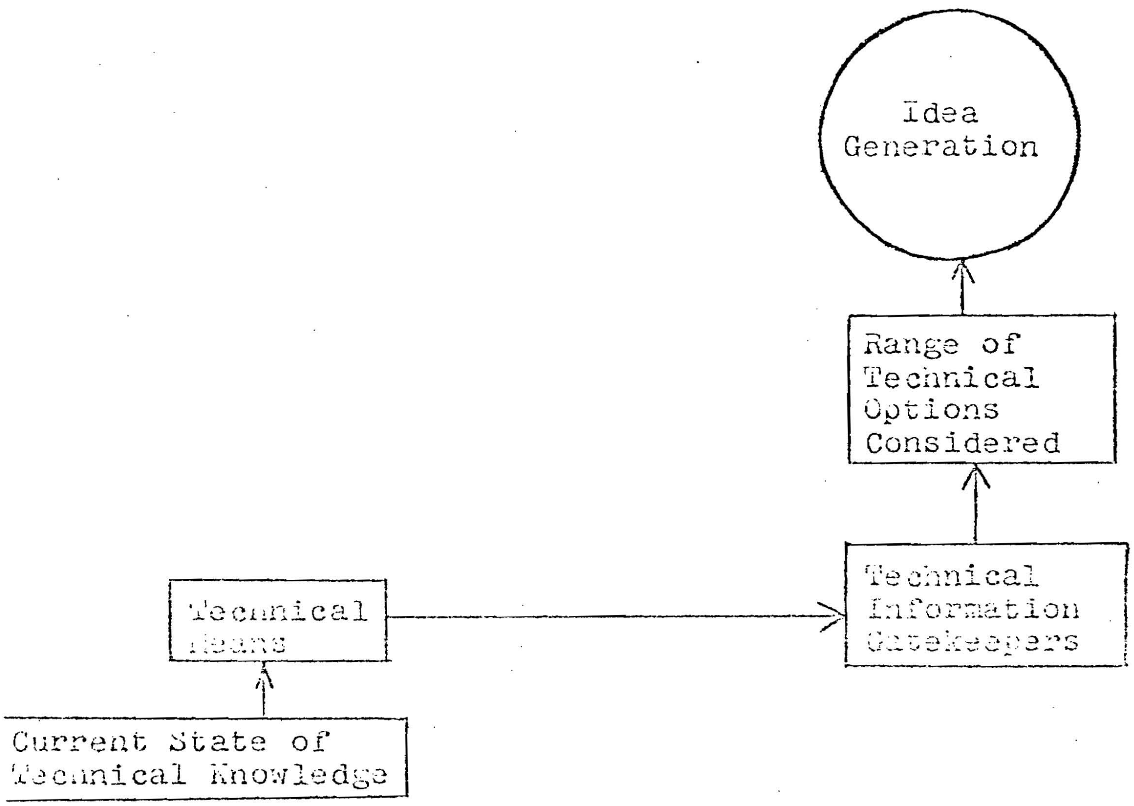


Figure 6.

To begin with the box in the lower left of this figure, it will be recalled that Utterback (11) stressed two crucial environmental boundary conditions for technological innovation. The first, the degree of economic and social utilization of existing technology, was discussed above. The second is the current state of technical knowledge. We will restrict our consideration of this factor to the characteristics of its flow.

The primary means for the transfer of technological information is oral (64, p. 130; 67, p. 1053). The publication of results is less important than in science since the utilization of an innovation is more important to its developers than the information about it. While a journal system exists for technology, it is not cumulative to the degree that scientific literature is (68). Thus the technological researcher both publishes less and finds his professional literature a less rewarding source of ideas than do scientists.

Marquis and Allen (67, p. 1053) have argued that even in his oral communication the technologist differs from the scientist. He is limited by organizational barriers in the formation of invisible college networks analogous to those demonstrated by Price to exist for scientific communities. This limitation results primarily from the mission-oriented nature of the organizations that employ the vast majority of technologists.

> This organizational identification works in two ways to exclude the technologist from informal communication channels outside his organization. First, there are the usual requirements that he work only on problems which are of interest to his employer and, second, that he refrain from early disclosure of his research, to prevent the employer's competitors from profiting from the results. (67, p. 1053).

As evidence of the differences in the information flow patterns, Allen compared the frequency with which ideas were brought to the attention of researchers through various channels in seventeen development and two research projects. These results are as follows:

Table 2.

Sources of Messages Resulting in Technical Ideas Considered during the Course of Nineteen Projects

Channel	Seventeen engineering development projects		Two physics research projects	
	Number of messages produced	Percentage of total	Number of messages produced	Percentage of total
Literature	53	8%	18	51%
Vendors	101	14	0	0
Customer	132	19	0	0
Other sources external to the laboratory	67	9	5	14
Laboratory technical staff	44	6	1	3
Company research programs	37	5	1	3
Analysis and experimentation	216	31	3	9
Previous personal experience	56	8	7	20

(67, p. 1054)

The significance of customers and vendors is evident here, as is the relatively smaller contribution of literature sources.

Even this result concerning reliance on literature sources must be further qualified. The most widely used of all written materials within technology are unpublished or "inhouse" technical reports (67, p. 1055). This seems to be the case because of the proprietary interest that firms have in much of the technical information they produce. The technical report with its limited dissemination serves the need to record such information while restricting its domain of use. While it is difficult to monitor and control the dissemination of such inhouse reports beyond the first user, Marquis and Allen do report the existence of a norm at the interorganizational level. "There seems to be a rather strong norm against the transfer of another organization's reports beyond the limits of one's own organization." (67, p. 1057) Such a norm could only operate to limit dissemination to a <u>third</u> organization, not in the case of a direct flow from one to another.

In fact, the interorganizational flow of unpublished technical reports would seem to be a significant factor in a researcher's effort to stay abreast of his field. As we shall note later this tends to be a two-step flow, mediated by the technical information gatekeeper.

If one were to generalize even further on the above findings, which have been distilled by Marquis and Allen from a wide range of empirical studies, it would be to observe that an "invisible college" does characterize the flow of technological information, but in a subtler sense than Price has demonstrated for science. The subtlety is introduced by the conflicting needs that firms have to protect their proprietary interest in information on the one hand, and to stay abreast of the state-of-the-art by acquiring information on the other. The interesting conflict between these needs is that they can only be satisfied, and then only partially, by trading one off against the other. That is, a firm must give as well as receive in one way or another to maintain a state of the art awareness for itself; but in the giving some proprietory interest must be sacrificed.

The situation is analogous to the college classroom situation in which the grades are determined "by the curve". One is reluctant to share his notes and knowledge widely, since to do so will change the grade distribution to his disadvantage. But strictly on his own he may fall behind the rest of the class and thus fair poorly. Thus he may decide to share, but selectively so as to disturb the distribution as little as possible. And something like the norm described above goes into effect. It is a two-party transfer with an implicit prohibition on wider dissemination for obvious reasons. To complete this little analogy, the grades are not curved in the invisible colleges Price has found in the sciences. There the free flow of information throughout the class (community) helps everyone.

For the technological community the "curve", with a distribution of firms along it, is introduced by the market in which the firm competes. If every competing firm had rapid access to the same new idea and technical capacity then of course none would have an advantage in the market. But if interorganizational flow was greatly diminished by rigid security and sanctions, then all would also suffer since the state-of-the-art knowledge of each would be so poorly maintained.

As with all analogies, this one breaks down if pushed too hard. Perhaps its most obvious point of distortion lies in the implication that firms, like students, might get together and quite consciously, with due assessment, agree to share with each other but not with a third. Such deliberate negotiations with tradeoff agreements would hardly fit the facts. But it must be clear to everyone that such interfirm flow does take place with regularity and if a firm did not see long-term advantage in its continuation, then it would effect the internal security necessary to stop it. In particular cases this does take place.

We would suggest that the means by which the transfer of technical information takes place are quite informal and operate well down in the corporate structure rather than at or near the top. The technical information gatekeepers, who are turned to as resource persons by others in the lab, are primary channels by which the firm gives as well as receives. And like their student counterparts, the arrangements by which they share while disturbing the "curve" as little as possible are informal and personal. They are also primarily oral, though at times involving unpublished reports. For such arrangements to work, the "third-party" norm would have to be respected.

If such a variation on the invisible college hypothesis has substance, it has not been demonstrated, or even suggested in the research literature. But the phenomenon of interorganizational flow of technical information is clear enough so that its existence cannot be doubted. So too is the proprietory interest that organizations have in the information they possess. Likewise, the role of gatekeepers in bringing information into the firm is frequently noted. If the analogy offered is inadequate as an account of the interaction of these known variables, perhaps it will at least serve to stimulate the research necessary to understand it aright.

Before leaving this point, let us add one final consideration. There are items of technical information that are public rather than proprietary, and to which the impetus of the curve does not apply. These are government held patents and technical reports. Utterback reports that they "are seldom used in a commercially or socially important application other than the specific one from which the patent or information arose." (9, p. 8) The explanation for this somewhat curious fact is undoubtedly complex and perhaps lies largely elsewhere. But it is interesting to speculate that perhaps a part of the explanation lies in the very fact that in such instances there <u>is no curve</u>. That is, since such information is equally accessible to all there is particular advantage to none in its exploitation.

It will be recalled from our earlier discussion that the majority of innovations have as their initial impetus the recognition of a market need. In the less common situation a technical capability is first recognized, followed by the search for a need to which it can be applied. In the former or "need/means" pattern there are usually several, oftentimes many, technical alternatives theoretically available. In practice this range is narrowed by several considerations, perhaps the most significant being individual and group biasing-sets, as discussed above. The technical gatekeeper also plays an important role here, in directing attention towards certain of the possible approaches.

In the latter or "means/need" pattern the process begins with a particular technical capacity, and the search is among a range of market needs. An interesting question here concerns the nature of certain technical capacities that makes them attractive enough to stimulate the search for a need. Utterback suggests that, "Older technical possibilities seldom attract attention spontaneously." (9, p. 8) On the other hand, a new discovery or technical possibility often does attract the researcher's attention and thus stimulate the search for applications. Thus <u>newness</u> would seem to be a characteristic of technical means that initiate the process. There is also evidence that "the larger technological changes tend to be of the 'discovery push' type." (4, p. 75) That is, innovations stimulated by the recognition of a need are more often smaller and more incremental, while those stimulated by a new discovery or capability are of greater magnitude and constitute greater discontinuities with the past.

One should not automatically assume, however, that these larger technological changes have their origin in a scientific discovery. Price (68) in fact argues that science and technology develop quite independently of one another, with communication being limited to that which occurs in the education of technologists. Marquis and Allen (67) agree with this general assessment, but offer two refinements of it. First they note that:

> Occasionally technology encounters a problem blocking its advance, the removal of which requires a fundamental understanding of the scientific basis of the phenomena involved. In this way, science discovers voids in areas which have been bypassed by the research front. Quite frequently, too, difficulties encountered during the research process in a seemingly unrelated area will reveal a gap in the understanding of a basic segment of science. Here again, in a sense, application helps to determine the direction or priorities in scientific investigation. (67, p. 1057)

Most cases of technological advance are not of this sort; they neither require information from the forefront of science nor define a problem there. But when technological advance does require a "gap-filling" contribution from science, "the communication is bilateral, direct, and quite rapid." (67, p. 1059)

The second enrichment of Price's general hypothesis involves a distinction among technological areas. Some, perhaps electronics, are probably more closely related to work on the frontiers of science than others, for instance mechanical technology. Neither of these refinements, however, disturb the general point that there is very little direct dependence of technological innovation on science. As science builds on earlier science, so technology builds on previous technology.

Let us turn now to the technological gatekeepers whose role in the flow of information has been anticipated several times above. Allen and Cohen hypothesize the existence and function of such individuals as follows: "There can exist in an R&D laboratory certain key individuals who are capable of effectively bridging the organizational boundary impedance and who provide the most effective entry point for ideas into the lab." (27, p. 6) Such individuals can be characterized in several ways. First, as opposed to most individuals within the lab who have few contacts outside, the gatekeepers' contacts are extensive. There is some evidence that such gatekeepers do not all tend the same gate (or all gates) with equal facility. Some concentrate more on the technical literature, while others maintain a richer network of informal contacts outside of the organization. Given the dominance of oral communication noted previously, the latter would seem to provide the more fruitful information channels. Allen and Cohen, however, provide a finer-grained distinction which is worthy of note:

> The literature has been shown to provide information which is important for keeping abreast of the state of a technological field, while oral sources are probably better in providing more specific detailed information about particular techniques. Gatekeepers who specialize in knowledge of the state-of-the-art would then tend to expose themselves more to the literature, while those specializing in particular research techniques would interact more with external oral sources. (27, p. 24)

This distinction between written and oral sources and specializations may prove valuable, but one must wonder if it is quite so clear-cut as it may seem.

A second and obviously related characteristic of the gatekeeper is that he is the individual to whom others in the lab turn most frequently for technical advice and discussion. Unfortunately Allen and Cohen do not relate this point as clearly as one would wish to the role of the primary group in attitude reinforcement and change. The gatekeepers are referred to as the "sociometric stars" in the sense of the frequency with which others choose them for technical discussion. In addition, two of them were responsible for introducing into the lab the four ideas that were almost unanimously picked as the most important ideas of the previous year. But as Allen and Cohen point out, their data was not sufficiently detailed to reveal the direction of the causal arrows. Additional research is needed to determine the extent to which the gatekeepers mold the attitudes of the primary group or vice versa.

Further, the research literature is silent on the related question of the extent to which the gatekeeper serves, not only as a channel of information from the outside, but also as a filter for such information. This point has been raised above as to the influence of the mind-sets or biases to which the gatekeeper is subject owing to his past experience. It also needs to be investigated.

The final point to be made concerning the gatekeepers has also been anticipated above, but apparently not been dealt with in the literature. We have suggested that gatekeepers not only bring information into the firm from the outside, but also share their firm's information with their contacts in other firms. That is, they are not only _receivers_ as has been previously noted, but also _transmitters._ At this point the evidence for such a hypothesis is not empirical but purely logic-based speculation. To review the argument here, at least some and perhaps most of a gatekeeper's contacts outside the firm would be those who perform a similar function in other firms. If this is so, then the needs of all parties would be similar and the relationships by necessity would be reciprocal, i.e., information would have to be _exchanged._ If one party sought to _receive_ only, and never _give_, the informational needs of his contacts would not be met and the relationships would quickly break down. The research agenda suggested by this point rests on the _prima facie_ possibility that there exists for technology a communications network, or "invisible college," analogous to that which has been demonstrated for science, but with quite distinctive characteristics owing to the influence of the market.

Before proceeding to the next topic, mention should be made of another avenue by which technical information enters the firm. Burns (69, p. 12) has concluded that the transfer of technical information is the work of "agents not agencies", and one of the most efficient forms of this transfer is "on the hoof". That is, an individual researcher joining a firm brings with him not only a certain level of training and technical competence, but also a store of ideas and technical information acquired in his previous work settings. As to the importance of such ideas and information to his new employer, Langrish et al found that, "In the cases we have studied, the most frequent single mode of technology transfer was by a person joining a firm." (4., p. 44; see also Table 7, p. 79).

The final topic in this section is the range of technical options that are considered by an individual researcher or laboratory in generating an idea. This topic in effect forces a recapitulation of much of the foregoing since it points to the discrepancy between the current state of relevant technical knowledge and the subset of such knowledge actually considered in a particular instance. We will be quite brief in summarizing the influences that can contribute to this discrepancy. First, however we should note that we are here concerned only with those cases that begin with the recognition of a need. Those that are means-induced obviously deal only with a single technical approach. How the search for a market need is conducted in such cases is an interesting and unexplored question but it lies outside our present focus.

The number of technical options that are actually considered as possible solutions to a market need is a function of a wide range of variables. The perceived nature of the need itself is the primary filter. Then since most of the firm's store of relevant technical information was not generated within but enters from the outside, and primarily by oral channels, the richness and state of repair of its communications network is of enormous influence. If its gatekeepers are particularly able and if the filters introduced by their own biases are not in this instance counterproductive, then the range of technical options may be quite large. If, in addition, new people with relevant experience have entered the lab recently then the range could be increased even further. If consultants are available at this early stage this again can be a positive factor.

Anticipating the next section, if the time pressures associated with current project work is not so heavy as to preclude the exploration of a number of possibilities for a new idea, then clearly the number that can be considered will increase. Related to this point, if the firm has established mechanisms for the explicit purpose of encouraging new ideas (see the description of one firm's "Idea Generation Groups" in 57), then the range explored should be larger. This should also be the case if the firm's reward structure is such as to recognize and reward effort spent in idea generation activities as well as effort directed to other responsibilities.

In another dimension of our earlier concern, the number of technical options will vary with the composition of both the work group concerned and the primary socialization groups within the lab. With the former the concern is with the degree of homogeneity of attitudes that prevails towards the efficacy of certain technical approaches. In the latter instance it is a matter of the extent to which the individual's mind-set or technical bias is either shaped or reinforced by the primary social group of which he is a member. And, finally, the number of technical options that receive serious consideration is also a function of the mind-sets of individuals, especially those who enjoy status as opinion leaders.

This check-list of screens or filters that can function to widen the gap between the technical options that exist and those that actually come to be considered in a particular case is on the face of it quite foreboding. The actual extent of the counter-productive effect of these filters individually and in concert will, of course, vary with the nature of each need that occasions a search and over time. Minimizing their counterproductive effects is a task which accrues to those in supervisory positions within the lab. In the face of such complexity words of wisdom take on the hollow ring of folly. So we will offer none. But the research community can offer something far sounder than platitudes, namely a greater understanding than we now possess of this range of variables and their interaction.

To date the search for this understanding has just begun.

Section VII: Post-Idea-Generation Filters and Linkages.

In terms of the process phase boundaries we have delineated, the problem definition and idea generation phase of the process of technological innovation ends with an idea for a new product, device or process. But the functional boundaries of this process phase will be extended somewhat to include certain other activities that must take place before an idea can achieve project status and the next phase can begin. We have noted earlier that Rubenstein (13) takes a new idea as potential proposal for work requiring a substantial commitment of resources. Until an idea is actually communicated to management it is not a proposal and thus there is no linkage between this and later process phases. Considerations determining the submission or non-submission of ideas thus constitute an additional filter-point in the process. Further, once a proposal has been submitted there exists yet another filter. Management, by whatever criteria it takes as appropriate, must accept, reject, shelve, or otherwise decide what to do with the idea. Only if it is accepted and thus funded does an idea achieve project status and enter the next phase.

The movement of an idea through these inter-phase linkages and the influences upon these filters may be represented as follows:

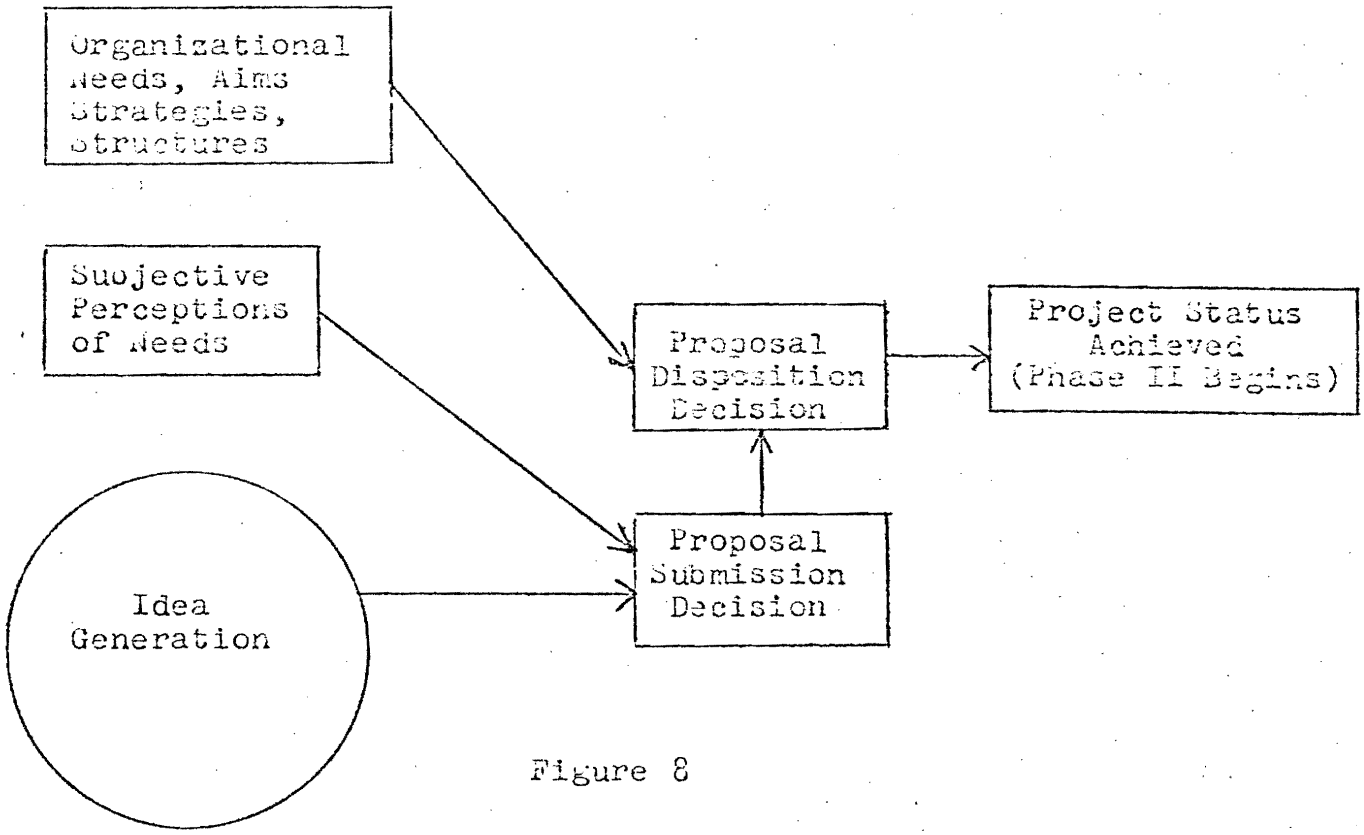


Figure 8

There are some research results (70, 71) which indicate that more ideas are generated in the context of R&D labs than are submitted to management for review. This is perhaps not surprising. An additional finding is surprising, however. In one study a mechanism (Idea Generation Groups) was developed within the lab whereby ideas previously generated but not submitted came to be submitted for management review. When these previously unsubmitted ideas were compared with other ideas submitted in the normal course of operation, the former contained a significantly higher percentage of "good" ideas. These evaluations were initially obtained from a panel of qualified judges who subjectively rated both the previously unsubmitted and the "control" ideas. They were independently confirmed by the subsequent dispositions made of ideas in both sets after management review.

> The managerial review decisions also indicated a relatively high percentage of quality ideas in the set of "not submitted" ideas. Perhaps the most startling finding was that 38% of the ideas achieving project status came from the "not submitted" set which contained only 15% of the total ideas. (10, p. 107)

The distribution of the ideas examined in this study, by their original status and subsequent disposition by management was as follows:

Table 3

Disposition Decision / Status	Project Status	Shelved	Communicated, No Response	Rejected	Totals
Not Submitted	11	6	4	26	47
Control	18	43	18	177	256
Total	29	49	22	203	303

> where
> "Project Status" - idea became new project or task in an on-going project.
> "Shelved" - management decided to postpone the disposition decision.
> "Communicated, No Response" - idea judged to be more relevant to another company division and was communicated, but there has been no response.
> "Rejected" - idea was rejected as a new project or task.
> For all three sets of data, the hypothesis that the distribution of ideas into categories is independent of the status can be rejected at the 0.05 level of significance by a chi-square test of independence. (10, p. 108)

As at least a partial explanation of these results Baker and Freeland point to the following characteristics of the corporate R&D environment. The mechanisms for the periodic review of a researcher's achievements and for rewarding such achievements in highly visible ways are well developed in the case of current project activity. Such mechanisms have the effect of focusing the researcher's attention on the current project. On the other hand, the generation of new ideas is often not viewed as, "an effective strategy for obtaining organizational rewards because the mechanisms for scheduling review and measuring achievement are less well defined." (10, p. 110) In addition to these biasing pressures towards one's current project as opposed to ideas for new ones, the researcher's subjective perception of need and thus what likely is to be viewed as a "relevant" idea is also a factor in his decision to submit or not submit an idea.

In order to gain an empirical measure of such factors Baker and Freeland developed case histories for the previously unsubmitted ideas uncovered by the "idea generation groups" they studied. The contributing factors cited, the frequency of their occurence, and the independent evaluation of the quality of these previously unsubmitted ideas is summarized as follows:

Table 4

Summary of Factors Resulting in Ideas Not Submitted

Factors	N	%	Idea Ratings		
			Fair, Poor	Good, Excellent	Best
Time Pressures	28	60%	14	14	4
Anticipated Negative Evaluation from Management	4	9%	0	4	2
Negative Evluation from Peers	3	6%	1	2	1
Negative Evaluation by Group Leader	2	4%	1	1	1
Previously Rejected by Management	2	4%	2	0	0
Submitted, No Response	8	17%	1	7	5
Total	47	100%	19	28	13

(10, p. 111)

From this study, then, it is clear that the pressures to perform on the current project, reinforced by more fully developed review procedures and the greater certainty of reward, is the strongest influence in the researcher's decision to not submit ideas he has had. Perhaps the objection might be raised that if one has an idea it really takes little time to present it. But an idea must at least be developed well enough to permit management to evaluate it. This calls for either the exploration of the feasibility of one or more technical approaches on the need/means pattern, or the search for an application on the means/need pattern.

And in both cases support must be mustered for the relevance of the idea. In short, an idea is a <u>potential</u> proposal, and its submission requires the form of an <u>actual</u> proposal. One is reluctant to go with a "half-baked" idea, especially if the rewards tend to lie elsewhere.

This brings us to the next screen in the process which is the evaluation and disposition of ideas that are submitted. Baker <u>et al</u> (71) report a study in which management criteria for dealing with new ideas were compared with the criteria employed by researchers in submitting ideas. The concept of relevance was operationalized in terms of two independent variables.

> 1) <u>urgency</u> - immediacy of the need, problem or opportunity toward which the idea is directed; and
>
> 2) <u>predictability</u> - the degree of certainty with which the methods and procedures for researching the idea are known.
>
> A third independent variable, time horizon, was added in the belief that it considered aspects of both urgency and predictability and, yet, also included additional considerations:
>
> 3) <u>time horizon</u> - the expected length of time from initiation to completion of the research activity. (17, p. 119)

It was found that the ideas that were given a very high rating by researchers tend to be highly predictable and have a short time horizon, while those most favorably received by management were directed at the most immediate needs, problems or opportunities. Thus the major influences on the researchers' perceptions were different from the major influence on management's decision. In spite of this difference in the evaluative criteria employed, however, the disposition decisions and subjective evaluations were highly correlated. "Ideas that are given high subjective evaluations by researchers and technicians are more likely to receive favorable idea disposition decisions from management." (17, p. 120) It should be noted here that the management decisions were made without knowledge of the subjective evaluations gathered in this study.

The data also indicates that while the primary management criterion is urgency, there is tradeoff behavior between urgency and predictability. Within limits less urgent ideas are more likely to be accepted if they are highly predictable, and conversely, fairly unpredictable ideas are more likely to be given project status if the need is urgent. Interestingly none of the ideas that were rated as fully predictable were given project status. It was suggested that perhaps fully predictable ideas were not considered new or interesting enough to carry forward. In view of a previous discussion, we might suggest that ideas that are viewed as fully predictable might also be considered as more or less common property among the firm and its competitors and thus as providing no competitive advantage which would justify its development.

From the point of view of maintaining (or increasing) the flow of new ideas, it is probably not important that the researchers and management are employing different criteria for evaluating ideas, since these criteria can be thought of as covariants. What is important, however, is a possible implication of the evidence in this study that management hedges its bets by shelving borderline or marginally acceptable ideas. While this is a reasonable strategy in itself, it is important that such indefinite deferrals along with the reasons for them be communicated to the researchers who generated such ideas. No response can be viewed as an unfavorable response. The cumulative effect of such perceived unresponsiveness can be a decrease in the rate of idea submission.

To sum up this discussion of the linkages of problem definition and idea generation with the next process phase, it is clear that creative activity leading to new ideas is not sufficient from the organizational viewpoint. Such ideas must also be submitted for review and decision. Non-submission can result from a range of factors, all of which relate directly to the researcher's subjective perceptions; perceptions of "the way things are" regarding the corporate review and reward mechanisms, corporate needs, objectives and circumstances, the predictability of success that an idea possesses and the time normally allowed for the completion of such projects, past receptivity of management to new ideas, etc. Such perceptions are not accessible to direct control by management. They are, however, sensitive to changes in the immediate environment; changes which a perceptive management can, oftentimes, effect.

Once an idea has been submitted, the process does become amenable to direct action. Proposals can be weighed against one another, against the availability of both resources and necessary competence, against corporate needs and objectives, etc. But in this process, too, the perceptive manager will be sensitive and responsive to the impact of his decisions and behavior on researcher perceptions.

In conclusion, we have stressed throughout the rich and subtle network of environmental influences that enhance or inhibit problem definition and idea generation, and in turn hone or blunt a firm's competitive edge in technological innovation. In attempting to shape this environment there are few reliable rules, but a methodological reminder is appropriate. The technological tradition is strongly oral. In the experience related by Hyman earlier, when the R&D manager finally respected and utilized this tradition he got two quick responses that both ring true. First, his research group said, "Well, if that's what he wants, why didn't he say so?" And secondly, they produced a wealth of new ideas. Too simple, of course, but instructive.

REFERENCES

1. C. Kluckhohn, <u>Mirror for Man</u> (New York, 1949).

2. R. Hyman, "Creativity and the Prepared Mind: The Role of Information and Induced Attitudes," in C. W. Taylor, ed. <u>Widening Horizons in Creativity</u> (New York, 1964).

3. J. Jewkes, D. Sawers and R. Stillerman, <u>The Sources of Invention</u>, 2nd ed. (New York, 1969).

4. J. Langrish, M. Gibbons, W.G. Evans and F.R. Jevons, <u>Wealth from Knowledge</u> (London, 1972), p. 14.

5. Langrish <u>et al</u>, p. 14.

6. J. Rossman, Industrial Creativity: <u>The Psychology of the Inventor</u> (New Hyde Park, N.Y., 1964).

7. A.N. Whitehead, Science and the Modern World (New York, 1925), p. 91.

8. Langrish <u>et al</u>, p. 12.

9. J.M. Utterback, "Innovation in Industry and the Diffusion of Technology," Working Paper, <u>Graduate School of Business</u>, <u>Indiana University</u> (Bloomington, 1973).

10. N.R. Baker and J.R. Freeland, "Structuring Information Flow to Enhance Innovation," <u>Management Science</u>, 19, no. 1 (September, 1972).

11. J.M. Utterback, "The Process of Technological Innovation Within the Firm," <u>Academy of Management Journal</u>, 14, no. 1, (March, 1971), p. 85.

12. Utterback, 1973, p. 2.

13. A.H. Rubenstein, "Organizational Factors Affecting Research and Development Decision-Making in Large Decentralized Companies," <u>Management Science</u>, 10, no. 4 (July, 1964).

14. V.H. Whitney, as quoted in G. Gordon, S. Marquis, and O.W. Anderson, "Freedom and Control in Four Types of Scientific Settings," <u>American Behavioral Scientist</u>, 6 no. 4 (December, 1962), p. 39.

15. G. Gordon and S. Marquis, "Freedom, Visibility of Consequences and Scientific Innovation," <u>The American Journal of Sociology</u>, 72, (September, 1966).

16. Gordon and Marquis (1966) p. 197.

17. Gordon and Marquis (1966) p. 199.

18. Gordon and Marquis (1966) pp. 199-200.

19. Gordon and Marquis (1966) p. 201.

20. S.C. Hill, "A Natural Experiment on the Influence of Leadership Behavior Patterns on Scientific Productivity," IEEE Transactions on Engineering Management, EM-17, no. 1 (February, 1970), p. 11.

21. Hill, 1970.

22. D.C. Pelz, "Some Social Factors Related to Performance in a Research Organization," Administrative Science Quarterly, 1 (1956).

23. Hill, 1970, p. 12.

24. D.C. Pelz and F.M. Andrews, Scientists in Organizations: Productive Climates for Research and Development (New York, 1966), p. 33.

25. F.M. Andrews and G.F. Farris, "Supervisory Practices and Innovation in Scientific Teams," Personnel Psychology 20, no. 4 (1967).

26. Andrews and Farris (1967), p. 513.

27. T.J. Allen and S.I. Cohen, "Information Flow in an R & D Laboratory," Working Paper, Sloan School of Management, M.I.T. (Cambridge, 1966).

28. Allen and Cohen, (1966), p. 21.

29. Hyman, (1964), p. 70.

30. Allen and Cohen, (1966).

31. Allen and Cohen, (1966), p. 6.

32. Allen and Cohen, (1966), p. 7.

33. Allen and Cohen, (1966), p. 21.

34. Allen and Cohen, (1966), p. 21.

35. Allen and Cohen, (1966), p. 21.

36. Allen and Cohen, (1966), p. 21.

37. Allen and Cohen, (1966), p. 21.

38. J. M. Utterback, in G. W. Wilson, ed., Technological Change and Economic Growth (Bloomington, 1971), p. 139

39. D. H. Peters, The Incidnce and Exploitation of Commercial Ideas in University Departments and Laboratories (Doctoral dessertation, MIT Sloan School of Management, Cambridge, 1968).

40. L. V. Morse and B. Gordon, "Creative Potential and Organizational Structure," Proceedings of the Academy of Management (1968 p. 37

41. Langrish et al, (1972) p. 67

42. Baker and Freeland, (1972), p. 111.

43. T. J. Allen and D. g. Marquis, "Positive and Negative Biasing Sets: The Effects of Prior Experience on Reach Performance," IEEE Transactions on Engineering Management, EM-11 No. 4 (December, 1964), p. 158.

44. H. G. Birch and H. S. Rabinowitz, "The Negative Effect of Previous Experience on Productive Thinking," Journal of Experimental Psychology 41 (1951).

45. Birch and Rabinowitz, (1951), pp 122-123.

46. A. S. Luchins, "Mechanization in Problem Solving," Psychological Monographs 54 No. 248 (1942).

47. N. R. G. Maier, "Reasoning in Humans," in T. L. Harris and W. E. Schônn, eds., Selected Readings on the Learning Process (New York, 1961).

48. Allen and Marquis, (1964), p. 161.

49. S. J. Parnes and A. Meadow, "Development of Individual Creative Talent," in C. W. Taylor, ed., Scientific Creativity: Its Recognition and Development (New York, 1963), p. 315.

50. S. J. Parnes, "Reasearch on Developing Creative Behavior," in C. W. Taylor, ed. Widening Horizons in Creativity (New York, 1964), p. 149.

51. S. J. Parnes, (1964), pl 150.

52. J. M. Utterback, (1971).

53. J. M. Utterback, (1971), p. 80.

54. J. Schmookler, Invention and Economic Growth (Cambridge, 1966).

55. J. L. Enos, "Invention and Innovations in the Petroleum Industry," in R. R. Nelson, ed. The Rate and Direction of Invention Activity (Princeton, 1962).

56. J. M. Utterback, (1971), p. 81.

57. N. R. Baker, J. Siegman, and A. H. Rubenstein, "The Effects of Perceived Needs and Means on the Generation of Ideas for Industrial Reasearch and Development Projects," IEEE Transactions on Engineering Management, EM-14, No. 4 (December, 1967).

58. C. F. Carter and B. R. Williams, Industry and Technical Progress: Factors Governing the Speed of Application of Science (London, 1957).

59. J. D. Goldhar, An Exploratory Study of Technological Innovation (Doctoral Dissertation, George Washington University, Washington, D. C. 1970).

60. C. W. Sherwin and R. S. Isenson, "Project Hindsight: A Defense Department study of the Utility of Reach," Science, 156 (1967).

61. J. Langrish, "Technology Tansfer: Same British Data," R&D Management 1, No. 3 (1971).

62. S. Myers and D. G. Marquis, "Successful Commercial Innovations," National Science Foundation report 69-17 (1969).

63. M. Tannenbaum, et al, "Report of the Ad Hoc Committee on Principles of Reasearch-Engineering Interaction," National Academy of Science--National Research Council Materials Advisory Board, MAB 222-M (1966).

64. J. M. Utterback, The Process of Innovation in Instrument Firms, (Doctoral dissertation, MIT Sloan School of Management, Cambridge, 1969).

65. J. M. Utterback, "The Process of Innovations: A Study of the Origination and Development of Ideas for New Scientific Instruments," IEEE Transactions on Engineering Management EM-18 No. 4 (November, 1971), p. 129.

66. J. Galbraith, "Environmental and Technological Determinants of Organization Design," in J. W. Lorsek and P. R. Lawrence, eds., Studies in Organizational Design (Homewood, Ill., 1970).

67. D. G. Marquis and T. J. Allen, "Communication Patterns in Applied Technology," American Psychologist 22 (1967).

68. D. J. de S. Price, "Is Technology Historically Independent of Science?" Technology and Culture 6 (1965).

69. T. Burns and G. M. Stalker, The Management of Innovation (London, 1961).

70. N. R. Baker, "The Influence of Several Organizational Factors on the Idea Generation and Submission Behavior of Industrial Researchers and Technicians," (Doctoral dissertation, Northwestern University, Evanston, 1965).

71. N. R. Baker, J. Siegman and J. Larson, "The Relationship between Certain Characteristics of Industrial Research Proposals and Their Subsequent Disposition," IEEE Transactions on Engineering Management, EM-18 No.4 (November, 1971).

G-02.602

October 1973
Morris Mitzner

THE DIFFUSION PHASE OF THE INNOVATIVE PROCESS

Table of Contents   Page No.

The Diffusion Phase of the Innovative Process

Introduction

The diffusion studies in the innovation literature are characterized by either an interminable flow of words or by an absence of definition. Semantic chaos emerges from the absence of convention among diffusion researchers as to the nature and parameters of the diffusion process. This in turn can be traced to the differential backgrounds and interests of the researchers.

This paper will attempt to assess the state of our current understanding of the diffusion phase of the innovative process by focusing attention on a number of relevent items.

a. The proliferation of diffusion definitions.
b. The specialized concerns of researchers that guide diffusion conceptualizations.
c. The empirical findings, generalizations, and operational models of some systematic researchers.
d. The linkages and possibilities for synthesis in the work of systematic researchers.
e. Suggestions for further investigation.

As the diffusion of innovation makes deeper inroads into social life, conceptual study becomes more urgent. Schemes for investigating, diagnosing, and acting on the human-social problems arising in a variety of settings proliferate. But first the diffusion process itself must be understood. Questions must be asked before they can be answered, and asking meaningful questions is often far more difficult than answering them. This is especially true for administrators who live and work among the phenomena of organizations. They are so much in and of the stream of happenings that it is difficult for them to achieve a perspective on the patterns underlying the events. Without this perspective they may find that time and again they have addressed symptoms rather than actual causes. Thus, there is a role for concepts in ordering and giving coherent meaning to the various facets of technological innovation. This paper then will address itself to the phenomenon of diffusion in the technological innovative process.

Proliferation of Diffusion Definitions

An examination of the meanings assigned to the term diffusion by a cross-section of researchers in the field of technological innovation highlights one of the difficulties in assessment. Hagerstrand presents it as a process of permeation and transformation:

> ...If in a subregion of the system a hitherto unknown element is introduced, say, for example, a new technical device, a new way of alloting social roles, or a new cultural manifestation, this event constitutes a perturbation that under certain conditions may be transmitted out into the surrounding regions and propagate itself until eventually the whole system has become permeated and at the same time to some degree transformed. A permeation of this kind, either partial or total, is known as a diffusion of innovation...(1)

Rogers with Shoemaker (1971) conceive of diffusion in terms of the communication of new ideas.

> ...diffusion is the process by which new ideas are communicated to the members of a social system... (2)
> ...Diffusion is the process by which innovations spread to the members of a social system...(3)

Whereas Hagerstrand sees diffusion as some kind of "disturbing"event that permeates and to some extent transforms a whole system, spatially, Rogers and Shoemaker conceptualize diffusion as a subset of communication research concerned with messages that are new ideas. Mansfield, et al, (1971) conceptualizes diffusion as a learning process.

> ...Diffusion is essentially a learning process where the learning takes place among a considerable number of users and producers...(4)

Morgan (1972) disucsses diffusion in terms of transfer of technology from one country to another.

> ... the process through which the production system of a country (public and private enterprise) acquires a technology produced in another country for incorporation in those enterprises. (5)

Fredrikson (1970) is also interested in technology transfer as a form of diffusion, but his interest differs from that of Morgan. He is concerned with the utilization of technology in products or processes beyond the scope of the original development. (6) Utterback (1971) conceives of diffusion as a two part mechanism by means of which an innovation has both social and economic impact.

> ...Diffusion is the mechanism of communication and use by which an innovation comes to have a wider social and economic impact. (7)

Although Mansfield, Morgan, Fredrikson and Utterback are interested in the same problem area - the diffusion of technological innovations - their individual definitions of diffusion reflect selective concerns with problems in this general area. These concerns are psychological, political, technical and economic.

Alesch (1971) also has concern with technology transfer as an aspect of technological diffusion. His definition is somewhat similar to Fredrikson's.

> ...Technological transfer is the application of technologies developed elsewhere to appropriate and comparable situations...(8)

Robertson (1971) reflects still another interest in his definition of diffusion - the adoption of new products and services as a result of marketing activities. Nevertheless the definition is both broad and vague.

> ...Diffusion is the process by which something spreads from its source of invention to its ultimate users or adopters...(9)

and again

> ...Diffusion is (1) the adoption (2) of new products and services (3) over time (4) by consumers (5) within social systems (6) as encouraged by marketing conditions. (10)

Gruber (1969) earlier appears to combine the interests of Morgan, Fredrikson and Utterback and defines diffusion as the "multiplication and application in other physical, institutional or economic settings than the original." (11)

Pavitt (1970) is primarily interested in performance in technological innovation. He defines diffusion as "a process - after innovation - whereby a new or better product or process is produced by a wide number of firms." (12) He then goes on to say that diffusion can take place within a country or between countries.

Crane (1972) is interested in the growth of knowledge in scientific communities that seems to take the form of the logistic curve. She defines diffusion as "a kind of contagion effect that occurs in which individuals in a social system who have adopted an innovation influence those who have not yet adopted it." (13)

Any attempt to aggregate the definitions of the 13 aforementioned diffusion researchers in order to "distil the one definition" would lead to even greater confusion. Utterback (1973) in a state-of-art survey of factors affecting innovation in industry and diffusion of technology succinctly points out that past work even in the one context (industry) has been of a descriptive, non-cumulative and non-comparable nature. (14) This holds true as well for diffusion studies in a variety of contexts. The diverse orientations of the researchers, the special purposes of the various diffusion studies and the non-random heterogeneous nature of the samples from which empirical findings are derived frustrate the development of common definitions. It is therefore perhaps more useful to look at the specialized concerns of researchers that guide their diffusion conceptualizations.

Specialized Concerns of Researchers that Guide Diffusion Conceptualizations and Cross-Disciplinary Findings

The orientations and experiences of diffusion researchers are reflected in their concerns, in their conceptualizations, and in their findings.

Kranzberg's comments and citations on diffusion in the Ecology of Innovation support this contention. (15) He points out that each field of study selects those aspects of the diffusion process which correspond to its special concerns.

a. Sociocultural Resistance to Change

Anthropologists and sociologists focus on sociocultural resistance to change. Kranzberg cites Rogers, Diffusion on Innovations, 1962 (16) and Homer G. Barnett, Innovation, The Basis of Cultural Change. (17) To those two works may be added the synthesis of 1500 publications in the form of 103 generalizations in the recent work of Rogers with Shoemaker, 1971, Communication of Innovations: A Cross-Cultural Approach. (18) Rogers and Shoemaker's special concerns in the diffusion process are innovativeness, rate of adoption, and relative success of programs of change. The writers use a propositional inventory approach to the existing diffusion literature (through 1969). They assume that each research publication is an equivalent unit of analysis in synthesis. Since the studies are primarily descriptive and non-cumulative in nature, this assumption is open to question. Furthermore, there are difficulties regarding comparability of results and bias in the samples which form the base for the empirical findings. This observation is not to negate the utility of the 103 generalizations. They are however essentially heuristic at this stage of overall theory construction and are phrased in simple bivariate form. (See notes for examples). (19) They are somewhat prone to the logical fallacy of affirming the consequent. However, with proper modifications in appropriate <u>multivariate</u> research designs, they have the potential for shedding light on the multi-faceted nature of the diffusion process. (Rogers systematic work will be discussed in greater detail, later in the paper.)

It is worth noting that Rogers special interest in innovativeness, rate of adoption, and relative success of programs predates his professional experience as a rural sociologist. As an agronomist, Rogers discovered that his home community resisted his stock of carefully researched and "well-intentioned" agricultural innovations. His ensuing frustration probably served to guide him into a life-long investigation of the diffusion process. He was further driven by the fact that by the late fifties the average U.S. farmer could support 50 other persons rather than 27 if he adopted already developed innovations. (19) It is apparent from the foregoing that not only does a particular field of study select those aspects of the diffusion process which correspond to its special concerns, but that the selection process is further modified by past as well as present experiences of individuals in a given field. The perceptual and experiential nature of the selection process impedes or at least makes more difficult the development of common definitions.

b. Spatial Diffusion

Geographers are concerned with spatial diffusion, and specifically the relationship between innovativeness of adopters and physical space. Hagerstrand (1968) points out that the diffusion of innovation shows three stages of growth in the spatial distribution of adopters. In the initial stage adopters are usually concentrated in a small cluster or a set of clusters. In the intermediate stage, expansion likewise takes place in a pattern that indicates that a new adoption is more likely to occur in the vicinity of existing adoptions than farther away. The "neighborhood effect" creates an outward movement along a more or less sharply defined frontier, while at the same time the general density of adoption behind the frontier is continuously growing. A saturation stage may be reached in the central area of dispersal while the frontier is still growing. If the number of individual adopters are plotted over time, an S-shaped curve normally appears. This curve shows a slow take-off stage of varying length, an intermediate stage of more rapid development, and a final stage of declining growth, which seems to approach a ceiling asymptotically. The spatial and temporal aspects of this pattern seem to apply to various categories of innovation adopter units - individuals, villages, cities, and firms. (21)

Hagerstrand feels that personal communication between pairs of individuals and direct observation are still the basic instruments for the diffusion of innovation. In turn, the fact that the spread of innovation from certain centers tends to follow repeatedly the same spatial course seems to indicate that the communication network has a very stable configuration over time. The frequency of contacts between areas and places remains very much the same through time. (22)

Hagerstrand has pioneered in simulation studies of spatial diffusion of innovations. (23) In particular he has utilized Monte Carlo game theory techniques to simulate the spatial diffusion and adoption of farm innovations. In one particular instance he studied the spatial pattern in the adoption of subsidized pasture improvement innovations in Sweden. This covered a period of 20 years. He used Monte Carlo simulation techniques based upon the probability assumptions of "neighborhood effect". He then compared this with empirical data and came to the conclusion that not only does spatial proximity increase the probability of adoption but that simulation techniques and the model of S-curve growth are useful predictors of spatial diffusion of innovations. (24) This type of simulation of agricultural innovations has since been repeated by Hanneman (1969) in Columbia (25).

Hagerstrand appreciates the limitations of his "neighborhood effect" model and acknowledges that there are "receptivity factors" which affect the spatial pattern and rate of adoption of innovations. These factors include cost, returns, attitudes, predispositions and value systems. These latter must be taken into account. (26) However, it would seem that these latter characteristics are not as amenable to simulation techniques as physical space. Yet they are the modifiers of the "neighborhood effect" and need to be considered in a multivariate research design of the diffusion process. They embody the dynamics of resistance to change. (For a comprehensive understanding of innovation diffusion as a spatial process see Hagerstrand 1952 (27) and Hagerstrand 1967 (28)).

Brown, an urban geographer and economist is concerned that the Hagerstrand model of spatial diffusion deals only with adoption. Hagerstrand's information flow model would suffice for situations involving only adopters. (29, 30) There is a hierarchy of networks of social communication through which an innovation filters from a few innovators to the general population. The basic tenet of Hagerstrand's conceptualization of the spread of innovation across the micro scale landscape according to Brown is that the adoption of an innovation is primarily the outcome of a learning process. This implies that operationally only factors related to the effective flow of information need be considered.

Brown as an <u>urban geographer</u> questions this. There are market factors that must be considered along with information (network) factors. The market factors would include the distribution policy of the propogator of the innovation. It would also include the shopping behavior of potential adopters determining markets at which they trade. Brown and Cox (1971) indicate there is a difference between situations in which there is a propagator(s) of the innovation with an interest in its rapid and complete diffusion and those where there is not such a person or entity. Related to this is a distinction between situations involving both innovation diffusion agencies and adopters and those involving only adopters. (31)

Brown distinguishes macro scale and meso scale diffusion from micro scale diffusion. Macro scale diffusion takes place within an urban system. It encompasses the processes of diffusion from the propagator of the innovation to diffusion agencies including the establishment of the agencies themselves. Meso scale diffusion studies diffusion within the hinterland of a single urban system. The meso scale primarily encompasses the processes of diffusion from the agencies to the population at large. (32, 33) Micro scale diffusion encompasses the processes of diffusion among individuals comprising that population. It takes place within a small area or single community.

Brown is interested in identifying conditions that influence spatial aspects of diffusion at the macro and meso stages and the patterns of diffusion generated by these conditions. A proposal to this effect has recently (1973) been funded by the National Science Foundation. Brown hopes that this research will provide a bridge between work in the genre of Myers and Marquis on propagator decisions to produce innovations (34) and work in the genre of Rogers on the adoption of innovation by individuals (35).

C. Economic Factors and Technological Change

Economists have specialized concerns of their own regarding diffusion. Griliches in his study of the diffusion of hybrid corn was interested in presenting a model of the process of technological change. (36) He fitted logistic growth functions to the data by state and crop reporting districts, reducing differences among areas to differences in estimates of the three parameters of the logistic: origins, slopes, and ceilings.

his implied hypothesis was that profit maximization is directly related to the regional development of hybrid corn. Griliches major empirical findings were two-fold:

> 1. Differences in the long-run equilibrium use of hybrid corn (ceilings) and in the rates of approach to that equilibrium (slopes) are explained in part, by differences in the profitability of the shift from open pollinated to hybrid varieties in different parts of the country.
> 2. The lag in the development of adaptable hybrids for particular areas and the lag in the entry of seed producers into these areas (differences in origins) are explained on the basis of varying profitability of entry, "profitability being a function of market density, and innovation and marketing cost."

Griliches' major conclusion was that the process of innovation, the process of adapting and distributing a particular invention to different markets, and the rate at which it is accepted by entrepreneurs are amenable to economic analysis. He felt that the development of hybrid corn was largely guided by expected payoff, "better" areas being entered first. (37)

This study is cited again in this paper (having been cited numerous times by other writers.) It is a significant contribution to diffusion studies because the pay-off model may be applied to the study of the diffusion of other inventions. However, "pay-off" may be experienced in terms other than economic. Griliches' profit maximization model treats an important aspect of technological change. A multivariate pay-off model including economic, social-psychological and cultural factors might explain more fully diffusion of innovations in a number of different areas.

Nelson, Peck and Kalachek are interested in the relationships of technological change and economic growth (38). (Technological and economic growth are here envisioned as a special kind of diffusion.) They examine first the relationship between technology and economic growth and secondly the adjustment of the labor market to technological growth. They then make recommendations for public policy to encourage technological growth. They find that economic growth occurs unevenly from sector to sector and that in advanced nations technological advance plays the leading role in economic growth with capital formation and education providing the necessary support. One of their major recommendations for policy consideration is the development of a National Institute of Technology and an industrial extension service to help diffuse technical information to firms who can use it quicker than they would ordinarily get it.

Schmookler is also interested in the relationship between technological change and economic growth. (39) He uses patent statistics of capital goods to show that long term economic growth is primarily the reslt of the growth of technological knowledge. However, he appears primarily concerned with invention (prototype) rather than innovation(application).

Mansfield as an economist stands out as the giant in his wide-ranging interests in the problem area of technological change and economic growth. He is concerned with the measurement of technological change, the determinants of technological change and the diffusion of innovations. (40) He can't seem to conceptualize any one of these elements apart from the others. An innovation's rate of diffusion seems to be determined in large part by four factors:

1. The extent of the economic advantage of the innovation over older methods or products.
2. The extent of the uncertainty associated with using the innovation when it first appears.
3. The extent of the commitment required to try out the innovation.
4. The rate at which the initial uncertainty regarding the innovation can be reduced.(123)

Mansfield has provided evidence that more profitable innovations spread more rapidly than less profitable ones. (42) Profitability is directly related to rate of adoption. (43) He studies 12 cost-reducing innovations in bituminous coal, iron and steel, brewing and railroads. His units of analysis are industrial firms. This is similar to Griliches' major empirical finding in the varying adoption rate of hybrid corn - profitability. (44)

The uncertainty factor associated with an innovation affects its rate of diffusion. If potential users are very uncertain of an innovation's performance, the invention tends to spread less rapidly than if they are relatively sure of its performance. New ideas that are relatively simple to understand seem to be accepted more rapidly than more complicated ones. (45) This factor is brought out even more sharply by Rogers (1971) in his generalization that the complexity of an innovation, as perceived by members of a social system, is negatively related to its rate of adoption. (46) This seemed to be equally true with farm innovations, as Mansfield found it to be with industrial innovations. Rogers cites studies by Kivlin (1960) (47), Singh (1966) (48) in Canada and Petrini (1966) in Sweden (49). Rogers states that Petrini found that complexity, along with relative advantage (which would incorporate Mansfield's and Griliches' factor of profitability) explained 71 percent of the variance in the rate of adoption of innovations among Swedish farmers (50).

Mansfield includes in the second factor of extent of uncertainty two other aspects besides complexity. Ideas which can easily be verified, which have observable results and those which are consistent with existing ideas and beliefs, seem to spread more rapidly than others. Rogers is in accord with this concept as well. He generalizes that the observability of an innovation, as perceived by members of a social system, is positively related to its rate of adoption (51). Rogers cites studies by Hruschka and Rhemwald (1965) that the more observable innovations which were demonstrated by German "pilot farmers" diffused more widely than the less visible innovations. (52) It is interesting to note that although Mansfield is primarily concerned with the diffusion of industrial innovations and Rogers with the diffusion of agricultural innovations, they appear to be in point to point correspondence with reference to the relationship of the perceived attributes of an innovation to its rate of adoption.

The third factor Mansfield deems important is the extent of commitment required to try out the innovation. Mansfield (1961) clearly demonstrated that the rate of diffusion of an industrial innovation is inversely related to the size of the investment required to use the innovation. (53) In other words an initial relatively small investment for an innovation is directly related to its rate of adoption. Rogers makes this same point for agricultural innovations in slightly different fashion. (54) He says that the trialability of an innovation as perceived by members of a social system, is positively related to its rate of adoption. By trialability, he means the degree to which an innovation may be experimented with on a limited basis. In support of this statement he cites studies by Fliegel and Kivlin (1966) (55), Singh (1966) (56), and Fliegel et al (1968) (56). Again in this respect, industrial diffusion is not different from diffusion in agriculture.

Mansfield's fourth factor related to diffusion - the rate of reduction of the initial uncertainty regarding the innovation's performance - is mostly conjecture. Difficulty in obtaining supporting evidence is due to the differential intrinsic nature of innovations. The nature of some innovations is such that information regarding their performance can be obtained quickly;

others require a long time. Also potential users differ in degree of sophistication and training.

Rogers also alludes to the rate of reduction of initial uncertainty regarding the innovation's performance when he discusses what he terms the "confirmation" function. He indicates there is some evidence that a decision to adopt or reject is not the terminal stage in the innovation-decision process (58). (See Mason (1964) (59), and Francis and Rogers (1962) (60)). At the confirmation function the individual seeks reinforcement for the innovation-decision he has made, but he may reverse his previous decision if exposed to conflicting messages about the innovation.

Using the four factors, Mansfield (1961) constructed and tested a simple model (multiple correlation) that the probability that a firm will introduce a new technique increases with the proportion of firms already using it, and the profitability of doing so, but decreases with the size of the investment required. He used secondary data. The model promises to be a useful forecasting device for interfirm diffusion of innovations.

Mansfield (1963) is also concerned with the rate of diffusion within an industry (62). He defines intrafirm rate of diffusion as the rate at which a particular firm, once it has begun to use a new technique, proceeds to substitute it for older methods. Mansfield studies the substitution of Diesel locomotives for steam. He examines the rate of diffusion in the various railroad companies. He constructs and tests an econometric model to help explain differences among railroads in the rate at which once they had begun to dieselize, they substituted diesel motive for steam. "Once they had begun to dieselize" is operationally defined as 10 per cent achievement of dieselization. Substitution is considered fairly complete when 90 per cent dieselization has taken place in a firm. Mansfield makes the following assumption: Given that one knows the per cent of the firms in the industry that have begun to use the innovation at each point in time, and the average per cent of output produced by the innovation by these firms at each point in time, one can simply multiply them to get the corresponding measure of the rate of diffusion in the industry.

Nine years were required, on the average, to increase a firm's stock of diesels from 10 to 90 per cent of the total. Since these findings pertain to only one innovation (diesels), they provide little information regarding the usefulness of a model of this sort for new techniques in general. (The rate of reduction of the initial uncertainty pertaining to an innovation's performance varies from one innovation to another.)

The model used in the study of diesel diffusion is similar in structure to the one used to represent rate of interfirm diffusion of an innovation (Mansfield 1961). This suggests a certain degree of unity and similarity between the two diffusion processes (interfirm and intrafirm). Further efforts should be made, using secondary data for other innovations to test this sort of econometric model of the rate of diffusion of an innovation. Something however will have to be done about operationalizing the rate of reduciton of the initial uncertainty.

Mansfield's most comprehensive work on the diffusion of industrial innovations is Industrial Research and Technological Innovation: An Econometric Analysis (1968) (63). Large national industries are investigated: iron and steel, petroleum, coal and railroads. Again secondary data is utilized to analyze the adoption of 150 innovations by firms in these four industries, from 1919 to 1958. A hypothesized regression model is tested for fit of actual data (64). Mansfield builds on all his previous assumptions and models (1961-68).

A number of findings emerge from Mansfield's herculean task. The length of time a firm waits before using a new technique tends to be inversely related to its size and the profitability of its investment in the innovation. (65) About 20 years or more are required for all the major firms in an industry to adopt an innovation. (66) The number of firms already adopting is positively related to the rate of adoption. This is in keeping with the S curve of growth prior to the saturation stage. Diffusion of innovations is directly related to the firm's innovativeness. However, the results of Mansfield's study revealed no close relationship between a firm's rate of growth and the rate at which it adopts an innovation. (67)

From his early studies, Mansfield expected to find that larger firms would introduce innovations more quickly than smaller firms. (Mansfield 1963) (68). Larger firms might be expected to introduce an innovation more quickly than small ones because larger firms have greater financial resources and more extensive engineering departments. They can pioneer more cheaply and with less risk. This finding was substantiated in Mansfield (1968) and Mansfield et al (1971) (69). Mansfield found no group of firms that exhibited consistent leadership with respect to the diffusion of innovations. Interestingly enough, firms with younger top management did not adopt an innovation sooner than firms with older top management. This is consistent with Rogers analysis of farm innovations (See Rogers 1971, p. 185).

It is small wonder that Mansfield (et al. 1971) simply defines diffusion as essentially a learning process where the learning takes place among a considerable number of users and producers. He has so much to say about diffusion in so many different contexts.

In one of his most recent collaborative works Mansfield et al (1971) look at intrafirm research and innovation. The purpose of this book is to enhance understanding of the relationship of technological change to economic growth. One section of this book deals with the diffusion of numerical control in the tool-and-die industry. (71) The authors investigate secondary data gleaned from the literature, government reports and Ph.D. dissertations. The findings are of interest because a number of them are other than economic and are in agreement with some of the assumptions re diffusion of anthropologists and sociologists. Some of the major findings include the following:

> 1. The diffusion process is slowed by lack of knowledge and resistance to change.
> 2. The primary reason given for non-use was that the innovation would be unprofitable.
> 3. Both the larger firms and firms with more highly educated owners tend to be early users.

Barnett, an anthropologist in a highly speculative, psychological work dealt with the problem of resistance to change (72). A new idea, he felt, must be compatible with the norms of the social system and the adopter must perceive relative personal advantage before he will adopt (73). As Rogers (1971) points out changes will occur in the meaning of an innovation and the use to which it is put even as diffusion proceeds (74). If the adopter is unable to satisfy himself on the score of compatibility and relative advantage, he will resist change. There is then always the problem of "merchandizing" the innovation in such a way that the potential adopter can maximize his future expectations.

Barnett (1953) based his psychological speculations on resistance to change, on his own experiences with a number of different ethnic and religious groups. He did not employ any quantitative analytic techniques. Mansfield et al (1971) were able to establish quantitatively that resistance to change (rejection of numerical control in the tool and die industry) within the firm (which is simply one kind of social system) did exist, and were only partially able to explain it on the basis of profit maximization. Rogers (1971) makes the generalization that the relative advantage of a new idea as perceived by members of a social system, is positively related to its rate of adoption (75) and supports it on the basis of findings from 29 different industrial, agricultural and cross-cultural studies (76). Mansfield et al (1971) claim that not enough is known about the diffusion of innovations in the industrial sector.

What is needed perhaps is further research into the nature of "relative advantage" and "resistance to change" - beyond the economic aspect. Social, cultural and psychological components need to be investigated.

Two separate studies heuristically illuminate social, cultural and perhaps psychological components of resistance to change in the diffusion of technological innovation. Spicer (1953) (77) studies fifteen cross-cultural cases of successful and unsuccessful attempts to introduce new ideas and methods in agriculture, industry and medicine and comes to the following conclusion:

The change agent, coming from another culture, must be careful to operate through existing channels of communication and cooperation and must understand the relationship of his position as an outsider to the members of a given social system. He must attempt to map the linkage of customs and to make sure that his product will be perceived as compatible. Aobve all he must avoid ethnocentrism - making judgments in terms of the standards, norms and values of his own culture.

Bright (1964) (78) in studying resistance to innovation as a problem of management comes up with a number of findings that are similar to Spicer's (1953). Resistance is associated with the degree to which institutions and individuals are threatened. Where slight changes in work behavior and habit are required, there is less resistance to change. Resistance is enhanced if the innovator is contemptuous of other members in the social system of work and existing work routines.

The manager attempting to introduce (diffuse) technological innovation is not really a member of his subordinates social organization. In this sense he is the outside change agent. In any event, the social and cultural components of resistance to change merit further systematic investigation. It is also worth looking at traits, characteristic ways of behavior, as intervening variables in the innovative process.

Rokealh (1960) developed a Dogmatism Scale which reliably establishes whether an individual has an open or closed mind when confronted by a new and unfamiliar situation. (79) This trait has a bearing on resistance to change and could be a significant intervening variable in the diffusion of innovations.

Relative advantage psychologically is a function of selective perception. It has been established by Bruner and many others that perception is a highly subjective phenomenon (80). Each individual may perceive a new idea, process or product in terms of his own past experience and technical competence, his current needs, and his future expectations. This differs from one individual to another. Both the potential propagator and potential adopter of an innovation are going to modify its purely economic and other intrinsic aspects through selective perception, thereby affecting its rate of adoption. When Mansfield et al (1971) said that the primary reason given for non-use of an innovation was that it would be unprofitable, they were saying in effect that there were other reasons (of selective relative advantage or disadvantage) left unstated (81). That relative advantage is a major reason given for adoption of innovation, but that it means different things to different groups is supported by Rogers (1971) in his analysis of the perceived attributes of 286 innovations and their rate of adoption (82).

Mansfield et al's (1971) finding that both the larger firms and firms with more highly educated owners tend to be early users is in point to point correspondence with Rogers (1971) generalizations that earlier adopters have larger sized units (farms) than later adopters and that earlier adopters have more years of education than do later adopters. The latter generalization is supported by 203 studies (83). The former generalization (about larger sized units) is supported by the findings from 152 studies (84).

Nelson and Phelps economists are interested in the relationship of "human capital" (in the form of advanced education) to technological diffusion and economic growth. They offer a tentative explanation for the generalization that earlier adopters of innovations, in both industry and agriculture, have more education. They suggest that in a technologically progressive or dynamic economy, production management is a function requiring adaptation to change and that the more educated a manager is, the quicker he will be to introduce new techniques of production (85). Educated people make good innovators, so that education speeds the process of technological diffusion. Nelson and Phelps point to the experience of United States Agriculture (86). They contend that this is so because the greater education of the more educated farmer has increased his ability to understand and evaluate the information on new products and processes disseminated by the Department of Agriculture,the farm journals, the radio, seed and equipment companies. Rogers supports the generalization that not only do early knowers of an innovation have more education, but they also have more exposure to mass media channels of communication than later knowers (87). However, Rogers points out that although 203 studies out of 275 (74 per cent) support the generalization that earlier adopters have more years of education than do later adopters, 72 studies do not support this generalization (88). It might be suggested to Nelson and Phelps that although education is an important factor as far as early adopters are concerned, it is not a necessary and sufficient condition. It has been reliably demonstrated, that dogmatism, which is a trait (a characteristic way of behavior) may predispose even well educated people to resist change, to reject any new and unfamiliar situation (89).

Nelson and Phelps view the process of education as an act of investment in people, that educated people are bearers of human capital. According to their speculative model, the rate of return to education should be greater, the more the economy is technologically progressive (90). Historians of technology like Hayami and Rutlan (1971) (91) tend to support the speculative model of Nelson and Phelps. They assume that significant growth in productivity cannot be brought about by reallocation of resources in traditional agricultural systems. Growth opportunities become available by diffusion of changes in technology. This diffusion they most recently point out (1973) takes the form of three phases of technology transfer: material transfer, design transfer and (what is especially germane here) capacity transfer (92). Investments in research and education (human capital) provide a basis for technology transfer, technical change and productivity growth in agriculture. After studying the history of agricultural innovation and macro diffusion in the United States and Japan and then examining developing countries currently experiencing enhanced agricultural production, Hayami and Rutlan reach the conclusion that one of the major factors in a country's capacity to transfer and adopt agricultural innovation from elsewhere, as well as diffuse indigenous technology, is public sector investment in education. In Japan the scarce factor in agricultural output was land. In the United States, it was labor. Mechanical technology by increasing the power available per worker facilitated increases in the land area that could be worked by a labor force of a given size. New biological and chemistry technology increased the efficiency of the process of solar energy conversion by plants and was, in effect, a substitute for addtional land. The technology that was invented and widely diffused relieved the constraint on production imposed by limited land area in Japan and that imposed by an expensive labor force in the United States.

d. Diffusion of Innovative Ideas

A number of diffusion researchers are particularly interested in the various aspects of the diffusion of innovative ideas. Crane (1972) for example is interested in the relationship between the social structure of scientists and the diffusion pattern of ideas (93). Crane concludes, after studying scientists in a small branch of mathematics and diffusion researchers in rural sociology that they comprise invisible colleges with social structure related to scientific activity. The number of scientists forming an "invisible college" simulate an S-shaped curve over time. Furthermore, the opinion leaders in a given field (determined sociometrically by Crane through a series of interviews) produce most of the published work. (94).

Sir Joseph Needham (1935) with special interests in the historical development of advances in the field of Embryology also discusses the relationship of social structure to the spread of innovative ideas (95). He points up how an individual's position in a society with a given class structure influences the development of his thought and scientific activity. Thus, the rather sharp cleavage between the philosophic biologist of the Hellenistic Age and the contemporary medical man who might often be a slave contributed to the sterility of ancient Mediterranean medicine, including obstetrics and gynecology. The Hellenistic divorce between scientific thought and empirical technique is an important case in point. Empirical technique was not thought fitting for a man of "good birth". The results of this were inevitable. Classical surgery and obstetrics benefited practically nothing from the speculations of the biologists. There was no social interaction between the two groups and no diffusion of necessary innovative ideas and techniques. Surgeons and midwives remained members of the painter-cobbler-builder group, the group of "base-born mechanics", entirely distinct from the astronomer-metaphysician-biologist group, the group familiar with courts.

Needham also points out how the absence of mental technique served as a limiting factor in the diffusion (among biologists) of innovative ideas in embryology. On several occasions research came to a standstill on account of the lack of a satisfactory terminology. (This factor certainly merits further investigation in the study of the innovative process). For example, in the 13th century Albertus of Cologne had arrived at a point beyond which progress was impossible in the absence of new words. When there was no other means of describing the sero-amniotic junction in the hen's egg than by speaking of the hole on the left side of the vessel which runs above the membrane on the right hand of something else accuracy was difficult and speed impossible.

Not only must the right concepts and words be chosen, but the wrong ones must be abandoned to allow for progress and the diffusion of innovative ideas. (This certainly merits further investigation). The expulsion of "ethical" terminology from biology and embryology is an excellent example. Good and bad, noble and ignoble, beautiful and ugly, honorable and dishonorable, are not terms with a biological meaning. This is a proposition which took many centuries for biologists to realize.

Ideas of good and bad entered biology partly under the concept of "perfection". In 1260 Albertus was maintaining that male chicks always hatched from the more spherical eggs and female chicks from the more oval eggs, because the sphere is the most perfect of all figures in solid geometry and the male the more perfect of the two sexes.

Ben-David (1971) is interested in the diffusion of scientific ideas in terms of the scientist's role in society (96). He treats the subject from a historical and comparative perspective. The main sociological concept is that of the "role". The scientific role, for example, is the pattern of behavior sentiments, and motives conceived by people as a unit of social interaction with a distinct function of its own and considered as appropriate in given situations. This concept implies that people understand the purpose of an actor in a role and are capable of responding to it and evaluating it. Positive evaluation implies legitimation. In the absence of such a publicly recognized role, there is little chance for the transmission and diffusion of the knowledge and skills pertaining to a particular scientific activity. The emergence of a new social role takes place, according to Ben-David, within a more comprehensive social setting. Its emergence implies a change of social values. In the case of the scientific role that change in values meant the acceptance of the search for truth through logic and experiment as a worthwhile intellectual pursuit. This modified philosophical and religious authority, and raised the dignity of technological knowledge. The emergence of the scientific role necessary to the diffusion of scientific ideas was connected to changes in the institutions regulating cultural activity (97). This first occured in England and later in America. A closer examination of Ben-David's conceptual perspective(conditions necessary for the emergence of the scientific role) may have relevance for administrative decision-making in the cross-cultural diffusion of scientific ideas.

Kranzberg (The Ecology of Innovation) is concerned among other things with institutionalized mechanisms for the dissemination and diffusion of innovative ideas.

Among these he mentions, exhibits, world fairs, trade shows, trade publications, annual meetings of professional societies with published proceedings, sales engineers and salesmen and medical detail men with demonstrator models. He suggests that these are all rich sources of primary and secondary data. As a historian of technology he concerns himself with the possible output of these social mechanisms rather than with the dynamics of the process of information flow involved in each of these mechanisms.

An unusually successful institutionalized social mechanism for the diffusion of information about technological innovation has been the agricultural extension service. Kranzberg, departing from practice, goes into the dynamics of this particular institutionalized diffusion mechanism and makes the following salient points (which may have relevance for the successful diffusion of industrial innovation.)

1. The agricultural program was deliberately fostered and supported by the Federal government.
2. The Morril Land Grant Act, forming the basis for the land grant colleges combined all elements of the innovative process (not only the diffusion phase.)
   a. Agricultural research stations adjacent to land grant colleges generate discoveries and inventions.
   b. The extension service through county agricultural agents implements the application and diffusion of the discoveries and inventions.
   c. The county agricultural agent not only transmits scientific, technical and economic information, but he also serves as a means for feedback from the farmer-user to the innovative and information generating sources.

We see here built in self-correcting mechanisms affecting all the linkages in the innovative process.

problem definition - invention
problem definition - research and development
problem definition - application
problem definition - diffusion
invention - research and development
invention - application
invention - diffusion
research and development - application
research and development - diffusion
application - diffusion

The agricultural program fostered by the federal government did not attempt to restrict activity to any particular phase of agricultural innovation. Perhaps what is now needed is a similar federally sponsored, non-restrictive program for general technological innovation and diffusion with adequate feedback mechanisms between innovator and ultimate user for the solution of social and economic ills unique to urban life.

Before passing from the problem area of the different mechanisms of diffusion of ideas, one ought to mention the concerns of some diffusion researchers with the people-to-people transfer of information (Thomas Allen) (98) and the diffusion of ideas through literature (Derek Price, Garfield) (99). In the first instance, engineers and workers are on the move in a highly mobile society. In addition, Allen's "technological gatekeeper" and informational entrepreneur and Roger's more general "change agent" exercise selective perception and to that extent control information flow. In the second instance academics, professionals and administrators more or less read what others in their respective fields have to say about new developments and again through a process of selective perception, cite them to support their own contentions and programs.

Institutionalized mechanisms for the diffusion of innovative ideas (conferences and journals) and the role of the technological change agent and informational entrepreneur are not really mutually exclusive. A recent example may be cited. Reference is here made to the annual conference of the Mechanical Engineers in Israel in 1972 and to the Journal of the Association of Engineers and Architects, March 1973 (100). Most of the articles in this issue contain part of the lectures given in the 1972 conference. The lead editorial reflects the role of informational entrepreneur and change agent. The following points are made:

1. When the technical possibilities of a developing country like Israel are compared with the state of art in industrially developed countries a gap is found.
2. It is no longer possible for most of the mechanical engineers to occupy themselves with pure engineering problems.
3. The general tendency is that the engineer holds more and more positions in industrial engineering, such as production planning, and quality-control.
4. An engineer working in modern industry has to know today how goods are produced by machines regulated with numerical control.
5. Many measurements, recordings, analyses and other time-consuming calculations are no longer feasible without the help of a computer.
6. Therefore, an engineer in a leading position in industry should be able to direct his colleagues at work in those special branches of engineering requiring special knowledge.

7. To close the gap between industry in Israel and industry in the developed countries engineering education must be revised and upgraded. (101)

The articles in the issue then proceed to inform and persuade members with the data of past experience, how to, and decision making in the use of machines regulated with numerical control. For example Rosenstook, Production Manager - Aero Equipment LTD in discussing the economical aspects in the operation of numerically controlled machines offers the following pieces of "persuasive" information.

1. Evolution of N/C from the first experimental machine in 1954 to nearly 40,000 in current use. (There are already plenty of satisfied adopters.)
2. The range of application of N/C machines in terms of batch quantity and part complexity (compatibility.)
3. Criteria for determining the economy of producing a part on a N/C machine (decision-making and reduction of uncertainty.)
4. A comparison of the production costs between the conventional and the N/C method, for two typical parts, and the justification of the investment in N/C machines (Relative advantage).
5. Additional advantages of the N/C system, like high accuracy and reduction in inspection costs, lead time and technical manpower (Relative advantage). (102)

e. Organizational Structure, Social Psychological Dynamics, and Technological Innovation

A number of thinkers and researchers concern themselves with the relationship of organizational structure and the introduction and diffusion of technological innovation. Schon (1967) develops a speculative approach to the processes of invention and diffusion of innovations within the context of organizations. (103) It is based upon his extensive personal experiences with Arthur D. Little and a number of governmental agencies. Schon tries to encapsulate the nature of diffusion of technological innovation in U.S. industry and offers examples from the textile, machine tool and construction industries.

Schon explores the nature of invention and diffusion of innovation and their relation to risk, uncertainty and corporate decision-making in today's large corporations, whole industries and even governmental agencies. He comes to the conclusion that these entities are confronted with a dilemma. They cannot live without a continuing stream of inventions and innovations and remain competitive and creative. Yet they cannot well live with them or readily respond to them. Innovation threatens the chain of command, the division of labor, and the system of rules. Because innovative practices and techniques tend to make old skills obsolete, there is also the psychological threat of loss of identity.

Firms and industries are self-maintaining and self-reinforcing systems of communications and control. They have a tendency to be closed systems, so that they go on being just what they are and doing very much what they have been doing. They maintain the myth that invention is a rational process, yet it is essentially a non-rational process. The adoption of innovation occurs in the face of uncertainty and risk (104). The non-rational thesis is supported by two additional facts:

1. Innovation by invasion is a major source of industrial innovation (105).
2. The rate of industrial innovation is affected by government policy (106).

Organizations for the most part are designed to deal with the known. The unknown injected by innovation introduces the correlates of uncertainty-anxiety and fear. These in turn create the climate for resistance to change and thereby impede the diffusion process.

The interpersonal dynamics of organizational resistance to change are perhaps tangentially illuminated by Dubin (1958) (107). To the degree that individuals in a formal organization identify with the ongoing _means_ and _specific goals_ of the organization, and to the extent that superiors perceive this and back up their approvals and disapprovals with rewards and punishments, this will function to motivate these individuals to conform to and _internalize_ group shared expectations - the system of roles and rules pertaining to the various positions in the organization. By the same token, once the internalization process is complete and the individuals are safely and satisfactorily esconsed in the system, they will tend to resist change.

Vroom (1964) succinctly states that the ability to control the behavior of its members is a prerequisite of a viable organization. To attain its objectives, each organization must determine the functions that have to be performed allocate these functions to organization members and establish behavior patterns on the part of its members which lead to the predictable performance of these functions (108). This implies an organization motivation system dealing with the known.

The motivation system in formal organization may be seen as a complex of forces initiating and maintaining human activity along guide lines deemed necessary to the achievement of the organizational purpose or objective. Dubin makes the point that motivation in formal organization is continuous. When a person is rewarded for displaying a behavior pattern appropriate to his role, it is internalized and becomes a guide for future activity (109). Innovation often demands the unlearning of patterns of behavior that have been rewarding in the past. This creates situations of stress and uncertainty and an initial disinclination to behave in new and unfamiliar ways. This resistance to change is experienced up and down the chain of command. Mitzner (1968) has demonstrated over 66 different organizations that there is no significant difference in levels of dogmatism, capacity of logical response to new and unfamiliar situations, between the upper and lower ranks in the chain of command of formal organization (110). Differences in the innovativeness of industrial organizations are to be sought in the nature of their structures rather than the personality styles of their participants. Personality styles act as modifiers and intervening variables rather than as determinants of diffusion of technological innovation. (Only as modifiers do they merit further investigation.)

Burns and Stalker (1961) do an in-depth study of the organization motivation system in their study of 19 rayon and electronics firms in England and Scotland. They are primarily interested in the relationship between type of management practice, organizational behavior and diffusion of new ideas.

Burns and Stalker found that the adaptation of relationships between individuals rather than of individuals themselves toward the requirements of the technical tasks of the organization, determines organizational innovativeness. This is largely brought about by management practice (the organization motivation model). The "mechanistic" system of management practice is appropriate to stable situations but inappropriate to the innovative process. The "organic" system of management practice is appropriate to conditions of change.

MECHANISTIC SYSTEM OF ORGANIZATION PRACTICE
Characteristics

1. Problems and tasks facing organization are broken down into specialties.
2. Each individual pursues his task as something distinct from the real tasks of the organization as a whole. 'Somebody at the top' is responsible for seeing to its relevance.
3. Technical methods and duties and powers attached to each functional role are precisely defined.
4. Interaction within management tends to be vertical -- between superior and subordinate.
5. Operations and behavior (working) are governed by instructions and decisions issued by superiors.

6. The command hierarchy is maintained by the implicit assumption that all knowledge about the situation of the firm and its tasks is, or should be, available only to the head of the firm.
7. Management (often visualized as the complex hierarchy familiar in organization charts) operates a simple control system with:
(a) information flowing up through a succession of filters
(b) decisions & instructions flowing downwards through a succession of amplifiers.

ORGANIC SYSTEM OF ORGANIZATION PRACTICE
Characteristics

The organic system is adapted to unstable conditions when problems and requirements for action arise which cannot be broken down and distributed among specialist roles within a clearly defined hierarchy so that:

A. Individuals have to perform their special tasks in the light of their knowledge of the tasks of the organization as a whole.
B. Jobs lose much of their formal definition in terms of methods, duties and powers which have to be redefined continually by interaction with others participating in a task. (This may induce some anxiety but is not intended to foster insecurity.)
C. Interaction runs laterally as well as vertically.
D. Communication between people of different ranks tends to resemble lateral consultation rather than vertical command.
E. Omniscience can no longer be imputed to the head of the firm or organization.

Organizational impediments to innovative practice occur when there is a disregard of behavioral aspects. Why doesn't a working organization of an agency change its system from mechanistic to organic as its circumstances change with entry into new commercial and/or technical fields? What behavioral aspects need to be dealt with? Burns and Stalker claim that every single person in a firm not only is (a) a member of a working organization but also (b) a member of a group with sectional interests in conflict with those of other groups and (c) one individual among many to whom the rank they occupy and the prestige attaching to them are matters of deep concern.

Any firm contains not only a working organization but a political structure (power) and a status system. The following then is operative.

> The existing political system and status structure may be threatened by the advent of a new group.
>
> Technical information available to newcomers may be used as an instrument for political control (technical information is a scarce and valued resource).

The individual manager becomes absorbed in conflicts over power and status because they present him with interests and problems more immediately important to him and more easily comprehended than those raised by the organizational milieu. Increases in the <u>rate</u> of <u>technical</u> and <u>commercial</u> <u>change</u> may mean more problems, more unfamiliar information, a wider range of work relationships, and heavier mental and emotional commitments. (It is at this point that different levels of dogmatism may modify orgnaizational outcomes. This merits further investigation.) Many individual managers find it impossible to accept such conditions for their occupational lives. To keep their commitments limited means: gaining more control over their personal situation or claiming exemption because of status.

These purposes involve manoeuvres which persistently <u>run</u> <u>counter</u> to the development of an organic system and which can only be resolved by reversion to the mechanistic (non-innovative) system.

The process, just outlined, needs to be understood more fully. It impedes the development of the "organic" system of management practice which is appropriate to innovative practice and the diffusion of new ideas. The dynamics of social-psychological resistance to change need to be understood conceptually in order to remove impediments to the innovative process.

Etzioni (1961) deals with management practice conceptually (112). His findings appear to be relevant to organizational impediments to innovative practice. Although formal organizations have similar structural characteristics - a chain of command, a division of labor, a system of rules and limited objectives, -- institutional differences do exist. Etzioni suggests that these differences are manifest in the compliance patterns of formal organizations. He has chosen the nature of compliance in the organization as the basis for classification. Compliance is a relationship consisting of the power employed by superiors to control subordinates and the orientation of the subordinate to this power (113). The compliance pattern is a variable that combines structural and motivational aspects: Structural, since there is concern with the kinds and distribution of power in formal organizations; motivational, since there is concern with differential commitments of actors to organizations (as units which exercise power over them). This variable should be pertinent to the investigation of the problem at hand since it reflects the articulation of the social system (the chain of command) and the personality system. It takes into account that units of interaction exist and assumes that organizational behavior can most adequately be thought of as occurring in a system of interdependent forces. In sum, there are two parties to a compliance relationship: an actor who exercises power, and an actor subject to this power who responds with either more or less alienation or more or less commitment (114). On the basis of a priori reasoning, the implementation of innovative practice is facilitated with commitment.

In the classification of compliance, power differs according to the legitimate means employed to make subjects comply. These means may be physical, material, or symbolic. Coercive power rests on the application, or the threat of application of physical sanctions. Renumerative power is based on control over material resources and rewards through the allocation of salaries and wages, fringe benefits, services and commodities. Normative power rests on the allocation and manipulation of symbolic rewards and deprivations through the employment of leaders, the distribution of "acceptance and response" and an ongoing system of symmetrical (two-way) communication.

The other aspect of the compliance pattern is the evaluative orientation of a subordinate to the particular type of power exercised over him. It determines the nature of his involvement in the organization. The intensity of involvement ranges from high to low and the direction is either positive or negative. Etzioni lists three kinds of involvement on the involvemental continuum:

> Alienative - intense negative orientation
> Moral - positive orientation of high intensity
> Calculative - designating either a negative or
> positive orientation of low intensity (115).

When the kind of involvement that subordinates have and the kind of involvement that tends to be generated by the predominant form of organizational power are the same the relationship is congruent and there should be no social-psychological impediments to the implementation of innovative practices. In this instance the presupposition is a union of normative power with high commitment.

George A. Miller (1967) in a study of the relationship between alienation from work (type of involvement) and two dimensions of organization structure--degree of organizational control (kind of power exercised) and number of professional incentives--in a major American aerospace company, finds that work alienation is associated with type of organizational structure, but that scientists and engineers may differ less in their experiencing of alienation (116) than previous research would indicate (117). However, there may be as much structural variation within a particular organization as there is between different types of organization - and this may be a source of difference.

There are two distinct organizational units in the aerospace company, the laboratory and the aero-space group. The goals of the laboratory are more concerned with pure or basic scientific research, whereas the goals of the aero-space group are more concerned with traditional research and development. Therefore, the laboratory should be characterized by less organizational control and more professional incentives (moral involvement) than the aero-space group. In addition, since alienation from work is related to differences in organizational structure (degree of organizational control--kind of power exercised) professionals working in the laboratory should experience less alienation than those working in the aero-space group.

Miller shows striking differences in the control and incentive structures of the two organizational units. In each of four comparisons, there is less control and more incentives in the laboratory than in the aero-space group (118).

These differences in organizational structure are reflected in differences in the degree of work alienation. In the laboratory only 4 per cent experienced high work alienation as compared with 34 per cent of those persons in the aero-space group. Moreover, 63 per cent of those in the laboratory experienced low alienation as compared to only 25 per cent of those in the aero-space group. Thus, type of organizational unit is clearly related to degree of work alienation in the expected direction (119).

The restriction of information and communication apparently is related to the effectiveness of organizations that utilize coercive sanctions and generally exercise relatively high degrees of control (mechanistic management practice). Conversely, the free flow of communication (symmetrical) is related to the effectiveness of organizations that utilize normative sanctions and exercise relatively little control (organic management practice).

The diffusion of innovative practice within organizations apart from technological, gatekeeper and economic considerations is largely dependent upon the appropriate compliance pattern and the resolution of individual managerial conflict over power and status. (Baker discusses this in some detail in his section on Research and Development).

The nature of the interaction of the organization motivation model (as expressed in Burns and Stalker management of innovation and Etzioni's compliance pattern) and the individual's personality traits (as partially expressed in Rokeach's Dogmatism Scale) still remains problematic as far as diffusion of innovation is concerned.

Doby (1966), in his chapter of "Perceiving and the Social World," Argues that percepts are the end-products of a slow process of neurological and cognitive construction through manipulation of and experimentation with the external world. Percepts change as the effects of experience change. Therefore, learning plays an important role in the development of perceptual products. This is true of innovative practice. Perceptual materials become organized and form the basis for learning sets which, in turn, affect subsequent learning. Accordingly, from the point of view of social psychology, we may study the effects of learning on perception or the effects of perception on learning (121). A basic problem in social psychology, and one germane to this study, is the question: "How are various perceptions learned?" (It is worth recalling that Mansfield et al (1971) define diffusion as essentially a learning process.)

Do the needs, values, goals, and fears of the individuals affect them? Hilgard notes that in the search for environmental stability, the whole person achieves his perceptions; that is, he regulates them in the service of need satisfaction (122). It goes without saying, at this stage of research, that the internal structure alone, as measured by personality traits, does not account for perceptual behavior. Allport implies that one can often afford to disregard differences among situations, because a "trait" will make them functional equivalents (123). But if we add that one can sometimes afford to disregard traits because the situation may make them functional equivalents, the explanation becomes problematic again. The organization motivation system, for example, may reduce a wide range of individual tendencies to functional equivalents so far as the social process is concerned. The motives of those who participate in formal organization may vary widely yet contribute to a common social process and specific goal. The participants will usually try to conform to group shared expectations as they perceive them. Society and its formal organizations tend to set aside those individuals who are so dissociated from reality that their activity bears no relationship to their immediate environment (124).

Once a person becomes acclimated to his work organization, he acknowledges, in a sense, his particular internalization of the motivational patterns established by that organization. The formal organization sustains, in this way, systems of motivation. Each member of the work organization, for example, discovers that some rewards of his work come in the form of rights and privileges. These rewards, in turn, require that he carry out the tasks and duties of his work with at least minimum responsibility and obligation. In the very components defining work assignments (organizationally given role demands) are to be found the channels of behavior appropriate to work, and the rewards that reinforce compliance with these selected channels of behavior. The molding character of the work environment results in built-in, continuous, systematic motivation (125).

The statement of one of the above points of view (Allport's emphasis on the importance of "trait") without the other (Dubin's explication of the "motivation of organization activities") may be sterile in any attempt to develop a theory of behavior relative to diffusion of innovation in formal organization.

Moss (1961), in turn, has shown that the generality of cautious behavior as measured by personality tests is highly dependent upon the psychological situation, and that the prediction of behavior, therefore, requires an analysis of the interaction of individual and situational variables (126).

Social-psychological study of group influences on perception strengthen the claim that research on human behavior must explore the systematic influences of interaction. The classic studies by Asch, Back, Lewin, Bennet, Sherif, and others established that the group setting within which perception takes place affects that perception (127). Such variables as degree of unanimity and expressed norms of the group, interacting with personality tendencies (self-confidence, dogmatism, and authoritarianism, for example) clearly influence the process of perceiving and learning. Since the aforementioned studies suggest that the way in which a person is predisposed to handle his needs may mediate the manner in which evaluation of degree of adherence to the organization motivation system and cognitive selectivity interact, measures of personality traits should be included in an investigation of the social psychological conditions related to the diffusion of innovation. There can be no probability statement of perception and diffusion of innovative practice independent of the human observer (in the organization) who formulates it, nor can there be a statement of probability wholly free of assumptions regarding the conditions under which the event occurs. To round out the picture, one must include and assess the uniqueness of the individual member in the work organization. The prevailing use of secondary data in diffusion studies though useful in developing econometric models restricts the researchers (Mansfield, Griliches) to economic and demographic (age, sex, education) data. It does not take into account social psychological phenomena.

A discussion of the relationship of organizational structure to diffusion of innovation would be incomplete without mention of the informal organization. Farris (1972) examines the effect of individual roles on performance in innovative groups (128). The conclusions reached are based on data from a survey in a NASA research center. Problem-solving of scientific groups is facilitated by supervisors, fellow group members and scientists from outside the group. High innovative groups tend to work more as technical teams, and members name each other more often as helpful in the performance of technical roles. Colleagues seek each other out frequently during the proposal development stage, and don't hesitate to subject their ideas to critical evaluation by their supervisors. Farris suggests that the supervisor is the key man; that he should be the "gatekeeper"; that he should encourage his subordinates to exchange technical information and help each other think through their technical problems. All this lends further support to Burns and Stalker's organic system of management practice and Etzioni's normative compliance pattern as being most appropriate to the diffusion of innovation.

Rogers (1973) has performed a singular service for academic and technological practitioners interested in the relationship of organizational structure to the diffusion of innovation. He has identified 373 publications in the Diffusion Document Center dealing with the diffusion of innovations within, or to, organizations (129). The publications deal with organizations as the unit of adoption for an innovation. Rogers cautions users that the publications do not as yet focus on the social psychological process by which an innovation is adopted within an organizational structure.

f. Social Networks

By and large most diffusion researchers (Rogers, Hagerstrand, Crane, etc.) regardless of their specialized concerns agree on the importance of social networks in the diffusion of innovation. An early study by Coleman, Katz and Menzel (1957) points up the key rolé of social networks in the diffusion of an innovation among physicians (130). The investigators focus on the ongoing social processes which finally led to the widespread adoption of a new drug (euphemistically called "gammamyn") by physicians in four cities. They are concerned with the effectiveness of networks of interpersonal relations at each stage of the diffusion process.

Structured interviews were conducted with the physicians in the four cities to determine individual attributes as to whether they were profession-oriented or patient-oriented. It was possible through this procedure to ascertain the sociometric choice of colleagues and the patterned network of interpersonal relations. They then ensued a systematic search of the prescription records of local pharmacies to determine the month in which each physician first used the drug in the 15 month period following its release.

Networks of social relations in this study are mapped prior to the introduction of the new drug being studied, in the sense that friendship and consultation are recorded independently of any decision the doctor has made. This procedure is to examine the potential relevance of the various sociometric patterns of relationship to the transmission of influence.

Doctors who were profession-oriented (associated with doctors) adopted the drug sooner than those who were patient-oriented. The degree of a physician's integration among his local colleagues was strongly and positively related to the date of his first use of the drug. The "integrated" doctors - tnose named as "friends" by three or more of their colleagues - were much faster to introduce gammamyn.

This study emphasizes the importance of social contacts among doctors as a crucial determinant of their early use of a new drug. In situations which are objectively unclear (uncertainty surrounding the use of a new drug) the social validation of judgments becomes most important. This finding is in keeping with Mansfield (1961, 1968) and Rogers (1962, 1972) generalization that reduction of initial uncertainty pertaining to an innovation's performance is directly related to its rate of diffusion. This finding is also in keeping with Farris' (1972) conclusion that problem solving of scientific groups hampered by initial uncertainty is facilitated by the social interaction of colleagues and their mutually supportive relationships. What needs to be known is not that social networks help reduce initial uncertainty pertaining to an innovation's performance and thereby enhance its rate of diffusion, but what contributes to mutually supportive relationships in scientific and generally innovative groups.

The Coleman, Katz and Menzel (1957) article is considered one of the classics in the diffusion studies, and is therefore treated at some length in the Tech Project diffusion assessment phase. It is particularly relevant to an understanding of the diffusion phase of the innovative process because the decision maker (adopter) is not the only source of information concerning his decision. Objective data from prescription records are used as well. The role of different influences is assessed not only on the basis of the decision-maker's own reconstruction of the event, but also on the basis of objective correlations from which inferences concerning the flow of influence can be drawn. For example, early adopters were more likely to participate in out of town medical specialty meetings. This is in keeping with empirical findings that doctors who were profession-oriented adopted the drug sooner than those who were patient-oriented. (The profession-oriented doctor by definition, was more concerned with the social approval of his colleagues. The patient-oriented doctor was more concerned with the social approval of his patients.)

The findings of the "gammamyn" study point up the necessity of treating informal social relationships as units of statistical analysis relevant to the dependent variable diffusion. It is therefore necessary to develop scales containing exhaustive, but mutually exclusive categories of informal social relationships. Bringing to light the web of potentially relevant relationships in which the "adopter" is embedded, would be useful in predicting decision making behavior.

Katz (1957) examines social networks in terms of the two-step flow of communication. The major hypothesis advanced is that ideas often flow from radio and point to opinion leaders and from these to the less active sections of the population (131). He does an ex post facto examination of a number of studies of diffusion in order to examine the extent to which the two-step flow hypothesis of communication finds confirmation.

The hypothesis has three distinct components:

(1) the impact of personal influence
(2) the flow of personal influence
(3) the relationship of opinion leaders to the mass media.

Katz finds the following:

1. Personal contacts appear to have been both more frequent and more effective than the mass media in influencing decisions.
2. Opinion leaders are to be found on every level of society and are very much like the people whom they influence.
3. Opinion leaders are considerably more exposed to the formal media of communication. However, most opinion leaders are primarily affected not by the communication media, but by still other people.

Therefore, the two-step flow of communication may be an oversimplification of the process. In general, the audience is not a mass of disconnected individuals hooked up to the media, but instead project the image of networks of inter-connected individuals through which mass communications are channeled. Mass media play an informing role while professional media and interpersonal relations play a legitimating role. However, influentials and influences may change roles in different spheres of influence. Hence, both subject matter and competence are important factors in the study of diffusion of innovation. This item is of "above average importance" in the diffusion literature, because it points up the possibility that the flow of communication of innovative ideas is actually a subset of the flow of communication generally. Katz and Levin (1959) (132) and Katz (1961) (133) consider diffusion studies as those tracing the movement of a given new idea, over time, through specific channels, within a social structure. They are therefore concerned with communication through social networks.

Rogers (1962) points to the lack of communication (among diffusion researchers) through social networks as the reason for inadequate diffusion of diffusion research findings and the subsequent absence of a research tradition (134). For this reason successive studies are rarely influenced by preceding investigations. For example the Coleman, Katz, and Menzel (1957) "gammamyn" study did not benefit initially from the major findings of the Ryan and Gross "Hybrid Seed Corn" study (1943) (135). Yet in both instances first use followed bell-shaped distribution when plotted over time. In both instances either neighbors or close colleagues were the most influential source in leading to later adoption. In both instances early adopters were more cosmopolite and had higher social status. Both studies demonstrated that society is not a mass of disconnected individuals; that the diffusion of innovation takes place through social networks consisting of opinion leaders and their followers. The opinion leaders have greater access to mass media of communication and professional and technical journals. Allen (1970) in discussing roles in technical communication networks comes up with structural similarities in R and D laboratory social networks. There are a small number of people, called "gatekeepers" who serve as opinion leaders. They have better access to outside sources of information. Quite frequently they serve as first line supervisors and there is a great deal of social interaction between them and their colleagues. They (gatekeepers) are also more cosmopolite and are therefore in a position to mediate between their organizational coworkers and the outside world (136). The gatekeepers are better acquainted than others with members of the scientific and technological community and with the extant literature. It is therefore suggested that management learn to make effective use of "gatekeepers" for the diffusion of innovative ideas.

Rogers with Shoemaker (1971) are very much concerned with social networks in the diffusion of innovation. However, their major concern is the development of a middle range theory of diffusion in which they systematically treat social networks (channels) as one of a set of interdependent parts (137). It will be more useful to deal with their contribution in the section on systematic diffusion research.

g. Miscellaneous Diffusion Concerns Meriting Systematic Research

There are a number of current diffusion concerns meriting further systematic research. The role of the multinational firm is one. Recently executives of five American multinational corporations depicted themselves before a U.N. investigating panel as well-mannered guests of the foreign country in which they operate. Top officers of General Motors, IBM, Exxon, DuPont and Pfizer Inc., said their companies stayed out of politics abroad, hired and promoted local citizens, transferred technology to poor countries, donated money to education, and cooperated in social welfare programs (138). Secretary-General Kurt Waldheim was investigating the impact of the multinational company on foreign governments. It would be useful to determine empirically under what conditions the multinational firm contributes to diffusion of innovation and under what conditions it contributes to sociocultural resistance to change. Most of the executives (of the five American multinational corporations) took issue with statements in a working paper prepared for the panel by the U.N. staff to the effect that multinational companies challenge the national sovereignty of small, weak governments. So far there has been a great deal of rhetoric and very little research.

The increasing role of organized labor in the introduction and management of technological innovatión (as more and more skills become obsolete) may be producing an impact in the rate of diffusion. This merits systematic investigation. Mansfield (1971) points out that union policies toward management's adoption of new methods and equipment can be classified into five types:

1. Willing acceptance.
2. Outright opposition.
3. Competition with a new technique.
4. Encouragement of technological change and the adoption of new techniques. (In this case the union plays an active role in promoting change.)
5. Adjustment to change (In this case there is an effort by the union to control the use of the new technique and to deal with the opportunities and problems it presents.)

At this stage, there are heuristic implications with one or two examples in Mansfield's classification of union policy with reference to innovation (139). What is needed, is a research design allowing for systematic investigation of the impact of union policy on diffusion of innovation. In general, management-labor relations has an impact on the relative advantage of technological innovation and therefore affects its rate of diffusion. This needs to be studied. An account by Dubin (Human Relations in Administration, 1961, pp. 563-565) with reference to Hybrid Seed Corn focuses attention upon this phenomenon. A field of corn may come into tassel within a few hours. If these tassels are not removed within the next few hours, the pollen can spread into neighboring fields and ruin what is considered priceless quantities of seed. In one instance supervisors and workers had a falling out. The workers refused to work. It rained and tassels appeared. Practically all the fields in that locality, amounting to about 600 acres had to be condemned because cross-pollination had taken place. The corn could not be used for seed purposes. This naturally didn't set very well with the farmers who owned the land and who had expended all the effort necessary for the premium price they would have obtained for their seed corn if it had been satisfactory. The story no doubt spread and must have had some impact on the rate of diffusion of hybrid seed corn.

Industrial technology transfer is an important aspect of the diffusion of innovation but has been treated by Kranzberg and Baker, members of the Tech Project team. One aspect of technology transfer, the transfer of military technology to civilian use, requires periodic updating to assess spinoff. By technology transfer is meant the acquisition, development, and utilization of technology in a context different from that in which it originated (140). Just as there may be a long time lag between the appearance of an idea or invention and its use in an innovation, a substantial time period usually separates the first adoption of an innovation and its spread. Rosenberg (1967) points out that studies of the diffusion of important 20th century industrial innovations show that ten to twenty years commonly elapse from the date of first use to the time an innovation is employed by most of its potential users (141). This is somewhat complicated in military technology transfer by two factors:

1. The original innovation and its diffusion is usually developed for a highly limited market of "users" under "pressure-cooker" conditions. It would be useful to learn how this affects the interval between first use and full use (10-20 years).
2. The transfer of military technology is often directly applicable to civilian use in its original form as in the case of preservation of food by canning, the jet aircraft engine and the electronic computer. On the other hand, Rosenberg points out that while devices for aiming artillery have few peacetime applications, the concept of the servomechanism and the advances it stimulated in the underlying sciences and technological arts have found important applications. Another example is the current use of game thoery in management (142). The last two instances required adaptation before adoption. It is important to ascertain <u>empirically</u> the possibility set of military technology transfer and the conditions under which there is greater and faster spinoff.

There is speculation for example that the overconcentration of research funds (federal and private) in aircraft, missiles, electrical equipment and communications will limit civilian spinoff. This needs to be ascertained systematically. Rosenblooom suggests that what is relative advantage in military technology may not be relative advantage in civilian use. The civilian application of a military invention must meet economic criteria which can only have been of secondary importance in the shaping of the new technology for its original military use. Furthermore what organizational and managerial techniques are called for in new institutional structures such as the Atomic Energy Commission and the National Aeronautics and Space Administration to facilitate military technology transfer?

4. Possibilities for Synthesis in the Work of Systematic Diffusion Researchers

It is now time to look at the work of two systematic diffusion researchers concerned with developing overall models of diffusion, Everett M. Rogers, a sociologist, and Lawrence A. Brown, an economist and urban geographer. Mansfield and Griliches developed highly sophisticated and functional, but limited ad hoc,econometric models predicated upon the use of economic and demographic, but non-behavioral, secondary data. Rogers and Brown are attempting to develop models of the diffusion process in all contexts that will be both multivariate and multifaceted and that will explain and predict as well as describe.

a. Everett M. Rogers

Rogers has made a very substantial contribution to diffusion research over the past 20 years in aggregating some 2400 publications in the Diffusion Document Center, subjecting them to content analysis, developing generalizations and then synthesizing a series of generalizations. The Diffusion Document Center only includes publications which deal with an innovation which is communicated through certain channels over time among the members of a social system. Rogers model is dealt with in great detail in Rogers with Shoemaker (1971).

Time is an important dimension in Roger's model of the process of diffusion. It is involved in the innovation-decision process by which an individual passes from first knowledge of the innovation through its adoption or rejection. It is involved in the innovativeness of the individual, the relative earliness or lateness with which an individual adopts an innovation when compared with other members of his social system. It is also involved in the innovation's rate of adoption in a social system, measured as the number of members of the system that adopt the innovation in a given time period.

The use of time in this manner allows Rogers to develop four main steps in the innovation-decision process; (1) first knowledge, (2) persuasion, (3) decision and (4) confirmation. It also allows him to develop adopter categories in terms of the innovativeness of the individual (1) innovators (the first 2.5 per cent to adopt a new process), (2) early adopters (next 13.5 per cent), (3) early majority (next 34 per cent), (4) late majority (34 percent), and (5) laggards (16 per cent). Finally it allows him to conceptualize in terms of the relative speed with which a particular innovation takes hold. Time thus becomes three dimensional--psychological, sociological and ecological (dimensions usually treated separately in the diffusion literature) - and the innovation-decision process, the innovativeness of the individual and the rate of adoption can be related to each other.

A social system to Rogers is a collectivity of units which are functionally differentiated and engaged in joint problem-solving with respect to a common goal. The units of the social system may be individuals, informal groups, complex organizations and subsystems. The social system constitutes a set of boundaries within which innovations diffuse. It thus constitutes social space and, in a sense delimits the physical space within which an innovation might spread. This concept of social system is useful in explaining microscale diffusion, or adopter diffusion within a given context. It creates some difficulty, however, in explaining diffusion from one context to another. Furthermore, it ignores the role of a deliberate propogator of innovation (market factors decision-making, for example, individual or corporate) in delimiting the location and rate of diffusion (144). In addition, the propogator may or may not be a unit of the social system in which the diffusion of the innovation is intended. Rogers tacitly recognizes and tries to overcome this difficulty in his conceptualization of the change agent as usually being different in background, norms, status and beliefs from those he is trying to influence.

Although Rogers hasn't reconciled the difficulty of relegating the entire diffusion process to the members of a given social system (microscale diffusion), he has eased the situation somewhat by subsuming the diffusion of innovation as a subset of communication theory and social change. Diffusion becomes a special type of communication concerned with the spread of messages that are new ideas. Essential elements in the diffusion of innovation are special cases of the components of the communication process.

The Communication Process	Diffusion of Innovation
The Source	Change Agents, Opinion leaders
Message	Innovation and its perceived attributes
Channels	Mass Media or Interpersonal communication
Receivers	Members of a social system
Effects	Consequences over time (knowledge, persuasion adoption/rejection)

Social change is the process by which alteration occurs in the structure and function of a social system. There are three sequential elements in the process of change. Rogers defines these as:

> a. Invention or the process by which new ideas are created or developed.
> b. Diffusion or the process by which new ideas are communicated to the members of a given social system.
> c. Consequences or the changes that occur within the social system as a result of the adoption or rejection of the innovation.

Thus diffusion becomes a subset of communication theory and social change and one cannot therefore utilize Roger's model of diffusion without taking into account all the other phases of the innovative process.

Rogers distinguishes 8 types of diffusion research in the 2400 publications.

1. Rate of adoption of an innovation in a social system.
2. Rate of adoption in different social systems.
3. Perceived attributes of innovations.
4. Innovativeness.
5. Earliness of knowing about innovations.
6. Opinion leadership.
7. Communication channel usage.
8. Consequences of innovation. (145)

He points out that overemphasis upon the nature of the innovation studied leads to separate diffusion traditions which in turn impedes the theoretical integration of the field (146). (Section 3 of this paper dealing with "specialized concerns of diffusion researchers" lends credibility to this argument.)

Rogers accumulates and <u>synthesizes</u> middle range generalizations from <u>empirical</u> results in the 8 types of research on diffusion of innovation. Each publication reporting empirical results is content analyzed in terms of its methods and the generalizations reported in it.

The major dependent variables in these generalizations turn out to be (1) rate of adoption, (2) innovativeness and (3) relative success of programs of change. His paradigm of diffusion applied across 8 types of empirical research yields the following significant generalizations and findings.

1. The perceived attributes of innovations are related to rate of adoption (147).
    1.1 The relative advantage of a new idea, as perceived by members of a social system, is positively related to its rate of adoption.
    1.2 The compatibility of a new idea is positively related to its rate of adoption.
    1.3 The complexity of an innovation, as perceived by members of a social system is not related to its rate of adoption.
    1.4 The trialability of an innovation as perceived by members of a social system is positively related to its rate of adoption.
    1.5 The observability of an innovation as perceived by members of a social system is positively related to its rate of adoption.

2. Various personal, socio-economic and communication variables are related to innovativeness (148).
    2.1 Earlier adopters have more years of education and are more literate than later adopters (age is no factor in either early or late adoption).
    2.2 Earlier adopters have greater empathy, are less dogmatic, and have a greater ability to deal with abstractions than do later adopters.
    2.3 Earlier adopters have a more favorable attitude toward change and risk than do later adopters.
    2.4 Earlier adopters have a more favorable attitude toward science than later adopters.

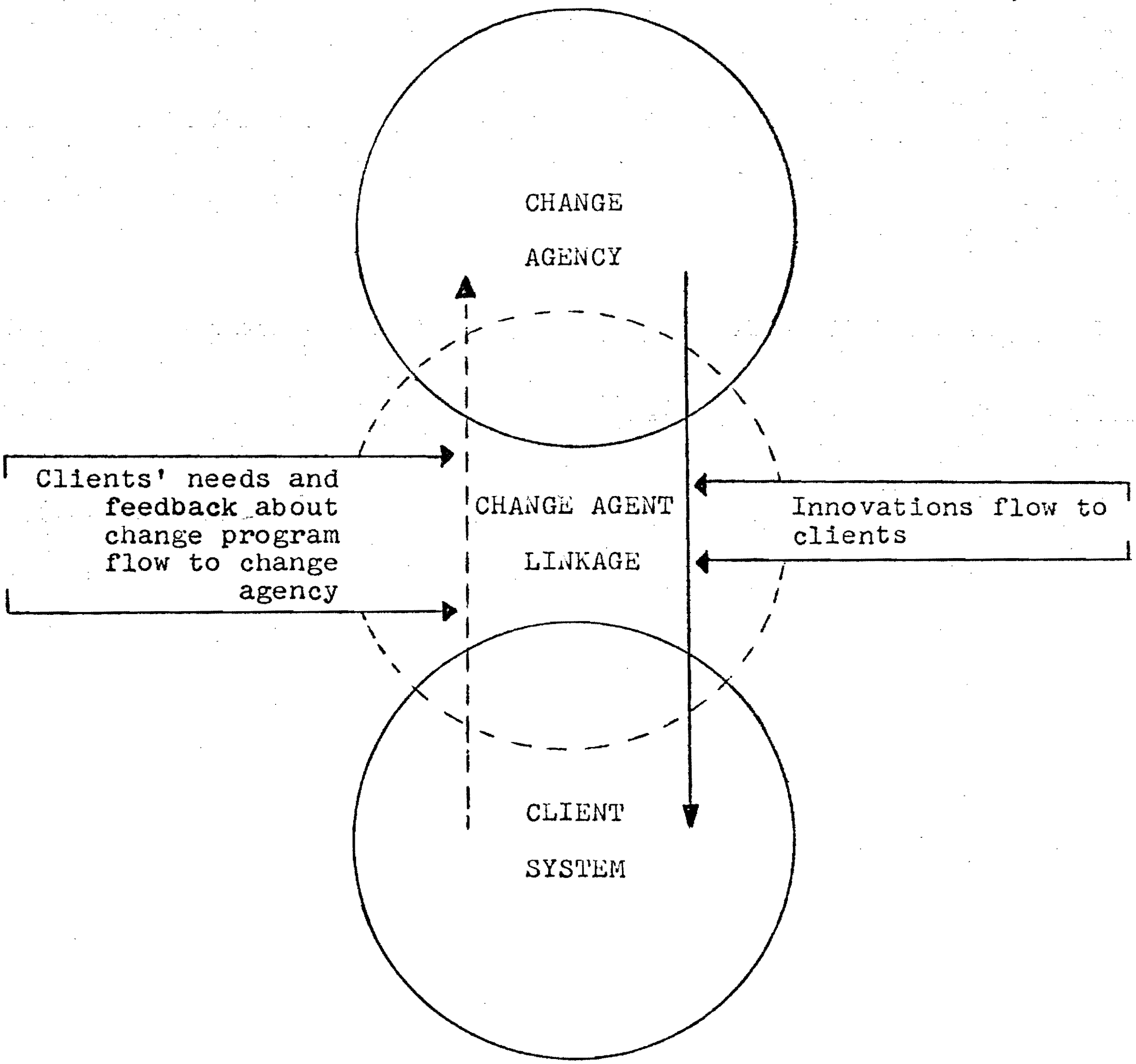
2.5 Earlier adopters have higher social status and a greater degree of upward social mobility than do later adopters.
2.6 Earlier adopters have larger sized units (farms, and so on) than do later adopters. They are also more likely to have a commercial (rather than a subsistence) orientation, and have a more favorable attitude toward credit than do later adopters.
2.7 Earlier adopters are more cosmopolite and have more social participation than later adopters.
2.8 Earlier adopters have greater exposure to mass media communication channels and more change agent contact than later adopters.
2.9 Earlier adopters have greater knowledge of innovations and have a higher degree of opinion leadership than later adopters.

3. Various personal, socio-economic, and communication variables are associated with opinion leadership (149).
3.1 Opinion leaders are more cosmopolite and innovative than their followers.
3.2 Opinion leaders have higher social status than their followers.
3.3 Opinion leaders have greater change agent contact as well as greater social participation than their followers.

4. Various personal, socio-economic, and communication variables are associated with change agent success (150).
4.1 Change agent success is positively related to effort and client orientation rather than change agency orientation.
4.2 Change agent success is positively related to the degree to which his program is compatible with client needs.
4.3 Change agent contact is positively related to higher social status, education and cosmopoliteness among clients.
4.4 Change agent contact is positively related to greater social participation among clients.

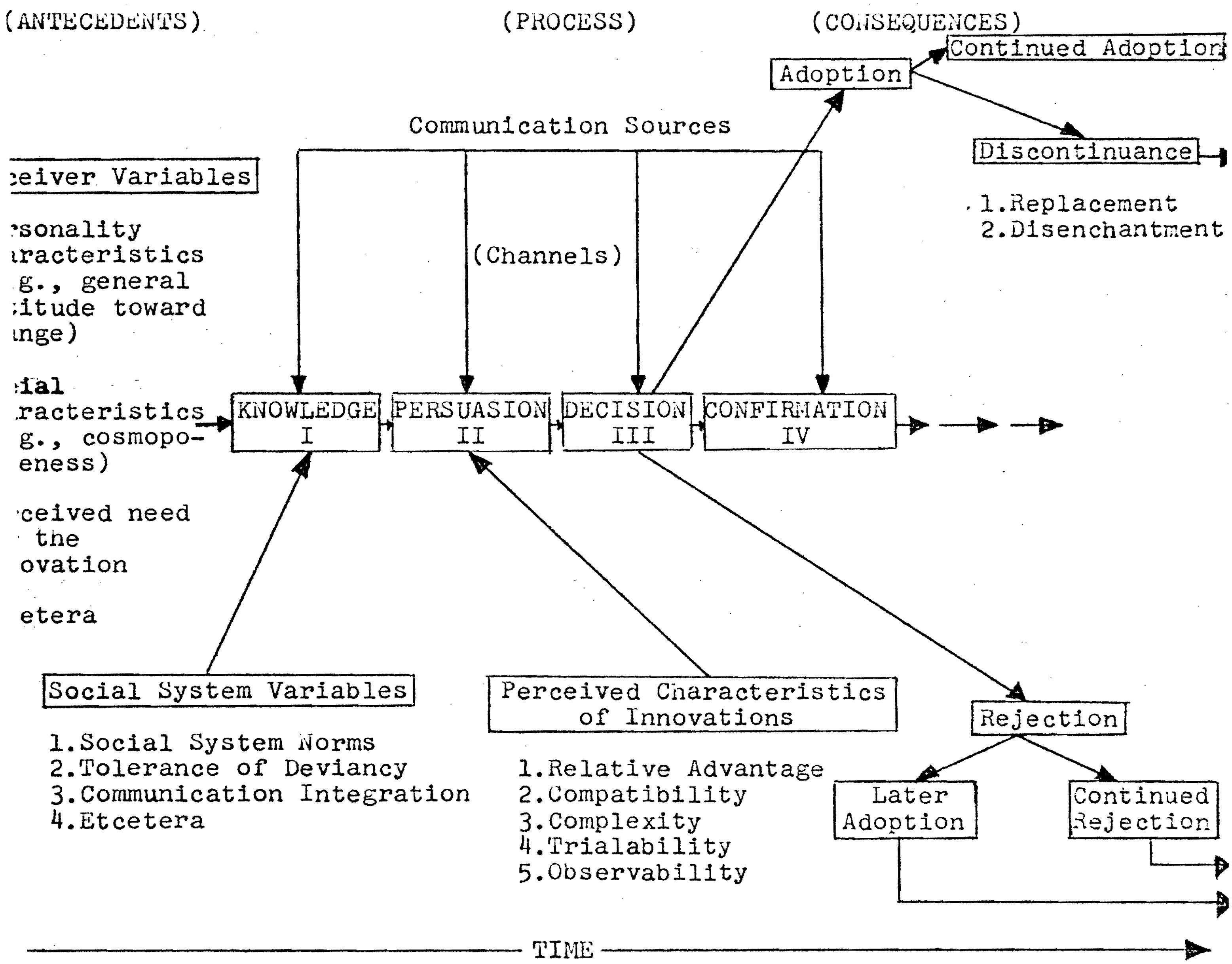
Rogers' model is based on studies that are largely drawn from agriculture (cross-culturally) and are primarily concerned with the adoption process within a microscale social system. In that sense the model though systematically developed is limited. It does not deal with the possible complexities and variations of source, except in the treatment of the change agent (who is usually an intermediary.) The change agency is briefly mentioned and is not dealt with to any great extent in the diffusion process. (151)

Change agents provide linkage between a change agency and a client system.



Rogers' emphasis on microscale diffusion is portrayed in his paradigm of the innovation-decision process (152).

Paradigm of the innovation-decision process.



The perceived characteristics of innovations, the perceiver's psycho-social, and socio-economic characteristics and the dynamics of interpersonal communication are the chief ingredients in the adoption and diffusion of innovation. Social system variables are treated as intervening variables.

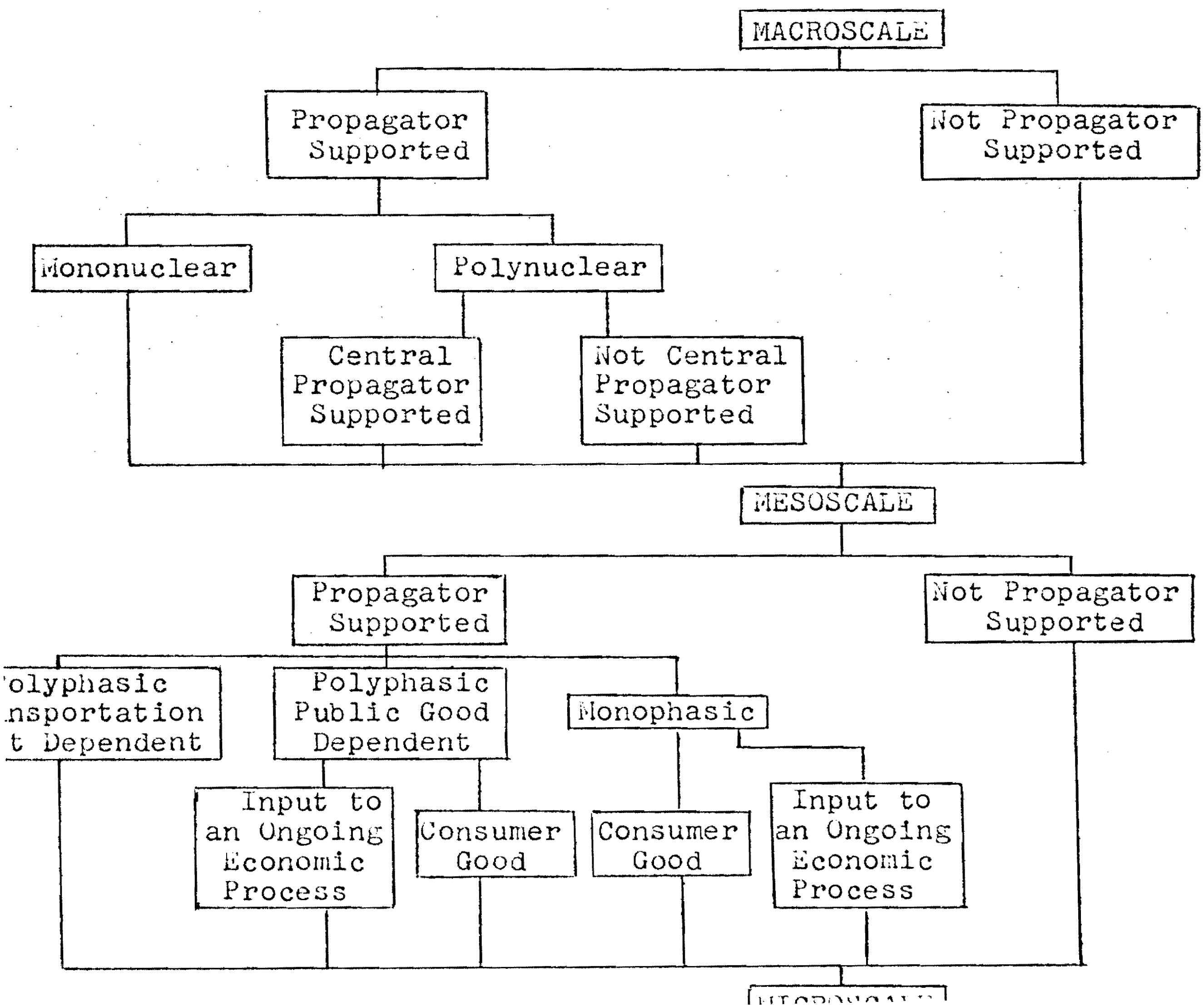
Rogers makes a distinct contribution in identifying the perceived characteristics of innovations, in developing a typology of innovativeness, in synthesizing the personal and social characteristics of opinion leaders and followers relative to innovativeness (from the 2400 publications in the Diffusion Documents Center.)

There are a number of methodological weaknesses in Rogers' generalizations. The 2400 publications are derived from investigations conducted in a variety of cultural settings. There is some difficulty in generalizing operational findings from one cultural setting to another. He treats all the publications as equivalent units of analysis. This may be questionable. It may be like comparing apples with oranges. The generalizations based upon simple bivariate association assume linear relationships. Rogers acknowledges that the real nature of diffusion is a cobweb of interrelationships among numerous variables (153). Comprehensive diffusion analysis should therefore incorporate multivariate analysis similar to the econometric models of Mansfield (1961-1968). The model however, should not be restricted to economic variables. Admittedly, it is more difficult to operationalize and mathematize sociological and psychological variables.

b. Lawrence A. Brown

Brown (1973) is interested in developing an overall systematic model of diffusion bridging the work of Myers and Marquis (1969) on propagator decisions to produce innovations and the work of Rogers with Shoemaker (1971) on information flow, persuasion and the adoption of innovations by individuals. He develops the following genealogical tree of the spatial diffusion of innovation in his 1973 National Science Founcation funded proposal.

A GENEALOGICAL TREE OF
THE SPATIAL DIFFUSION OF INNOVATION



Brown's objective is to identify conditions that influence spatial aspects of diffusion at the macro and meso stages and the patterns of diffusion generated by these conditions. (This hopefully may then be tied in with Roger's communication model.)

1. Construction of conceptual models relating to the macro and meso scale situations.

   a. Macroscale diffusion takes place within an urban system. This encompasses the processes of diffusion from the propagator of the innovation to diffusion agencies, including the establishment of the agencies themselves. Diffusion patterns are the result of decisions made by individuals associated with the propagation of the innovation either at the production or agency level. Brown and Cox (1971) indicate differences between situations in which there is propagator(s) of the innovation with an interest in its rapid and complete diffusion and those where there is not such an entity (In the latter instance, Roger's communication model of diffusion would be adequate.) Related to this is a distinction between situations involving both innovation diffusion agencies and aopters and those involving only adopters. Brown is interested in <u>the situation</u> involving both innovation diffusion agencies and adopters. In this instance there is a further conceptual distinction between a mononuclear propagation structure (single propagator entity) and polynuclear or fragmented propagation structures (many propagator entities.) Where there is a single propagator entity (mononuclear structure) the location and characteristics of the diffusion agencies are determined by it alone. This would then determine the macroscale pattern of diffusion. An example of mononuclear propagator structure would be the innovations associated with Holiday Inns. Where many propagator entities (polynuclear or fragmented) are involved, the location of diffusion agencies is carried on by each individually. Generally, each propagator will establish only one or a few agencies. Thus, characteristics of the diffusion agency system as a whole and the consequent macroscale patterns of diffusion represent the aggregation of many propagator decisions so that there is decentralization of decision making from an overall system point of view. An example would be diffusion of department stores in shopping centers. Underlying diffusion agency establishment in a mononuclear setting is the propagator's perception of a marketing surface (profitability) which in turn is related to its perception of a resistance surface and to the density of potential adopters (Brown and Cox, 1971.)

b. Meso scale diffusion takes place within the hinterland of a single urban center. The meso-scale primarily encompasses the processes of diffusion among individuals comprising that population. It involves the movement of innovation from diffusion agencies to the population at large. Diffusion patterns are the result both of decisions made by individuals associated with the propagation of the innovation at the agency level and of decisions made by individuals and households who are the ultimate users of the innovation. Institutional (transportation and electrical infrastructure) and agency actions constrain and mold the overall pattern of diffusion at the meso scale, defining its broad outlines, while details within this broad pattern are the result of household actions. At the meso scale level Brown makes a conceptual distinction between monophasic and polyphasic diffusion. Adoption in the pure monophasic case is only based upon flows occuring prior to adoption. The contents of flow involve information about the innovation itself. Monophasic diffusion occurs with innovations that are an input to an ongoing economic endeavor such as hybrid corn as well as innovations that are used directly by the adopter in his daily living such as a sewing machine.

Adoption of innovation in polyphasic diffusion is based upon a primary flow of the type associated with monophasic diffusion (information flow) and a secondary flow after adoption. One kind of secondary flow consists of the transport of some energy or service necessary to the functioning of the innovation. A second kind of secondary flow consists of transporting the output of the innovation from the location of the adopter to a market. The cost of the secondary flow may determine adoption of an innovation. If secondary flow costs are small, the decision to adopt is solely on the basis of the primary flow (information). If secondary flow costs do not show significant variation from place to place, the spatial pattern of adoption is not affected. (It would be useful to determine if this is consistent with Mansfield's rate of innovation. It is a legitimate test for his econometric model (Mansfield 1961-68) at the meso scale level of diffusion.) The response of the potential adopter where the innovation is propagator supported and polyphasic diffusion is taking place is conceptualized as depending upon a number of factors:

(1) the market price
(2) production and transportation cost related to the innovation
(3) opportunity cost of the innovation relative to that associated with alternative economic endeavors
(4) information about the innovation from both interpersonal and impersonal or media sources.

The last two (3 and 4) are in line with Roger's communication model of diffusion. Opportunity cost is synonymous with Roger's innovation attribute of "relative advantage." Brown in his funded NSF proposal (1973) assumes that market price and opportunity cost are spatially invariant; production costs are largely determined by site factors and transportation cost and information levels are largely determined by situation factors of the potential adopter. As with the macroscale, needed is the collection of data describing the diffusion of several innovations, representing all categories of monophasic and polyphasic diffusions and analysis to determine the correspondence between actual and expected patterns of diffusion. Brown therefore proceeds to phase 2 in his research design where he proposes to test and modify his simulated models of macro and meso scale.

2. Data collection and analysis relating directly to the proposed macro and meso scale conceptual models.

For his macro scale conceptual model, Brown proposed to use case studies of entrepreneurial policies and behavior guiding the marketing of innovations - planned regional shopping centers, for example. The research task would include the collection and analysis of interview data pertaining to actual cases of diffusion at the macro scale. There would be the application of interview or participant observer techniques to key actors in the macro diffusion process to identify the factors actually guiding their decisions. This would be buttressed by the collection and analysis of supplementary data from public or entrepreneurial records. To provide a tie in with the interviews, the supplementary data primarily will relate to innovations that are referred to by the marketing professionals in their interviews. For his meso scale conceptual model Brown proposes interviews of households in the hinterland of a single urban center and of entrepreneurial agencies serving that hinterland. The objective is to determine the interface and interactions between them leading to various patterns of diffusion.

3. Phase three consists of modifications to the conceptual models as a result of data collection and analysis.

4. Phase four would then consist of constructing operational models of the macro and meso scale diffusion processes as well as operational model integrating the macro, meso, and micro scales. Rogers would question Brown's conceptualization of macro and meso models. He would consider these aspects of classical grand theory which is untestable. At the same time Rogers criticizes raw empiricism and therefore espouses middle-range analysis which to him is most consistent with

analysis of micro scale diffusion. The omission of macro and meso scale generalizations (which must have some impact on Roger's micro scale generalizations) leaves him dangerously close to raw empiricism. His 2400 publications are not really equal units of analysis.

Brown claims that his macro scale diffusion model is testable. The framework for an operational model of macro scale diffusion is provided in the macro scale conceptual discussion. The interview and supplementary data will provide a basis for modifying the framework if necessary. Implementation of an operational model will employ this data as input for parameter estimation. The mathematical mode of the model cannot be specified until more is known about the diffusion system it will portray. By implementing such a model Brown and his coworkers expect to identify latent characteristics of the diffusion framework it embodies; to identify through sensitivity analysis (this is unspecified) the relative role and importance of each variable in determining diffusion patterns.

Rogers has a point about the importance of dyadic relationships in the diffusion process at every level. How about investigating communication patterns at the macro level. Someone must talk to someone else in policy development.

Brown's systematic diffusion research holds a great deal of promise. It should be possible to subsume Roger's "change agent" (Rogers, 1971) (154) under Brown's meso scale, propagator supported, monophasic phase spatial diffusion model. The synthesizing question Brown is trying to answer is how would agency location (macro and meso spatial diffusion) procedures vary given alteration of:

a. Innovation characteristics (Roger's concern) (155)
b. Firm characteristics (Mansfield's econometric concerns) (156)
c. Circumstances under which propagation is initiated (Myers and Marquis concern) (157)

What may be missing under firm characteristics is the inclusion of Burns and Stalker typology of management relative to innovation (158).

Both Rogers and Brown might take note of Utterback's observation that past work in the study of innovation in industry (and this would apply to agriculture as well) has been of a descriptive and non-cumulative nature (159).

5. Conclusions and Suggestions for Further Study

There are difficulties regarding comparability of results in the industrial, medical and agricultural diffusion studies. We are not dealing with equal units of analysis and there is bias in the "catch-as-catch-can" samples which form the base for empirical findings. There is no agreement on structured observation and methods of measurement and the findings are either heuristic, particularistic or tailored to ad hoc situations. The diffusion researchers are serious and competent, but most have highly specialized concerns and these are reflected in the many different ways the diffusion process is defined. This has led to the absence of an overall tradition in diffusion research and much painstakingly gathered valuable data threatens to become a fund of useless information.

A number of important studies particularly the econometric ones, are based primarily on the use of secondary data. There is no real knowledge as to whether or not the original data was collected, analyzed and recorded in a uniform manner. Furthermore, the highly particularistic nature of the subject matter makes it difficult to generalize the findings from one context to another. Nonetheless the econometric models carefully constructed for particular situations, could be modified and utilized in a more comprehensive diffusion model.

What is called for now is not the continued aggregation of valuable but disparate diffusion data but an overall conceptual approach to the diffusion precess. It is necessary to synthesize the conceptual thinking of Hagerstrand, Mansfield, Katz, Menzel, Rogers, Brown and Burns and Stalker into a multivariate model of diffusion of innovation. The literature reveals economic factors, organizational factors, cultural factors, information and communication factors, socio-economic and personality factors, ecological factors, technological factors and policy factors all related to the diffusion of innovation. Diffusion apparently takes place at every stage of the innovative process.

Problem definition

Invention

Research and development

Application

and in every context. (Just to mention a few)

International

Regional

National

Industry (within, between)

Firm (within, between, multinational)

Innovative persons

The multivariate model of diffusion needs to address itself to context and linkages with other phases of the innovative process. A milking of secondary information from data banks can be a starting point for scholarly studies regarding context and linkages between diffusion and the other aspects of the innovative process. For example the Georgia Tech Data Bank revealed that on Sept. 4, 1973 there were 1056 publications on the innovative process. 297 dealt with diffusion. The 297 diffusion studies were broken down into the following mutually exclusive categories

A TYPOLOGY OF DIFFUSION LINKAGES	f
Diffusion	171
Diffusion and Problem Definition	28
Diffusion and Invention	1
Diffusion and Research and Development	9
Diffusion and Application	36
Diffusion, Problem Definition, and Invention	4
Diffusion, Problem Definition and Rand D	1
Diffusion, Problem Definition and Application	8

Diffusion, Invention and Rand D	0
Diffusion, Invention and Application	7
Diffusion, Problem Definition, Invention, and Rand D	1
Diffusion, Problem Definition, Invention and Application	1
Diffusion, Problem Definition, Rand D, and Application	3
Diffusion, Invention, Rand D, and Application	9
Diffusion, Problem Definition, Invention, Rand D, and Application	7
	N= 297

The following questions can then be asked systematically in an exploratory study with a multi-variate conceptual framework.

1. What combinations of variables (factors) appear to be pertinent to each one of the linkages?
2. Do the combinations of variables for each of the linkages remain constant from one diffusion context to the next?
3. To what extent are data generated in answers to the first two questions derived from:
   a. empirical studies?
   b. non-empirical studies?
4. To what extent are there then either "empty calls" in the model or gaps in the literature?

Synthesis and modification of the conceptual model will follow from the emerging frequencies of cross-classification. The latter, initially, are a function of the computer literature classification system as developed in the research design.

Apparently studying a particular phase, such as diffusion apart from the other phases (problem definition, invention etc.) is merely an analytical device and not reality. Furthermore, if linkages between diffusion and other phases exist, then all the more reason for concerning oneself with the innovative process not merely from the point of view of the adopter. For these reasons we are not quite ready for a standard definition of diffusion.

We need more systematic knowledge about the role of the multi-national corporation in the diffusion process as well as military transfer of technology to civilian use. We know very little about the socio-economic characteristics and personality traits of propagators of innovation. Although we are familiar with information flow, both impersonal and inter-personal, on the microscale level, we are not quite familiar with the dynamics of the organization motivation model in the diffusion of innovation. But the very next step is an attempt at the development of a comprehensive multi-variate conceptual model(s) of the process of diffusion embracing its possible linkages and contexts.

BIBLIOGRAPHY AND NOTES

1. See Torsten Hagerstrand,"Diffusion: The Diffusion of Innovations" The International Encyclopedia of the Social Sciences, 1968, David L. Sills (ed.) Volume 4, P.174., New York, Mac Millan and FREE PRESS.

2. Rogers, Everett, M., with F. Floyd Shoemaket, COMMUNICATION OF INNOVATIONS: A CROSS-CULTURAL APPROACH, THE FREE PRESS, NEW YORK, 1971, P.7.

3. IBID, P.12.

4. Mansfield, Edwin; Rapoport, J., Schnee, V., Wagner, S. and Hamburger, M., Research and Innovation in the Modern Corporation, 1971, W.W. NORTON & CO., New York.

5. Morgan, Robert P. "TRANSFER OF TECHNOLOGY", (from industrialized countries to LATIN AMERICA: CONFERENCE PAPER) ACADEMY OF POLITICAL SCIENCE - PROCEEDINGS, AUGUST 1972, vol. 30, pp 141-152.

6. Fredriksen, E. Bruce, "Role of technology Transfer in Product Development and Marketing", Research Management, vol. 13, 1970, pp265-272.

7. Utterback, J.M., "The Process od Innovation: A Study of the Origination and Development of Ideas for New Scientific Instruments" IEEE Transactions vol. EM-18, No.4, Nov. 1971.

8. Alesch, D.J. "Strategy for State Government Innovation Through Science and Technology." Rand Report R-785 P.10.

9. Robertson, Thomas S., Innovative Behavior and Communication (Holt, Rinehart and Winston, Inc. 1971) P.20.

10. IBID. P.32.

11. Gruber, W.H., Factor in the Transfer of Technology, The M.I.T. Press, Cambridge, Mass. 1969.

12. Pavitt, K., "Performance in Technological Innovation and the Economy, M. Goldsmith (ed.) 1970, P.92. (Symposium held at Churchill College, Campridge, England, Spring 11-13, 1969)

13. Crane, Diana Invisible Colleges: Diffusion of Knowledge in Scientific Communities. Chicago: University of Chicago Press, 1972, P.23.

14. Utterback, James M., "Innovation in Industry and the Diffusion of Technology," Working Paper, Graduate School of Business, Indiana University, Bloomington, Indiana, June 1973.

15. Kranzberg, M., "The Ecology of Innovation"-Working Paper, 1973, Georgia Institute of Technology.

16. Rogers, Everett M., Diffusion on Innovations, 1962, New York: The Free Press of Glencoe.

17. Barnett, Homer G., Innovation: The Basis of Cultural Change, New York, 1953.

18. Rogers, Everett M. with Shoemaker, F. Floyd. Communication of Innovations: A Cross-Cultural Approach, The Free Press, New York, 1971.

19. IBID, See Appendix A for bivariate generalizations. Some examples are:

    Earlier knowers of an innovation have higher social status than later knowers. Earlier knowers of an innovation have more social participation than later knowers.
    The rate of awareness: knowledge for an innovation is more rapid than its rate of innovation.
    Earlier adopters have more years of education than do later adopters.
    Opinion leaders are more cosmopolite than their followers.

20. See Rogers, Introduction to Diffusion of Innovations, 1962.

21. See Hagerstrand 1968, pp 174-175.

22. IBID, P.176.

23. Hagerstrand, Torsten (1965), "Quantitative Techniques for Analysis of the Spread of Information and Technology," in C.A. Anderson and M.J. Bowman (Eds.) Education and Economic Development, Chicago, Aldine; Also see Rogers with Ahoemaker 1971, P. 194.

24. Hagerstrand, Torsten, A Monte Carlo Approach to Diffusion, European Journal of Sociology, vol.6, 43-67.

25. Hanneman, Gerhard J., A Computer Simulation of Information Diffusion in a Peasant Community, M.A. Thesis. East Lansing: Michigan State University; See Rogers with Shoemaker, 1971, P. 194.

26. See Hagerstrand 1968, P. 177.

27. Hagerstrand, Torsten 1952. The Propogation of Innovation Waves. Lund: Gleerup, Lund Studies in Geography.

28. Hagerstrand, Torsten 1967. Innovation Diffusion as a spatial Process. Chicago: University of Chicago Press.

29. Brown, Lawrence A. 1968 Diffusion Dynamics: A Review and Revision of the Quantitative Theory of the Spatial Diffusion of Innovation. Lund: Gleerup, Lund Studies in Geography.

30. Brown, L.A. 1969. "Diffusion of Innovation:A macro view," Economic Development and Cultural Change, 17:189-209.

31. Brown, L.A. and K.R. Cox. 1971. "Empirical Regularities in the Diffusion of Innovation", Annals of the Association of the Association of American Geographers, 61:551-559.

32. Brown, L.A. 1972 " Diffusion Processes: Recent Developments and Their Relevance to Growth Pole Effects," Paper prepared for the International Geographical Union Conference on Regional Aspects of Economic Development.

33. Brown, L.A. 1973. "Diffusion Processes at the Macro and Meso Scales: An Update on a Framework for Future Analysis," Economic Geography

34. Myers, S. and D.G. Marguis. 1969. Successful Industrial Innovations: A Study of Factors Underlying Innovation in Selected Firms. Washington: U.S. Government Printing Office for National Science Foundation.

35. See Rogers with Shoemaker, 1971.

36. Griliches, Zvi "Hybrid Corn: An Exploration in the Economics of Technological Change". Econometrica 1957, vol.25, No.4, 501-522.

37. IBID.

38. Nelson, R.R., M.J. Peck and E.D. Kalachek, Technology, Economic Growth and Public Policy, The Brookings Institution, Washington, D.C. 1967.

39. Schmookler, J. Invention and Economic Growth 1966, Harvard University Press.

40. Mansfield, Edwin "Technological Change: Measurement, Determinants and Diffusion", Report to President of the National Committee on Technology, Automation and Economic Progress, 1966.

41. IBID, P.123.

42. Mansfield, Edwin, "Technical Change and Rate of Innovation Econometrica, October 1961, vol.29, 741-766.

43. IBID, P.763.

44. See Griliches, Econometrica, 1957.

45. Mansfield, Tech. Change: Measurement etc. P.123

46. Rogers with Shoemaker, 1971, P.154

47. Kivlin, Joseph E. (1960), Characteristics of Farm Practices Associated with Rate of Adoption: Ph.D. thesis, University Park: Pennsylvania State University.

48. Singh, Ram N. (1966) Characteristics of Farm Innovations Associated with the Rate of Adoption. Guelph: Ontario, Agricultural Extension Education, Report 14.

49. Petrini, Frank (1966) The Rate of Adoption of Selected Agricultural Innovations. Uppsala: Agricultural College of Sweden, Report 53.

50. Rogers (1971), P. 154.

51. IBID, P. 156.

52. Hruschlsa, E. and Rheinwald, H. (1965). "The Effectiveness of German Pilot Farms," Sociologia Ruralis, 5:101-111.

53. Mansfield (1961), P.763.

54. Rogers (1971), P.155.

55. Fliegel, Frederick C. and Kivlin, Joseph E. (1966), "Farmers' Perception of Farm Practice Attributes," Rural Sociology 31:197-206.

56. IBID. Singh (1966).

57. Fliegel et al. (1968), " A Cross-National Comparison of Farmers' Perceptions cd Innovations as Related to Adoption Behavior," Rural Sociology, 33:437-449.

58. Rogers (1971) P.113.

59. Mason, Robert G. "The Use of Information Sources in the Process of Adoption," Rural Sociology, 29: 40-52.

60. Francis, David G. and Rogers, Everett M. (1962), "Adoption of a Non-Recommended Innovation: The Grass Incubator." Paper presented at the Rural Sociology Society, University Park, Pa.

61. See findings, Mansfield (1961) P.763.

62. Mansfield, Edwin, " Intrafirm Rates of Diffusion of an Innovation," Review of Economics and Statistics, vol.45, no.4, P.348, Nov. 1963.

63. Mansfield, Edwin, Industrial Research and Technological Innovation: An Econometric Analysis 1968, New York, W.W. Norton & Co., Inc.

64. IBID, Chapter 8.

65. IBID, P.204.

66. IBID.

67. IBID, P.203.

68. Mansfield, Edwin, "The Speed of Response of Firms to New Techniques" Quarterly Journal of Economics, May 1963.

69. IBID, Mansfield, 1968, pp202-204; Mansfield et al (1971) Ch. 9.

70. Mansfield et al (1971).

71. IBID, See Chapter 9.

72. Barnett, Homer G., Innovation: The Basis of Cultural Change, 1953, New York McGraw-Hill.

73. IBID, pp.330-360.

74. IBID, Rogers (1971) P.169.

75. IBID, P.142.

76. IBID Appendix A, pp.350-351.

77. Spicer, Edward, H. (ed.) Human Problems in Technological Change 1952, Russell Sage Foundation, New York.

78. Bright, James R. Research, Development and Technological Innovation 1964. Chapter "On Resistance to Technological Innovation," P.130.

79. Rokeach, Milton 1960 The Open-Closed Mind, Basis Books New York.

80. Bruner, Jerime and Postmar, L."Emotional Selectivity in Perception and Reaction," J. Personality, XVI (1947), 69-77.

81. Mansfield et al (1971) Chapter 9.

82. Rogers (1971) pp.140-141.

83. IBID, 353-355.

84. IBID, 361-363.

85. Nelson, Richard R. and Phelps Edmind S. "Investment in Humans, Technological Diffusion and Economic Growth", American Economic Review.

86. IBID, American Economic Association, Papers and Proceeding, 1966 56:69-75.

87. See Rogers, 1962, Diffusion of Innovations Chapters.

88. Rogers (1971) P.348.

89. See Mitzner, Morris, 1964, Some effect of Dogmatism, Anxiety and Perceptual Closure on Response to Unfamiliar Stimuli, Unpublished M.A. thesis, Emory University 1964.

90. See Nelson and Phelps (1966) P.75.

91. Hayami, Y. and Rutton, V.W. (1971), "Agricultural Development: An International Perspective, The John Hophins Press, Baltimore, Md.

92. Hayami, Y. and Rutton, V.W. (1973), "Transfer in Argricultural Development", Technology and Culture, 14, April 1973, 119-151.

93. Crane, Diana (1972) Invisible Colleges: Diffusion of Knowledge in Scientific Communities. Chicago: University of Chicago Press.

94. IBID, pp.70-75.

95. Needham, Sir Joseph, "Limiting Factors in the Advancement of Science as Observed in the Field of Embryology," Yale Journal of Biology and Medecine, 1935, vol.8.

96. Ben-David, Joseph, The Scientist's Role in Society, 1971, Foundations of Modern Sociology Series, Prentice-Hall Inc., Englewood Cliffs, New Jersey.

97. IBID, P.17.

98. Allen, Thomas.

99. Price, Derek.

100. Journal of Garfield the Association of Engineers and Architects in Israel, March 1973, No. 2, vol.XXXI.

101. IBID, 2.

102. IBID, 22-27.

103. Schon, Donald A. Technology and Change: The New Heraclitus, 1967, Delacorte, New York.

104. IBID,P.36.

105. IBID,P.169.

106. IBID,P.178.

107. Dubin, Robert, The World of Work (Englewood Cliffs, New Jersey, Prentice-Hall, Inc., 1958), pp 213-217.

108. Vroom, Victor H., "Some Psychological Aspects of Organizational Control" in New Perpectives in Organization Research, eds, William W. Cooper, Harold J. Leavitt and Maynard W. Shelly II. (New York-London-Sydney, John Wiley and Sons, Inc., 1964), P.72.

109. Dubin, Robert, "Motivation of Organization Activities", Human Relations in Administration, 2nd edition,ed. Robert Dubin (Englewood Cliffs, New Jersey, Prentice-Hall, Inc., 1961) pp49-55.

110. Mitzner, Morris, An Investigation of the Effects of Prestige, Compliance Patterns and Dogmatism On Interpersonal Perception of Esteem in the Chain of Command of Formal Organization, Unpublished Doctoral Dissertation, Emory University, 1968, pp159-160.

111. Burns, Tom and Stalker, G.M. The Management of Innovation, 1961, London, Tavistock Publications.

112. Etzioni, Amitai, A Comparative Analysis of Complex Organizations (New York, The Free Press, 1961).

113. IBIB, P.XIV.

114. IBID, P.3

115. IBID, P.11.

116. Miller, George A., "Professionals in Bureaucracy: Alienation Among Industrial Scientists and Engineers," A. S. R., XXXII (October, 1967), 755-768.

117. Kornhauser, William, Scientists in Industry: Conflict and Accommodation (Berkeley, University of California Press, 1962), pp. 148-149.

118. Miller 765.

119. IBID, 765-766.

120. Doby, John T., Introduction To Social Psychology, 1966.

121. IBID, P.146.

122. Hilgard, Ernest R., "The Role of Learning in Perception," Perception: An Approach to Personality, eds. Robert R. Blake and Glenn V. Ramsey (New York, The Ronald Press, 1951), pp. 95-120.

123. Allport, Gordon, Personality (New York, Henry Holt and Company, Inc., 1937), P. 295.

124. Dubin, P. 51.

125. IBID, pp.53-54.

126. Moss, Howard A., "The Influences of Personality and Situational Cautiousness on Conceptual Behaviour," J. Abn. Soc. Psych., LXIII (1961), 629-635.

127. Asch, Solomon E., "Effects of Group Pressure Upon the Modification and Distortion of Judgments," Groups, Leadership and Men, ed. Harold Guetzkow (Pittsburgh, Carnegie Press, 1951); Kurt W. Back, "Influence Through Social Communication," J. Abn. Soc. Psych., XLVI (1951), 9-23; Kurt Lewin, "Group Decision and Social Change," Readings in Social Psychology, 3rd edition, eds. Maccoby, Newcomb and Hartley (New York, Holt, Rinehart and Winston, Inc., 1958), pp. 197-211; Edith B. Bennett, "Discussion, Decision, Commitment and Consensus in Group Decision," Human Relations, VIII (1955), 251-274; Muzafer Sherif, "An Experimental Approach to the Study of Attitudes," Sociometry, 1 (1937), 90-98.

128. Farris, George F. "The Effect of Individual Roles on Performance in Innovative Groups," R and D Management, vol.3, No.1, 1972.

129. Rogers, Evereth M., June 1973, Organizational Diffusion Bibliography of the Diffusion Documents Center Funded by the National Research and Development Assessment Program of the National Science Foundation, through the George Washington University.

130. Coleman, James; Katz, Elihu; and Menzel, Herbert; "The Diffusion of an Innovation Among Physicians," Sociometry, Dec. 1957, vol.20, pp. 253-270.

131. Katz, Elihu, " The Two-Step Flow of Communication: An Up-To-Date Report on an Hypothsis," Public Opinion Quarterly, vol.21, pp61-78.

132. Katz, Elihu and Levin, Martin L. (1959) "Traditions of Research on the Diffusion of Innovation." Paper presented at the American Sociological Association, Chicago.

133. Katz, Elihu (1961) "The Social Itinerary of Technical Change: Two Studies on the Diffusion of Innovation," in Wilbur Schramm (Ed.), Studies of Innovation and of Communication to the Public. Stanford, Calif. Stanford University.

134. See Rogers (1962) P.54.

135. Ryan, Bryce, and Gross, Neal C., (1943) "The Diffusion of Hybrid Seed Corn in Two Iowa Communities," Rural Sociology, 8: 15-24.

136. Allen, Thomas J. "Roles in Technical Communication Networks," in Communication Among Scientists and Engineers, 1970, D.C. Heath and Co. P. 190.

137. See Rogers (1971) Chapter 2.

138. Associated Press Report Release Sept. 12, 1973, in Atlanta Constitution.

139. See Mansfield (1971), "Union Policies Toward New Techniques," in Technological Change, pp. 110-116.

140. Rosenbloom, R., "The Transfer of Military Technology to Civilian Use," in Kranzberg, M. and Pursell, C. (eds.), Technology in Western Civilization, vol.2, New York: Oxford University Press, 1967, pp. 601-612.

141. IBID, P. 603.

142. IBID, P. 605.

143. IBID, P. 609.

144. See Brown (1973).

145. See Rogers with Shoemaker (Chap. 2).

146. IBID.

147. IBID, pp. 350-352.

148. IBID, pp. 352-376.

149. IBID, pp. 378-380.

150. IBID, pp. 380-382.

151. IBID, P. 228.

152. IBID, P. 102.

153. IBID, P.93.

154. IBID, P.228.

155. IBIB, Rogers (1971).

156. IBID, Mansfield (1961-1968).

157. IBID, Myers and Margins (1969).

158. See Burns and Stalker 1961.

159. See Utterback working paper 1973.

APPENDIX I

LITERATURE SEARCH AND CODING PROCEDURE

Appendix 1.

Report on the Literature Search Phase of the Innovation Study (NSF Grant G43-602)

Department of Social Sciences
Georgia Institute of Technology

Prepared by:
N. J. Norman

August 29, 1973

(1)

Purpose of this Report

The purpose of this report is to describe the effort undertaken by the Georgia Tech Department of Social Sciences in preparing a data base for the project conducted under NSF Grant No. G43-602. This report is meant to serve not only as a documentation of the efforts involved in generating and computerizing the data base, but also as a guide for any future effort of this nature. It is hoped that those who read it can profit from the experience of this writer and his associates, and gain an insight into the processes involved in organizing and managing a project such as this one.

Objective of the Project

The purpose of this project, provided for by NSF Grant No. G43-602, is to determine and critically assess the present knowledge and understanding of the process of technological innovation. The major product of this project will be a final report to the NSF Office of National R&D Assessment.

Two major phases to be covered are the analytic phase and the assessment phase. The former involves both the classification and coding of a large body of the most recent research literature on the innovation process, and an in-depth abstracting of the most significant subset of this literature. The latter will be concerned with the quality of understanding of the process of technological innovation on both the theoretical and empirical levels. Included in this phase will be a state-of-the-art assessment, prepared by the Georgia Tech project group, and a number of complementary assessments prepared by consultants, known for their outstanding research in their special fields.

This report describes the work undertaken under the analytic phase in the classification and coding of a large body of literature. Reports on the other phases of the overall project will be prepared as they are completed.

Sources of Readings

A list of indexing and/or abstracting services is provided below with an explanation of usage and a subjective opinion of value by each entry. It was decided not to use any information prior to 1965, since the data base is to be used in a state-of-the-art study.

1) Business Periodicals Index: usage covers from ________ to ________. Some fairly good articles. Slightly below average in usefulness.

2) Economic Abstracts: usage covers from June 65 to May 72. Most articles were reasonably good, and occasionally an excellent article was found. Somewhat difficult to use, owing to the large number of articles not in English. ~~Above~~ About average.

3) Journal of Economic Literature: usage covers 1969-1972. Reasonably good articles. Not used for pre-1969 articles, since this time frame also covered by next indexing service. About average

4) Index of Economic Articles in Journals and Collective Volumes (American Economic Association): usage covers 1965-1968. Also reasonably good articles, but index is difficult to use ~~with books referenced~~ when referencing books. About average.

5) Public Affairs Information Service Bulletin: usage covers Oct 1964-July 14, 1973. One of the better sources of good articles. Above average.

6) Social Sciences and Humanities Index: usage covers April 1965-March 1973. Not quite as useful as PAIS, but still above average.

7) Sociological Abstracts: usage covers 1969-1971. Not very productive and very difficult to use. Below average.

8) U.S. Library of Congress Catalog of Books: not yet used extensively. Only 1971 volume covered thus far, with most books being duplications. About average.

9) Selected Rand Abstracts: usage covers 1966-1972. Although initially promising, this source did not provide the orientation needed for this project (too technical and specific, much forecasting, etc.). Below average.

10) Government Reports Announcement: usage covers 1971-March 1973. Not very productive and difficult to use. Rand Abstracts better. Below average.

11) Operations Research/Management Science Yearbook: usage covers 1971, only. This source was not used further because Dr. Baker provided the work group with several lists and bibliographies which he said should cover 95% of the references in this source.

12) Applied Science and Technology Index: usage covers 1965-May 1973. Many good articles. Above average.

13) Innovation Search - this journal has discontinued publication. The library throws out all but the most current issues. Only two were found.

14) Dissertation Abstracts: this source was not used since the abstracts were generally too short to warrant adequate coding. The dissertations themselves are not in the library, unless written by a Georgia Tech Ph.D. candidate.

15) The Engineering Index: usage covers 1969-Feb 1973. Generally, many good articles ~~and many of them~~ Above average.

16) Scientific and Technological Aerospace Abstracts (STAR): usage covers 1971. Highly technical and difficult to use. Below average.

17) International Aerospace Abstracts: usage covers 1972. Highly technical and difficult to use. Below average.

18) Science Citation Index: usage covers 1972, partially. <u>Very</u> difficult to use. Below average.

19) Subject Guide to Books in Print: usage covers 1972. Somewhat useful. About average.

20) Cumulative Book Index: usage covers 1971 & 1972. Somewhat useful. About average.

Other sources not widely distributed that were used include:

21) Core Bibliography from the Diffusion Documents Center of Michigan State University. Some articles useful, some not. About average.

22) Bibliography from the Center for Research on the Utilization of Scientific Knowledge (CRUSK) of the University of Michigan. Many articles in this listing, but only a small percentage useful to us. About average.

23) NASA Literature Search on Management of Research and Development. Most articles too technical for our purpose. Below average.

In addition to these indexing and/or abstracting services, various publications were found which devoted an entire issue to some facet of the innovation process. On other occasions, a book or article was discovered through pure chance that looked useful. These items were recorded in a "Serendipity File", which was periodically emptied and its contents listed and placed in the administrative system.

The faculty also provided some coding input. Their contributions came about as the result of coding an article being abstracted, or coding an article which they discovered that appeared to be useful.

Workroom Setup

Two adjoining workrooms were provided to the project by the library. (It should be noted that the library provided support for this project throughout the summer, and their cooperation and assistance are deeply appreciated.) One room was used primarily for filing and general administrative operations while the other was primarily a reading room. A remote access computer terminal and a telephone were installed in the administrative room. The terminal made excessive noise and should have been located away from the reading area. An alternative would be to use a silent terminal which would not be distracting to the coders.

Generating Articles

It was soon discovered that one person would be needed full time to generate articles and attend to the administrative details of the project. In fact, the general administration proved to be more time-consuming than was originally anticipated. Any future project of this nature should be critically assessed toward the end of reducing the administrative requirements involved. To generate a list of articles, the administrator would select an indexing or abstracting service (see Sources of Readings), record his choice on a chart to prevent duplication, and review the index looking for key words.

The references would then be copied by hand or Xeroxed and assigned unique numbers, determined by a scheme developed internally. Each article and its number were typed onto a 3"x5" card by the secretary who then filed them alphabetically by author's name. If a duplication is discovered, it was noted on the original list and on the card already in the file, while the new card was destroyed. The list was then given to someone who searched the library, pulled the books, and deposited them in the workroom. In this project, two girls working 1/4 time each proved to be sufficient. After the books were retrieved, the particular article had to be located in the volume and marked with a card indicating the article number. The volumes were then given to the coders who reviewed the articles and placed the volumes on a nearby table. The library personnel would then reshelve the books.

Exhibit 1

Index Subject Headings

Often, a book would not be in the library or not on the shelf at that particular time. Lists were made for each category and the "Not on Shelf" list was reviewed periodically to locate those books or volumes. If a book was not found on the third check, it was assumed lost and no further searches were made for that particular item.

Once it was read, an article was either coded or "dumped", which meant it was determined to be of no importance to this project. If the item was felt to be important, it was coded (see Coding) and eventually the coding sheet information was punched onto a computer card. (Originally, it was intended that the information would be input directly into a computer file via the remote terminal. However, due to the excessive noise of the terminal and frequent computer malfunctions, cards were deemed better.) All computer cards were checked and stored in the workroom until a systems run was required. Then it was a simple matter to load the data deck into the computer and then perform retrieval runs (barring computer malfunctions!).

Coding

Books and articles were coded accordingly to a specially prepared Coding Sheet, shown in Exhibit 2. Unfortunately, the large amount of information desired from each article necessitated creating two cards for each article. This was unfortunate because a one-to-one matching of the appropriate cards was then necessary in the computer program, which proved to be a problem as far as input was concerned. These two cards were the bibliography ("B") card and the coding ("Code") card. Since the article number had been previously assigned (see Generating Articles), the coder needed only copy this number into the code and bib cards. Dual entries were necessary in order to match the cards in the program. In order to input some information that did not easily correspond to the coding format, a series of coding conventions were devised, which are listed here:

1) If weekly publications did not have a volume and number specified, the month and year was not sufficient to identify the issue. In this case, the day of the month was entered in the "number" field of the Bib Card.

2) Because of restrictions in the computer program, only the senior author can be listed. His last name is entered first, followed by his initials in a spaced format. For example, an article by William A. Jones and Thomas R. Horton would have the author listed as: JONES W A with no mention of the second author.

lling Card:

eld	Code	Entry
-4	_ _ _ _	Article Number
-6	_ _	Month
-8	_ _	Year
	_	Type of Publication
-13	_ _ _ _	Name of Publication
	_	Abstracted?

COMMENT:

Process Phases:

_ Prob. Defn. & Idea Generation
_ Invention
_ R & D
_ Innovation
_ Diffusion

Definitions offered? If so, write out and file

Types of Variables:

_ Time
_ Organizational Factors
_ Economic Factors
_ Capital Intensive
_ Capital Saving
_ Labor Saving
_ Improved Natural Resource Utilization
_ Technical Factors
_ Decision Making
_ Decision Making - Descriptive
_ Decision Making - Prescriptive
_ Decision Making - Predictive
_ Human Variables - Individual
_ Human Variables - Social
_ Government Policy
_ Regulatory Agencies
_ Patent System/Anti-Trust Laws
_ Tax Incentives/Regulations
_ Expenditure Mechanisms

Process Context:

_ International
_ Regional
_ National
_ Govt Level
_ Industry
_ Govt Agency
_ Firm
_ Large Firm, Over $100 Million Sales
_ Med. Firm, $50-100 Million Sales
_ Small Firm, Less Than $50 Million Sales
_ Innovative Person
_ Individual Innovation

Procedural Approach:

_ Case Study
_ Controlled Experiment
_ Data Survey/Area Review
_ Antecdote
_ Literature Search
_ Speculation

Purpose:

_ Theoretical Development
_ Methodology Development
_ Hypothesis Development
_ Hypothesis Testing
_ How to Achieve Specific Objectives

Descriptors:

63 _ _ Field of Technology (use SIC Code)
_ Process
_ Device
_ Product
_ Service
_ Commercial Success or Failure
_ Human Values
_ Technology Assessment
_ Technology Forecasting
_ Technology Utilization
_ Environmental Quality

74 _ Innovation in the Public Sector
75 _ Rating by coder
76 _ Coder identification number

3) On the articles from the Rand Corporation, the company provides an article number which is placed in the "volume" column of the Bib card. "Number" and "page" are left blank. The internally assigned number is still placed in the first four columns of each card. Also, Rand has three types of publications: papers, reports, and Rand memoranda, identified on their documents by P, R, and RM. Since each type of publication has its own group of Rand-assigned numbers, the letters are omitted from the Bib card.

4) If a document is published quarterly, the "month" column is coded as follows:

Spring	13
Summer	14
Fall/Autumn	15
Winter	16

5) Some articles (numbers 1101-1112, for example) are only identifiable by their microfiche number. For these items, the Bib and Code cards are coded as follows: Microfiche number PB 204436 is coded

P B _ M	Volume	BIB.
_ _	Number	
_ _ 3 6	Page	
_ _	Year	
2 0	Month	CODE.
4 4	Year	

When this article is identified in a search, it will be printed as follows:

Date	Page	Volume	#
20 44	36	PB M	

(The "M" under volume indicates "Microfiche.")

It should be emphasized that under this coding convention, the "Page", "Year", etc. entries no longer correspond to the actual page, year, etc. of the article.

6) The identifying numbers for Scientific Technical Aerospace Reports (STAR) are coded as follows: STAR document N71-23506 is input as

S T A R	Volume	BIB.
	Number	
_ 5 0 6	Page	
_ _	Year	
7 1	Month	CODE.
2 3	Year	

A computer printout of this number is as follows:

Date	Page	Volume	#
71 23	506	STAR	

(The "N" is not input since it appears to be common to all STAR articles.)

As with the microfiche articles, "Page", "Year", and other entries do not correspond to the actual page, year, etc. of the article, under this convention.

7) After some deliberation, it was decided that an article which discusses a single country, other than the United States, would be coded "International" under the "Process Context" section of the Code Card.

8) The "type of publication" and "name of publication" entries were controlled by one person, according to the following convention: ~~Type of publication~~

Type of publication: (Cont. from previous page)

0 No coding

1 Journal

2 Book

3 Technical Report

4 Government Document

5 Proceedings/Conference Papers

6 Abstracting and Indexing Journal

7 Theses/Dissertation

8 Working Papers

9 (Open - not used)

The coder would enter the appropriate number and write down, in the margin, the name of the publication. This particular publication was later assigned a number, if it did not already have one, and listed on a Directory List. This list was also stored in the computer as part of the retrieval program.

Except for three items, all spaces on the coding sheet from number 14 are coded "zero-one." If the article deals with the item listed, a "1" is placed in the space - if not, it is left blank which will later be considered a zero. The definitions of these items are presented in Exhibit 3. Items 62 and 63 on the [illegible] card are used to describe the field of technology covered in the article. This list is presented in Exhibit 4. Note - if the article touches on more than one field of technology, the field which receives "the major emphasis" of the article is listed. However, if the article emphasizes more

Definitions (For use in conjunction with coding sheet)

Process Phase

15. Problem Definition and Idea Generation

Recognition of a potential technological capability which may be related to a scientific discovery and/or perceived need or opportunity, and the concept of a product, process or device.

16. Invention

A product, process, technique, or device which incorporates a technological capability with a perceived need or opportunity.

17. R&D

The developmental work undertaken to convert the invention into a useable, feasible end result.

18. Innovation

A successful application of a new technology and its first commercial introduction.

19. Diffusion

The spread of an innovation beyond its original context to other contexts.

Types of Variables

20. Time

Does the literature item contain a discussion of time as a variable in the innovation process? Yes/No

21. Organizational Factors

Does it deal with the interface of innovation with the chain of command or line of authority, the division of labor, or the plant rules. Yes/No

22. Economic Factors

Does the item treat innovation as an economic variable? Yes/No

If yes, then: Does it treat innovation as

23. Capital Intensive (increased capital/output ratio)
24. Capital Saving (reduced capital/output ratio)
25. Labor Saving (reduced labor/output ratio)
26. Improved Natural Resource Utilization
(lower natural resource/output ratio)

27. Technical Factors

Does it discuss the scientific or technical knowledge and factors upon which the innovation is based? Yes/No

28. Decision Making

Does the item discuss the process by which choices are made among competeing alternatives? Yes/No

If yes, then: Does it discuss the decision making process

29. Descriptively? (How decisions were arrived at)
30. Prescriptively? (How decisions should be made)
31. Predictively? (Decision making related to the possible prediction of outcomes)

Human Variables

Does it discuss the manner in which human behavior or attitudes impacts or the innovation process, either:

32. Human variables--Individual
33. Human variables--Social (the role of interpersonal an group relations)

34. Government Policy

Does the item discuss specific governmental policies, either restrictive or facilitating, in relation to innovation? Yes/No

If yes, then: Does it discuss;

35. Regulatory Agencies
36. Patent System/Anti-Trust Laws
37. Tax Incentives/Regulations
38. Expenditure Mechanisms

42. Government Level

Federal, state or local

50. Individual Innovation

Item dealing with a particular innovation

51. Case Study

The intensive study of selected examples

52. Controlled Experiment

The simultaneous gathering of various lines of evidence or data in order to test an hypothesis that one variable "x" is related to or influences another variable "y".

53. Data Survey

Data collection in descriptive studies using procedures such as interviews, questionnaires, direct observation, analysis of records, and participant observation.

54. Anecdote

The recording of the personal experiences, or the direct observation of the experiences of others, by an individual who is in a position to do so.

55. Literature Search

A bibliographical survey of studies relevant to the area of interest to review and build upon the work already done by others.

56. Speculation

A method not based on experimental test or observation.

Purpose

57. Theoretical Development

The presentation of a theory to explain or predict an aspect of the innovation process (or in some cases the whole process)

58. Methodology Development

The presentation of a new (or refined) method or procedure for studying the innovation process

59. Hypothesis Development

The development or presentation of hypothesis to explain or predict relationships between independently defined variables in the innovation process

60. Hypothesis Testing

Presentation of the results of the testing of an hypothesis dealing with the innovation process

64. Process

Items which discuss an innovation in the process by which something is done

65. <u>Device</u>

An <u>intermediate</u> good - an innovation which facilitates the achievement of some desired result - not normally a consumer item - example: a pollution control device on an auto

66. <u>Product</u>

A <u>final</u> good - an item to be used or consumed - normally retailed commercially; examples: a four-speed electric toothbrush

69. <u>Human Values</u>

A literature item which discusses some relationship between a technological innovation (or the process itself) and some socially desirable or undesirable end(s)

70. <u>Technology Assessment</u>

Items which discuss technological innovation in terms of an assessment of secondary or higher order consequences that have (or might) result from it.

74. <u>Innovation in the Public Sector</u>

Items that talk about innovations in such public sector areas as housing, transportation, health care delivery, etc.

FIELD OF TECHNOLOGY

00 no coding -- unable to classify

01 Agricultural

08 Forestry

09 Fishing, Hunting, and Trapping

10 Mining

13 Oil and Gas Extraction

15 Construction

20 Food & Drug

21 Tobacco

22 Textiles - Apparel

24 Lumber, Wood, Furniture

26 Paper and Allied Products

27 Printing, Publishing, and Allied Industries

28 Chemicals and Allied Products

29 Petroleum Refining and Related Industries

30 Rubber and Plastics Products

31 Leather and Leather Products

32 Stone, Clay, Glass and Concrete Products

33 Primary Metal Industries

34 Fabricated Metal and Machinery Products (except 36)

35 Electrical and Electronic

37 Transportation, other than 40, 44, 45

- 40 Railroad Transportation
- 44 Water Transportation
- 45 Transportation by Air

38 Measuring, Analyzing, and Controlling Instruments; Photographic, Medical, and Optical Goods; Watches and Clocks

39 Miscellaneous Manufacturing Industries

43 U.S. Postal Service

46 Pipe Lines, Except Natural Gas

48 Communication

49 Electric, Gas, and Sanitary Services

50 Wholesale Trade

59 Retail Trade

60 Banking, Credit, Securities, Insurance, Real Estate, other Investments and Loans

89 Services to Individuals of Businesses other than 78, 80, 81, 82

- 78 Amusement Services
- 80 Health Services
- 81 Legal Services
- 82 Educational Services

91 Administration of Social/Governmental Programs, Government Organizations, etc.

than one field, this item is coded "00" - unable to classify. Item 75 on the Bib Card is reserved for a subjective rating by the coder, as follows:

5 Very Important

4 Above average importance

3 Average importance

2 Below average importance

Initially, a rating of 1 was considered for "No importance". However, it was decided that it would be wasteful to store an article that was admittedly of no importance, particularly since computer memory storage was becoming scarce at that time. Therefore, any article that was considered to be of no importance in explaining the process of technological innovation was never entered into the computer data base. Item 76 on the Bib Card allowed for a coder identification number. These numbers were assigned as follows:

0 all faculty

1 Norman

2 Martin

3 Howell

4 Clark

5 Zimmerman

6 Green

7 Cox

8 Seminario

9 Wood

Also, an additional item was added to the [illegible] Card after the coding sheet was printed. A "1" in number 77 would indicate that the article discussed the "linkage" steps or processes between two or more phases of the innovation process, as defined under "Process Phases". (See Exhibit 5)

As might be expected, there was substantial confusion over the definitions (Exhibit 3) early in the program. To help achieve some consistency in this area, a series of cross checks were conducted in which the coders read a few of each other's articles. The disagreements were noted and a meeting was held after each checking session. Because a strong element of judgement is necessary in coding these articles, it was not expected that all disagreements could be completely eliminated. After the third and last check, the average number of disagreements per item was about 12.5, or 20% of all possible items. Note that this figure is somewhat overstated, since a difference in opinion on, say, the Process Phase will result in two disagreements.

ADDITION TO CODING SHEET

Use #77 to indicate literature items that discuss the nature of linkages between two or more process phases. By linkages we mean decision points or bridges between process phases. Do not include here items that just talk about more than one phase but do not discuss how they are coupled or related.

Summary

As stated previously, the major intent of this report is to provide documentation and insight into the coding phase of the NSF project on Technological Innovation. However, it is impossible to adequately provide an insight into the cooperation, enthusiasm, and dedication of the persons involved in this project. Therefore, while it does not truely do them justice, they should at least be recognized herein for a job well done!

Coders:

Mark Clark
L.O. Cox
Reginald Green
David Howell
Mary Martin
Carlos Seminario
Duncan Wood
Russell Zimmerman

Computer Specialist:

Taylor Little

Book Retrieval:

Farah Eslami
Jane Nelson

Secretary:

Lu Ann Sims

N.J. Norman,
Coding Coordinator

G-93-602

A P P E N D I X I I

S A M P L E

C R I T I Q U E S

PRELIMINARY COMMENTS: "THE DIFFUSION PHASE"

These comments are to be read in conjunction with the accompanying marked-up copy of the draft. Further comments are likely to follow, pending a meeting of our "C" Group to consider the draft material as a whole.

The most striking problem with this paper is the lack or inadequacy of organizational principle--the "specialized concerns" of researchers is too vague to serve this purpose. In its present form the paper fails to add anything to the literature; it does not succeed in making sense out of it.

Toward the end the paper suggests the necessity of a multivariate conceptual model of diffusion processes. Some of the dimensions you say the model should cover are type of factor, interface with other phases of the innovation process, and context. You indicate how the Ga. Tech. data bank could be used and the questions that should be asked. All of this seems potentially useful in redrafting the present paper.

The last section actually recommends an assessment be done, when that is the purpose of the present paper. The conclusion, which I take to be the first paragraph of the Introduction, seems overstated, too general, and not adequately justified. Somewhere you must pull together what the literature adds up to. Things that should be considered include the questions listed on pages 12-13 of the Ga. Tech. proposal:

A. What is the state of theoretical understanding?

B. What is the state of empirical knowledge?

C. What is the extent of agreement between empirical studies and theory?

D. Specific gaps or weaknesses.

This could also include discussion of methods and measurements used. Also in respect to the assessment aspect of the paper, the criteria for assessing the importance and relevance of works, and the research needs for the future are not clear. For instance, the research recommendations in the areas of multi-national corporations, the role of organized labor, and transfer of military technology all arise with very little prior discussion in the paper.

Another kind of problem is the lack of clarity as to just what is the scope of the topic to be covered. Some seemingly relevant topics are not covered at all, such as diffusion in the industrial context (which is not covered elsewhere in the papers) and diffusion of education innovations (See Richard O. Carlson "Summary and Critique of Educational Diffusion Research", enclosed.) Other fields, although introduced, are treated very briefly: diffusion of health innovations (See Arnold D. Kaluzny "Innovation in the Health System: A Selective Review of System Characteristics and Empirical Research", enclosed), internation technology transfer (see "The Effects of International Technology Transfers on the U.S. Economy", enclosed), and transfer of military technology. Dave recommends the Carlson and Kaluzny articles as good examples of assessment of a field.

In general there seems to be a confusion or lack of distinction between diffusion and technology transfer, and between diffusion and economic growth or technological change. Once the scope of the paper is decided upon, the appropriateness of specific topics will become more apparent. In my opinion, diffusion in the industrial and educational contexts should definitely be included. Any topic that is introduced should give an indication of the size and content of the literature; for instance, there is a larger literature than indicated in the fields of health and education innovation, transfer of military technology, and international technology transfer.

Another distinction that is not made is between senders and receivers in the diffusion process. Much of the literature focuses on the adopting unit--farmer, doctor, school superintendant, firm. Often the technology must be adapted before it can be used. How prevalent and important is this activity?

One important problem associated with the diverse nature of diffusion research is generalizability of models from one context to another. What attempts have been made (and with what success) to apply frameworks developed in one context to other contexts? I understand that attempts to apply Rogers' model to the industrial context by Rubenstein's group have been unsuccessful.

Aside from a brief discussion in the last section, the relation of diffusion to other phases of innovation and the innovation process as a whole is not covered.

The very important temporal aspect of diffusion does not come through.

STRASSMANN

MISCELLANEOUS COMMENTS ON KRANZBERG, et. al.

p. 13 "the two stimuli...military requirements and economic profit." These are not at the same level of abstraction. Military is specific - violence peraphernalia and protection against it. Economic is general. When people want spears and bombs, production and innovation thereof becomes profitable. Perhaps you could have a large category of cost-reducing innovations for known commodities - pure supply side - and then mention other demand categories. Remember, Mumford paired Venus with Mars. Once given necessities are cheaper, people want toys: Versailles fountains, weird watches, color TV, snowmobiles, purple hair dyes and other aphrodisiacs (Afro-disiacs?), LSD. There's also an insatiable demand for living longer with fewer ailments.

p. 18 I don't think you really want to become involved in dis-tinguishing "human" from "social" elements unless social becomes identical with the mere mechanized symbols in the mind of some pedant whose concepts don't really have much to do with people "out there."

pp. 28 fff. It is amazing that this long historical section cites all kinds of s tale cornballs and omits some of the few people who really had insights into issues such as why China didn't make it. For example, C;E;Ayres, Theory of Economic Progress. Instead Merton, Weber, and Tawney are dusted off. Where's Veblen? The footnote presentation seems designed to disguise what literature has been drawn on. Schumpeter's main statements and their empirical validity are ignored. Even wrong, he is more important than Schmookler. As Voltaire said, to the dead we own not respect, but the truth.

p.59 Even Acontio's patent was for a monopoly. Mentioning who got a patent for this or that seems a waste of space. More space should have gone to Machlup's excellent analysis, which probes deeper than this survey implies anyone has.

p. 1oo I'm s ure Nate Rosenberg will appreciate being mentioned on this page, but why do it with such a banality? Then on p. 1o5, you do it again! Why not stick in Coolidge's, "When many people cannot find work, unemployment results"?

pp.125-6 Unless your piece is to be anonymous, use the personal pronoun "I" ("my") in referring to your own work.

pp. 126-152. Omit all the case s tudies.

p. 162.Don't depend on Landes for insights on Britain: who there "basketry Habakkuk".THIS WHOLE SURVEY WOULD BE CRISPER IF CUT TO 1/3.

Ro ssini's Paper.

Its chop suey effect can't all be the fault of that poor secretary. That awful "goldmine" sentence on p. 13 can't be her fault. Maybe the paper has the bad luck of coming right after Kranzberg's. In fact, it's not chop suey, just a stale rehash.

On to Baker;

Starts off fairly well , but uses phrases like "define a conceptualization" p. 4 I'd rather remain ignorant than learn from such style.

A problem with this and the preceding paper is that the authors are describing something with which they themselves don't have any firsthand familiarity, where they have done no previous research. So they just condense and repeat what a lot of authorities have said in a kind of boring, obeisant way, like a schoolboy's essay when he doesn't know where the teacher stands.

(unfortunately schoolboys in the US are less and less drilled in how to write an essay or anything else --which may be part of the explanation)

p 21 ff.

I like the ideas of the various "Propositions," but the way they are expressed is stilted and obscurantist. Why not make Proposition 6, as an example:

"Superiors welcome clearly relevant ideas the most."

Or does that make the point too obvious? No wonder academic social science is viewed by others with contempt. Does "non-relevant" convey anything that "irrelevant" doesn't?

The final draft should not be written so that the reader begins to wonder whether in the big rubbish pile of words there is indeed a needle of insight.

p.35 Bright's classification of uncertainties is a poor one. Everything is "behavioral." If "competitive actions" are a kind of resistance to change, they should be put together in a general category of "interference risks." "Demand" must be synonymous with "consumer response." There are lots of "timing risks" besides obsolescence which actually is a technical risk - the purely technical features are not good enough given developments going on in the field. Whoever thinks Bright is the best - or up to par - on this doesn't know the literature.

p.40 this is the sort of thing that is helpful: it's more than just an endless A said this and B said that.

On the whole, there seem to be some meaty parts to this section of the study, which should be clearly and forcefully distinguished from those that are mere fraudulent academic wheelspinning. If the writers do not point this out in this review, they become part of the problem instead of helping with the solution, as the saying goes.

P.56. Enumerations are good, but should not all begin with the same word, "variables..." etc.

In general there is too much detail, of too much of an inventory of mediocre, evanescent ideas and non-ideas. Why not survey in a more general way who does this sort of pseudo-research, where, and why?

JULIAN D. TEBO, DR. ENG., P.E.
CONSULTING ENGINEER
30 SUTTON PLACE
VERONA, NEW JERSEY 07044
TELEPHONE 201 239-0125

December 15, 1973

Dr. Melvin Kranzberg
Callaway Professor of the History of Technology
Georgia Institute of Technology
Atlanta, Georgia 30332

Dear Mel:

I have been overwhelmed during the past several days with 5 papers on the various phases of the innovative process, as prepared by your Innovation Project Team. I doubt that I shall be able to meet the requested deadline of December 31, 1973 for my comments on all of these papers, but will try to do as much as I can before that time - and continue with the rest during January 1974. The Christmas season is a poor choice for deadlines, and I expect to be away from home for a part of the season.

Starting with one paper picked at random - "The Diffusion Phase of the Innovation Process", by Morris Mitzner, I was quite impressed with the enormous amount of work he must have done, examining the literature that involved 159 references. Although I am somewhat reticent concerning my qualifications as a commentator on the subject, I am sending herewith some thoughts that occurred to me as I read the paper. I shall list them in the order in which they came to me with the hope that they will be of some use to you and your team:

(1) Pages 1 - 5 Listing the definitions of diffusion seems to result in confusion; it is difficult to remember what"diffusion" means, as I found out later in the author's conclusions, page 83.

(2) Page 9 - I am unfamiliar with the term "Monto Carlo game theory", even though I was in Monte Carlo in 1970. Some explanation as to what it means would be helpful.

(3) Page 14 - Diffusion of technical information by a National Institute of Technology would require companies to give up closely guarded secrets - it is doubtful that such an Institute would be successful.

(4) Page 15 (and earlier) - A good example of the diffusion of of knowledge lies in the many suits (more than 600) that the infant Bell Telephone Company had to bring against infringers of its patents. Thus the "diffusion" of technical information created a time-consuming and expensive problem forced upon the Bell Company. The profitability the telephone business was the great attraction that caused the attempts of others to profit at the expense of the Bell people.

The author seems to assume that an innovation is free to be adopted by anyone. This is not generally the case, when there is a possibility of patent infringement.

(5) Page 17 - I question the statement that "the rate of diffusion of an industrial innovation is inversely related to the size of the investment required to use the innovation". A good example is the innovations following the invention of the transistor; a great amount of

investment was required to provide for the use of the transistor in its variety of applications.

(6) Pages 20 and 21 - The Bell System is unique in its use of innovations, because a new type of switching system, for example, cannot be introduced without keeping in mind it must work compatibly with the existing systems. Gradual phasing out of the older systems make take a long period of years to accomplish.

(7) Pages 24 and 25 - The importance of social and ethnic effects of innovation has always been of paramount importance. The rate of adoption depends upon the education of the people to be affected; they must learn the advantages before complete adoption is made.

(8) Pages 26 and 27 A knowledge of economics is highly important in the introduction of agricultural innovations.

(9) Pages 29 and 30 This is a good explanation of delayed innovative developments by the type of education that was popular. Some worthwhile thoughts on history are presented.

(10) Page 37 The problems of innovations causing obsolescence of skills just means that a continuous program of training in new skills is required.

(11) Page 38 Innovation must be accomplished without introducing uncertainty - anxiety and fear.

(12) Page 41 Organic system of organization practice would seem best for most innovative cases, because a person working on one aspect has a better idea of how his contributions are intended to be used. As an example, in the development of the crossbar swithing system, the apparatus and systems work were under the same supervisory head, and conferences with all concerned expedited the development.

(13) Pages 45 and 46 The problem of work alienation in the shop is well brought out; the dissatisfactions in the automobile industry, for example, have resulted in strikes and in deliberate damage to the product.

(14) Pages 50 - 55 Social networks of physicians used as an example, have a counterpart in Bell Laboratories' relations between scientists and engineers. It is also a factor in creating good feelings between Bell Laboratories technical people and visitors, who frequently remark about how much their visits have helped them.

(15) Page 58 Opinion leaders can have enormous influence in affecting the general public. If a newspaper, TV, or radio commentator has a pleasing manner, he can wield considerable influence on the voting public. Witness the effect of Father Coughlin's radio talks during the Depression years. This can have a beneficial or a damaging effect.

(16) Pages 68 and 69 Evidence is given that higher education and higher economic levels among society produce earlier adoption of innovations. This is generally true for the acceptance of advanced thought in fields other then innovation.

(17) General The author tells what other people think - not what he thinks, which I suppose is what this study is for. Future work, I suppose, will bring out the views of the author.

Some typographical errors are present, but I have ignored them.

This has taken far more space (as well as time) than I had at first intended. But I hope it will be of some use, and is what you have in mind. I'll write a separate letter about my EJC efforts, because I shall not be able to attend the SHOT meeting the latter part of this month.

With best regards,

Julian D. Tebo

Comments on Georgia Tech Working Paper: The R&D Phase

This should be read in conjunction with the marked-up copy of the paper. In making comments I will refer to the general assessment questions:

1) What is the state of the art? or What do we know?

2) How good is what we know?

3) What are the gaps in our knowledge?

4). What specific research should be undertaken to fill the gaps?

1) What do we know? This seems to apply most directly to a) literature selection and coverage, and b) definition of the bounds of the problem area.

In general the literature coverage seems very good. There are a few missing references that perhaps should be looked into:

Richard Rettig	- dissertation - Project Selection
Charles Douds	- dissertation - Parallel Development
H. C. Young	- dissertation - R&D and Marketing
Richard Barth	- dissertation - Organizational Climate
Jack Morton	- various works
Carter and Williams	- Investment in Innovation
Charpie Report	- costs of R&D
National Science Foundation	- data series on R&D expenditures

The other important issue here is the definition of the bounds of the R&D process and thus the literature to be covered. The first pages of the paper discuss the definitions of R&D and the subphases that it is conventionally thought to include: basic research, applied research, development, and engineering. These are not discussed in depth, however, and possible alternative frameworks are not gone into. On page 4 it is said that an alternative conceptual framework, the Rubenstein idea flow model, will be used. The use of this model as opposed to others must be supported, since the model used to guide the assessment will have a large effect on the types of literature that should be covered and gaps in the research that would be discovered.

Another bound implicitly set on the scope of the assessment is the R&D process as it occurs in industrial laboratories. Does R&D occur outside industry? Outside formal organizationa?

2) How good is our knowledge? The issue of concern here is developing criteria for judging how good knowledge is. My general reaction to the paper is that too often the research on which a statement in the literature is based is not evaluated in a meaningful way. Some hypotheses are not supported by references to literature at all. For example the work of Edwin Mansfield and Daniel Hamberg, both of whom are heavily referenced, is in need of more critical assessment. In the development of criteria for assessing project selection models it would be desirable to address the capability of the models in each of the following aspects:

a) descriptive capability
b) predictive capability
c) control or normative capability.

Much of the research referred to, especially in project selection and motivation and performance, seems to have originally been done in other contexts. This raises a question of the generalizability of this research. Why is it felt that it is applicable to the specific issues of R&D? It would be useful here, I think, to classify the literature according to the context to which it originally referred, say, the industrial R&D laboratory, the Federal laboratory, management science group, or university department. This would allow us to see what have been the sources of data in the past. I imagine this could be easily pulled off of the computer and presented in a table. More importantly, some assessment of the applicability of the results from these various context to R&D is needed.

3) What are the gaps in our knowledge?

I have already mentioned that the gaps in knowledge that you uncover will be heavily dependent on the model of the R&D process used. For this reason we need an evaluation of the idea flow model and an indication of the ways in which this affects the overall assessment. You are not consistent in the use of this model throughout the paper. For instance, in discussion of Mansfield one must use his phase model: basic research, applied research, and development. It seems both models (idea flow and phase) have advantages. Combining the two models in some way may raise some interesting questions. This might be possible for you to do.

There are some questions that were not addressed in the assessment. Part of the reason they were overlooked may be the particular model of R&D chosen to structure the assessment.

1) How is risk reduced as a project procedes through the phases of R&D? (I believe Mansfield addresses this in his latest book.)

2) What are the costs of R&D relative to other phases of the innovation process? What are the relative costs of the subphases of R&D? How does this vary from industry to industry?

3) What are the time aspects of R&D?

4) What are characteristics of participants in R&D in addition to motivation and performance?

4) What specific research should be undertaken to fill the gaps?

The research recommendations seem to be unduly weighed in favor of project selection areas. A set of criteria need to be generated for the selection of those areas where research would most add to understanding of the overall innovation process.

General Comments

The section on project selection is written in much more detail than the rest of the paper and can probably be usefully condensed.

If a recent state-of-the-art exists in a field, it should be referenced and concisely summarized. I do not think it is especially useful in this regard to append the entire Mansfield paper.

Policy recommendations should be held to a minimum, since another part of this Program's activity is devoting full-time effort to studying the effects of policy on innovation and various policy alternatives.

In summary, the strength of the paper is in broad literature coverage. There are problems, however, in the imbalance in favor of project selection models, and industrial R&D, as well as omission of areas such as project control. The main weakness is the lack of criteria for making important decisions such as: choosing the model for guiding the assessment, judging how good the support is for statements in the literature, and judging what research needs to be done. The two models of R&D introduced may profit by being carefully compared and contrasted, and if possible, their complementarities shown. Perhaps the research recommendations should be scored against some number of specific criteria.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

November 15, 1973

Professors Melvin Kranzberg
& Patrick Kelly
Department of Social Sciences
Georgia Institute of Technology
Atlanta, Georgia 30332

Dear Mel and Pat:

I have read all of the draft assessment now, except for Tarpley's section on "application" which has not been received. This letter is to communicate to you some of my thoughts regarding what I have seen, what remains to be done, and the midterm progress report which should be coming in soon.

As I mentioned on the phone, I noticed a tendency in the papers not to evaluate the support for statements found in the literature. The assessment should be more than a summary of what you find. This is something that should be given more attention in later drafts.

The length of the papers is the most noticeable characteristic. The final assessment must be of a readable length, so that people will read more than just the executive summary. Of course, Mel knows the limits of Technology and Culture, but we would hope that the total assessment be about 200 pages, but in any case no longer than 300 pages.

Accompanying this letter are staff comments on and a marked-up copy of Norm Baker's paper. These should be read together. You will probably want to look at it before passing it along. I hope to send similar detailed comments on each of the papers.

In addition to refining the assessment as it now stands, it is not too early to begin thinking about how you intend to pull all this, including the papers by the expert consultants, together into an integrated assessment of our knowledge of innovation processes.

It is to be hoped that this project will come up with a way of looking at the innovation literature that makes sense out of it. Now that you have become intimately familiar with the literature, do you feel that organizing the assessment by phase of the innovation process accomplishes this, or have you become aware of other possibly better ways of organizing the assessment? In regard to the phase method of organization, are phase distinctions reasonable and accurate? (Are they operationally meaningful?)

What are some of the problems with organizing the assessment this way? Although much of the literature falls into this kind of mode, it is awkward for handling works that deal with several or all of the phases. How will you handle the interfaces between phases, which some have suggested present the most difficult problems?

It might be interesting to brainstorm some alternative ways of organizing the assessment and investigate the advantages and disadvantages of using them. It might even be possible to use more than one scheme in the assessment--one chapter by phase, one chapter by variable studied, one by research or policy questions such as those raised in our program plans, etc. One concern is that the present arrangement places too much emphasis on the individual phases, although attention is paid to the interrelationships between the phases, simply because the reader sees the name of a phase at the head of each chapter. In any event your assessment report introduction should explain the framework utilized, the rationale for the framework, and other frameworks considered but not used, and why.

The output of the project is to be an integrated assessment, as opposed to a series of partial assessments. The draft assessment you are working on should be the basic foundation of the final report. Whatever the consultant papers have to add to the basic assessment should be incorporated into your paper. The consultant papers should be treated as inputs to the assessment and included in the appendices.

I spoke to Mel about establishing criteria to guide the assessment of the quality of knowledge, of concepts, and of what research needs to be done. The general purpose of this is to assure that the assessment is not arbitrary. It is probably not necessary to establish formal lists of criteria and instructions for applying them. Rather it will probably suffice to make explicit your concerns in evaluating the literature--that is, to discuss what you felt was important in assessing the state of the art and how you approached the task. It is also critical that the assessment of what research needs to be done narrow down to a relatively small number of most immediately important things. (Twenty possible projects for each phase will not do--two to four is more like it.)

I would like to receive a midterm progress report from you sometime in early December, so it will be possible for me to have some of our staff review it and come down to discuss it with you by late December. The report need not be lengthy, about 25 pages plus appendices of the outputs to date. It should cover what you have done up to this point and what you plan to do between now and the end of the project, including a detailed outline of the final output. We are interested in what you have learned about innovation and the state of the art, what your thoughts are on the form of the final assessment, and what you have actually done to date (number of abstracts, etc.) This is also an appropriate place to indicate how much of the funds and time have been spent and how this compares to your original project plan and any problems anticipated in finishing on schedule. The midterm report and attached outputs will be sent to the same reviewers who participated in the external review of your proposal.

The proposal specifies that a midterm report will be made after the Georgia Tech assessment has been critiqued by the expert consultants. It appears, however, that these critiques will not be forthcoming until after the consultants' assessments have been received. Since you are already into the second half of the project we will have to accept a midterm report that does not include these critiques or only some of them. This does not eliminate the need for the critique step, however, and you will be expected to solicit and incorporate consultant critiques into the Georgia Tech assessment, as indicated on page 15 of your proposal.

We also expect that the consultants may want to make some changes in their papers based on your assessment. As you recall, it was desired for the consultants' individual papers to build upon what you had already done.

The midterm report will not take the place of the monthly letter reports which are specified on page 20 of your proposal. I have received only one to date. These documents are important as formal indications of the progress of the project. They should be brief, indicating what has been accomplished to date, what remains to be done, unanticipated problems encountered, and how resources spent to date compare with the original plan. I expect reports for the months August, September, and October by the end of this month. The month of November may be included in the midterm report.

The question of whether it will be useful to have a symposium on your research results in Washington will remain open for a while. The utility of such a symposium rests on the quality of your output, the form which it takes, and the audience(s) of primary interest. There may be alternatives to a symposium. The decision on which way to go will be made after we have a better idea of what the final product will look like and, perhaps, after we see the coverage and response at the AAAS meeting.

By the way, Pat, you asked about submission of a proposal on our FY 1974 Program Plan. A proposal may be submitted at any time and will be considered in competition with others. However, a final decision will not be made until the output of your current project has been evaluated. Dave Roessner is in charge of our public sector activities, so you may want to contact him. A brief statement of research interest is appropriate as the first step.

Sincerely yours,

Mary Ellen Mogee
Office of National R&D Assessment

Kelly

GEORGIA INSTITUTE OF TECHNOLOGY
ATLANTA, GEORGIA 30332

.RTMENT OF
.L SCIENCES

October 26, 1973

Dr. Simon Kuznets
67 Francis Avenue
Cambridge, Massachusetts 02138

Dear Dr. Kuznets:

Please excuse my delay in responding to your letters of September 24 and October 15. I have been out of town on an extended lecture and conference tour, and then I wanted to digest the contents of your letters and also of your preliminary draft on "Technological Innovations and Economic Growth."

First, let me thank you for bothering to read my lengthy paper. As you realize, these rough drafts are meant for internal consumption, and the final versions will undoubtedly be much different. In my case, I deliberately over-wrote in order to include materials which properly belong in the reports of my Georgia Tech colleagues, in the hope that they will eventually use some of my material in their own papers. My final version will be about one-quarter the length of the rough draft.

I am glad that you both approve and find fault with different elements of my paper. (I find some fault with some things myself, and I intend to correct them in later versions.) You are right, we do not intend to arrive at a consensus. The subject is too complex to admit of reconciling our differences, and any attempt to do so would result in mushy generalizations which would be of very little help to future researchers on this topic. If ours is to be a true state-of-the-art study, it must reveal these differences in interpretation, because they are a true reflection of our current state of knowledge.

Not only will there be differences of interpretation between the consultants and the Georgia Tech report, but there will also be differences among our Georgia Tech group and among our consultants.

Some of these differences can be fundamental. For example, Jim Bright quarrels with our definition of application in terms of "first commercial introduction." He thinks that is very narrowly constrictive, and ignores a major aspect of innovative progress. It would exclude study of such "hardware" as jet engines, radar, atomic energy, integrated circuits, the computer, numeric control of machine tools, etc.; it would also exclude "software" such as quality control, interchangeable parts,moving assembly lines, weather forecasting, and operations research. Bright states that this is so because these innovations were either initially supported through R&D by the military or other government agencies, and second, the innovation was first used by the government or under government auspices. In other words, he quarrels with the idea of "commercial introduction" because he believes that "an enormous amount of technical innovation can be traced to government support or experimentation."

Although I agree with Bright that many major innovations were developed under government auspices, I do not think that our study excludes them. While we are more concerned with commercial application, we are interested in the totality of the innovation process -- and that would include the military and governmental inputs. In many cases, further innovation becomes necessary in order to transmute such innovations into the civilian economy -- and we are concerned with both kinds.

The important thing is not that Bright disagrees with our original definition -- we widened it to "application," without any restrictive adjectival modifier, in order to take cognizance of his objection -- but that such disagreement helps to focus attention upon a major issue in any conclusions we might reach. Actually, upon several issues: If governmental R&D (military and otherwise) is so important in innovative progress, how do we transfer these innovations into the civilian economy? What type of governmental R&D is most productive in stimulating innovation? In the absence of or decline of governmental support of R&D, how can we best stimulate civilian R&D and its contribution to innovation? Etc.

Turning to the first section of your preliminary paper, which I have read with great interest (it is now being duplicated for perusal by my colleagues, and their comments), I note that you fall into the same trap as I do, even though you struggle to avoid it. The trap is in the use of such terms as "marked advance," "substantial," "major." It is inevitable that we use such terms, but the fact is that while they sound quantitative, we are hard put to give them any numerical content. They remain semi-qualitative judgments because we don't have any measuring criteria other than commercial "success" (which can perhaps be measured by a balance sheet, but that too is subject to conflicting interpretations, which explains why accountants and tax lawyers make such a good living).

I have been perturbed by this question for many years now. My good friend and former colleague (when we were both at Case Institute), Russ Ackoff, used to argue with me about this problem.

As an operations researcher, he wanted to give a numerical factor to instances of technological innovation -- but we could never agree on a measuring stick, or unit of measurement. I had basically the same argument some half dozen years ago with Bob Lekachman at the Columbia University Seminar on Technology and Social Change; when he proved statistically that the steam engine had had very little effect on British production (just as Bob Fogel used econometrics to downgrade the importance of the railroads in American economic history), I challenged him by pointing out that production figures might not tell the whole story of the revolutionary impact of the steam engine on the way in which people worked, where they worked, and how they worked.

I suppose that such discussions simply prove that economists, operations researchers, and historians tend to look at different aspects of the same subject, which is perhaps as it should be. In any event, I applaud your attempt to distinguish among different kinds of innovation, to define "major" and "minor," and to outline difficulties in evaluating innovations and their impacts. Perhaps you will help me find some satisfactory answer to this question of quantitative-qualitative evaluation of technological change which has been troubling me for so long. To put it in Talmudic terms, you can be my economic Maimonides, offering a guide to this perplexed historian.

Sincerely yours,

Melvin Kranzberg
Callaway Professor of
the History of Technology

MKwl
cc: Patrick Kelly

Mary Ellen Mogee
11/30/73

COMMENTS: Problem Definition - Idea Generation

The greatest shortcoming of the paper is the failure to answer the assessment questions listed on pages 9-10, and to provide supporting evidence for some statements.

Another major problem is in your use of the idea flow model to guide the assessment. The same questions are raised as applied to Norm Baker's paper. Does the use of an explicit model of the process, depicting relationships between activities, allow an objective assessment of the field? What is the basis for selecting one model over others? It is likely that important aspects of the area of knowledge will not be covered by the model. There may be a tendency to preferentially report data that supports the model. One reader thought your paper came over more as a defense of the model than an assessment of the field. He thought the idea flow diagram represented an *a priori* conclusion of what your findings would be. You might consider making the model less explicit and therefore less determinant of the resulting assessment. Use of the heuristic device must be justified.

At one point, "problem definition-idea generation" is equated with creativity. Was this the result of a literature search on the operational meaning of creativity, or how did you arrive at it?

Later (page 16) the phase is defined more as a process by which technological capability and perceived needs or opportunities are synthesized into new products, etc. What is the relation between the two definitions? Are they the same? yes

The paper should cover individuals as well as organizations. Deliberate creativity does not exclude the individual. Individuals are important in innovation occurring within organizations. You should cover what we know about the characteristics of creative individuals, such as inventors and gatekeepers. will do

There seems to be less emphasis on the problem identification than on idea generation. Problem identification brings out the importance of the interfaces between the phases of innovation processes. As you suggest, inputs are made by salesmen, marketing research departments, production departments, etc. Perhaps more in your case than in the others, it is mistaken to limit the concept of the phase to the first phase in a linear series. Al Bean points out that problems are identified and ideas generated throughout the process. Some ideas and problems are alternatives for each other; others are relevant to a particular stage of activity in the progress of an innovation (for instance, ancillary technical problems that must be solved in engineering a pilot plant). Thinking of idea generation-problem definition as an "initial phase" is a result of taking the phase model too seriously. will handle in next draft correct

There are major gaps in the literature coverage of this paper, that reflect weaknesses in the other papers as well. The Georgia Tech group appears to be missing the bodies of literature on marketing, product management, and new venture analysis. Some relevant journals are:

Journal of Marketing Research
Marketing/ Communications
Industrial Marketing
Journal of American Management Association
Academy of Management Journal
Sales Management

Prominent authors include Frederick Webster, and Lawrence and Lorsch. An article pertaining to the "dual role of salesmen" (selling to the customer and persuading the company to tailor new products) is by Henry Pruden in the September 1969 Academy of Management Journal. For an assessment of the leadership literature see the Handbook of Social Psychology, 1970 edition. Dave suggests two works on rate of idea generation in organizations: a theoretical article by J. Q. Wilson in J. Thompson, Approaches to Organizational Design, and a follow-up empirical study by H. Sapolsky in the Journal of Business. A recent article by Hlavacek and Thompson in the Journal of the American Management Association on venture analysis should have a bibliography.

One aspect of the area that is not covered is how variations in context (industry, technology, etc.) affect the definition of problems and the generation of ideas.

You do not cover methodologies or measures used in the study of this area.

The style of the writing, especially in the introduction, tends to be wordy. This must be eliminated in all the papers in view of overall length limitations.

HARVARD UNIVERSITY

DEPARTMENT OF ECONOMICS

CAMBRIDGE, MASSACHUSETTS 02138

November 19, 1973

Professor Melvin Kranzberg
Deparmtment of Social Sciences
Georgia Institute of Technology
Atlanta, Georgia 30332

Dear Professor Kranzberg:

Many thanks for your letter of November 14th, commenting on the first draft of my paper. I am glad to know that you find the general content of the paper acceptable; and hope to profit from your specific comments in the revision ahead.

Two questions in connection with the latter: (1) Do I have to worry about space limitations? In order to deal with some of the points more clearly, I may want to use more space, in moderation. Is that all right, or is the paper too long already? (2) What is the submission date of the final draft?

Enclosed you will find a few comments on your paper. Do not take them too seriously, for some of them represent a different individual slant on the problems--and there would be natural disagreement. If just a few of them are helpful, I shall be content.

I do hope to receive more comments on the first draft of my paper; but will understand if the pressure of other concerns make prompt or even eventual response difficult. Meanwhile, I shall try to go through once more the remaining papers; but the comments will be delayed by a spell of idleness over the Thanksgiving holidays.

With best wishes

yours sincerely

Simon Kuznets

67 Francis Avenue
Cambridge, Mass 02138

Comments on Professor Kranzberg's
Paper, The Ecology of Innovation.

by [illegible]

In view of the prēliminary character of the paper, and lack of certainty as to how far revisions xx might already have been made, I am limiting my comments to the broader observations. They may not be helpful; and if so, should be freely disregarded.

Introductory Section, pp. 1-11

1. Here, and in the followingsx section, it would help to make a sharper distinction than is drawn, between invention and innovation. Thus the mythology etc. noted in the first few pages refers to the populatixx notion of an inventor, or the population notion of what causes an invention. There is a separatex mythology on the entrepreneur or innovator.

2. I have some difficulty with the phases listed on p. 9. Even if we admit the possibility of establishing the problem definition and idea generation, it is a point rather than a phase--unless you specify it further. The first phase suggested is then betwæen problem definition-ixdea generation and invention; the second between invention and R & D etc. Furthemore, R & D is an institutional rather than a functional concept--and may in fact cover invention and problem definition. Presumably it is the development component that is meant here. I am commenting in the light of the xx phase classification used in my paper. And I would not dxx reduce the importance of proper phasing xx because they are interrelated: they still represent important distinct segments in the significant sequence that constitutes the innovation process.

II Theories ... pp. 11-28

3. Some of the theories cited refer to invention; others to innovation; and, of course, all of them are far too simple. I wonder whether some parts of this section could be absorbed elsewhere; and the rest--which refer to obvious oversimplified hypotheses, often popular myths, could be omitted.

III Social Ecology, pp. 28-84

4. This is a long and key section. The sequence followed is fxxx from socio-cultural environment to economic incentives, to non-economic incentives, to institutionalization of socia-economic demand, to diffusion mechanisms. There is a close relation between the socio-cultural environment and institutionalization of socio-economic demand; and I am wondering whether one could start with the economic incentives, go on to the non-economic incentives, and then deal with recognition and

vironment, including the institutionalization of the response .

5. The section on diffusion (pp. 79-94) approaches the problem fromx the staddpoint of mechanisms of transmission of information. But this is only ene aspect of the diffusionp pnocess. Another aspect is the additional learning and gain in productivity that occurs from learning in the course of diffusion--not by transmission of information but by from learning by doing. The statement early in the section (p. 79) that "innovatiin must be viewed as new knowledge", should be qualified by adding that it is incomblete new knowledge.

IV The Technological Badkground, pp. 95-126

6. Here I am somewhat bothered by the discussion of the xix relations between science and technologyxx (this reaction can be easily surmised from discussion afin several parts of my paper). To be sure, many inventions wefe not dependent on antecedent scientific discoveries and many scientific discoveries were made without reliance on technological experience. But from the birth of modern science there has been an increasingly close inter-relation; and in terms of what might be called major invention clusters, the roles of science and technology and the feedback between the two (not dialactic in nature, if by dialectic we mean pa movement by resolution of contraxtimax dictions). In this connection I was much impressed by recent books by D.S,L. Cardwellx (I read only two, Steam Power in the Eighteenth Centzyy, very good on James Watt; and From Watt to Clasisus. The first was published in 1963; the second in 1971; and there is a third recent book, reviewed in a recent issue of Science).

V Cases. pp. 127=152

7. I am not sure how much the cases add to the discussion already presented. Given the necessary brevity of the discussion of each case, it is diffiault to go beyond the already familiar. If there is pressure for conserving space, this section would be my first candidate for omission.

8. With particular reference to the discussion of Watt (pp, 127-136 there are a number of points that might be quarrelled with (particularly in the light of the discussion by Cardwell). The pressure for Watt's engine was far less real than is suggested; he was far more dependent upon antecedent science than the discussion suggests; and the whole approach to the problem was essentially of an experimental scientist rather than of an inventory. The scientific origins and consequences of Watt's steamm engine are far more significant than appears from the discussion.

APPENDIX III

SAMPLE

ABSTRACTS

1. Authors: Baker, Norman R. and James R. Freeland

Title: "Structuring Information Flow to Enhance Innovation"

Journal: Management Science, Vol. 19, No. 1

Date: Sept., 1972

2. Scope (including process phase and context):

The paper discusses information flow and researcher behavior during innovation. A model of a management information system is structured. Critical problems of information search and dissemination are examined.

3. Methodology (procedural approach):

The paper consists of a data survey from several industrial research organizations and analysis of this data.

4. Previous work on which based and assumptions:

Assumption: A properly functioning research activity requires a periodic flow of ideas for researchable problems as a necessary input.

Previous work: Rubenstein, A.H., "Organizational Factors Affecting Research and Development Decision Making in Large Decentralized Companies", Management Science, Vol. 10, No. 4 (July 1964.)

5. Major hypotheses advanced:

The input of high quality ideas is a necessary, but not sufficient, condition for a research activity to function properly.

6. Major Empirical Findings:

(1) Ideas which were not submitted contained a significantly higher proportion of "good" ideas. (2) Necessary conditions for generating an idea: (a) Recognition of an organizational need, problem, or opportunity which is perceived to be relevant to organizational objectives, and (b) Recognition of a means or technique by which to satisfy the need, solve the problem, or capitalize on the opportunity. (3) Technical planning techniques, such as relevance trees, can be a useful source for tracing information flows. (4) Project selection and manpower planning is included in the process and the output from these subsystems specifies the identification and timing, as well as the routing, for the information sources.

7. Major conclusions (and author's recommendations for further research and/or policy considerations):

Management science techniques can integrate description and methodology to structure behaviorally feasible systems of information flow and interation with the innovative process. Additional work is recommended on parameter extimation, value measurement, and information source design.

1. Author: Allen, T. J. and Cohen, S. I.

Title: "Information Flow in Research and Development Laboratories"

Publisher: Administrative Science Quarterly, Vol. 14, p. 12

Date: March, 1969

2. Scope (including process phase and context):

The flow of information in R and D laboratories are vital to their existance. This article studies these flows in two R and D organizations with respect to internal and extermal flows of information.

3. Methodology (procedural approach):

Technical communication patterns in two R and D labs. were examined using modified sociometric techniques. The structure of technical communication networks in the two laboratories results from the interaction of both social relations and work structure.

4. Previous work on which based and assumptions:

Previous studies have show the existence of special routes through which technical information most effectively enters the laboratory and travels within a laboratory. Based on studies of mass communications (Lazarsfeld, Berelson, and Gandet, 1948, Kate and Lazarsfeld, 1955, Katz, 1960, Coleman, Katz, and Menzel, 1966), the existence of a 2-step process was hypothesized, through which the average engineer was connected by an intermadiary to information sources outside of his laboratory.

5. Major hypotheses advanced:

(1) In fluence, of organization sturctures
 a) formal organization (management relationships)
 b) informal organization (friendships and social relationshops)
(2) Technological gate keepers:
 a) will be better acquainted than others in the laboratory with the scientific and technological literature.
 b) will maintain a greater degree of informal contact with members of the scientific and technological community outside of their own laboratory.

6. Major empirical findings:

There is strong relationship in the selection of individuals for socialization and the selections of those for technical discussion. The informal organization is strongly related to the technical discussion network, but is for less influential in determining the flow of ciritical ideas. The relation between formal organization and technical communication is much stronger than was evident with informal organization.

7. Major conclusions (and author's recommendations for further research and/or policy considerations):

Gatekeepers help significantly move patents, published more papers and were first line supervisors. Factors which influence the flow of technical information should be understood and the value of gatekeepers should be recognized by R and D managers.

1. Author: Hitch, Charles

Title: "The Character of Research and Development in a Competitive Economy"

Identifier: Rand Corporation Papers P-1247-1309 (Q180.A1 R37x)

Date: 13 May, 1958

2. Scope (including process phase and context):

This paper discusses the criticism of military R&D management. It compares it to industrial (competitive) R&D management and draws conclusions.

3. Methodology (procedural approach):

The article is mostly speculative and the author uses no references for data or results he has brought into his arguments.

4. Previous work on which based and assumptions:

No previous work was mentioned. No assumptions were stated; the article was written purely from the author's observations.

5. Major hypotheses advanced:

3 each:

(1) the organization of R&D in the competitive economy is wrong
(2) critics of military R&D are wrong
(3) military R&D is different in character from competitive R&D, and call for different management techniques.

6. Major empirical findings:

(a) The quickest way to achieve many research objectives is to try multiple paths; (b) more duplication and waste of effort can be justified for high payoff or high uncertainty projects; (c) the cheaper the multiple paths, the more should be tried; (d) predictions of results of R&D are highly unreliable, research should be carried through; (e) the person qualified to choose a direction of research is the one doing the research; (f) the best incentive for research is competition.

7. Major conclusions (and author's recommendations for further research and/or policy considerations):

The problem in managing governmental R&D is not how to suppress competition, but how to divert it into more productive channels.

The first hypothesis was rejected, the second was considered partially true, along with the third.

1. Author: Edwin Mansfield

Title: "The Speed of Response of Firms to New Techniques"

Journal: Quarterly Jr. of Economics 77:290-311

Date: May 1963

2. Scope (including process phase and context):

The paper examines the factors that influence the diffusion of an innovation and the characteristics of the innovative firm. An examination of the results is made in light of the industries concerned.

3. Methodology (procedural approach):

A specfic model is generated to evaluate the innovation response period. Data were collected regarding fourteen innovations in the bituminous coal, steel, brewing,and railway industries.

4. Previous work on which based and assumptions:

Since Schumpeter asserted that a successful innovation is followed by a host of imitators, much work has been done in this area but little in the specific areas Mansfield examined.

5. Major hypotheses advanced:

Hypotheses are presented regarding the effects of various factors on the lingth of time a firm waits before using a particular technique, fourteen major innovations are examined to discover the firm's rate of response and rate of diffusion, and the extent to which innovative leadership is concentrated in a relatively few firms.

6. Major empirical findings:

The results suggest that the length of time a firm waits to introduce an innovation tends to be inversely related to the firm's size and the possible profitability. There seems to be no concentration of innovative leadership in the industries examined. There was no tendency for the length of time a firm waits to be inversely related to its profitability, its growth rate or its liquidity or directly related to the age of its president or its profit trend.

7. Major conclusions (and author's recommendations for further research and/or policy considerations):

The proposition that the speed at which a firm responds to an innovation directly related to its profitability. With a constant profitability it is more likely that a large firm will use the innovation before a small firm. The assumption that innovative firms consistently are the first to use the innovation was not validated.

1. Author: Joseph Ben-David

Title: "Roles and Innovations in Medicine"

Publisher: American Journal of Sociology, Vol. 65

Date: May, 1960

2. Scope (including process phase and context):

The function of the professional society is examined to see how it affects the innovative potential of individuals.

3. Methodology (procedural approach):

Medical researchers and clinical practitioners in countries where the careers are differentiated are examined to estimate the possibilities of practitioner research and the kind of communication that exists between the two roles. The careers of prominent medical researchers are examined to see how the role differences affected them.

4. Previous work on which based and assumptions:

The references used in this paper include: S. C. Gilfillan, The Sociology of Invention; R. H. Shryock, American Medical Reserach-Past and Present, and A. Flexner, Universities: American, English, German.

5. Major hypotheses advanced:

Specialized research personnel working in autonomous and affluent scientific organizations may be the most efficient method in promoting rapid scientific growth when a good idea is at hand of how to explore a series of well-defined phenomenon.

6. Major empirical findings:

In the long run, the knowledge gained from a limited range of problems may diminish. Increased productivity can follow only by shifting attention to new problems and using techniques to explore them. Professionalism of research does notiin itself decrease the chances of innovations by outsiders. Theorectically it increases the chances since the more differentiated the area, the greater is its likelihood of role hybridization. Closed academic systems such as the German school, tend to lose efficiency since they are reluctant to explore those marginal innovation opportunities in which there is a risk of losing status.

7. Major conclusions (and author's recommendations for further research and/or policy considerations):

None.

1. Author: Mansfield, Edwin

Title: Chapter 4, "Innovation and the Diffusion of New Techniques"

Identifier: in Technological Change, by Edwin Mansfield (W.W. Norton & Company)

Date: 1971

2. Scope (including process phase and context):

Using industrial experiences, the author discusses several topics involving innovation and the diffusion of new techniques.

3. Methodology (procedural approach):

The author uses case studies and other references to back up his discussion.

4. Previous work on which based and assumptions:

The author states that "an invention, when applied for the first time, is called an innovation." With this definition, his discussion begins, supported by several references to case studies and data surveys.

5-6. Major hypotheses advanced/Major empirical findings:

There are 12 sections in this chapter, and each one contains an hypothesis and empirical findings.

Section 2: How long is the lag between invention and innovation? The lag varies from industry to industry. Mechanical innovations appear to require the shortest time interval, with chemical and pharmaceutical innovations next. Electronic innovations took the most time. The interval appears shorter when the inventor attempts to innovate. The lag has been decreasing during the period 1885-1950.

Section 3: What factors should a firm consider in deciding whether or not to innovate? The firm should estimate the expected rate of return, risks involved in innovating.

Section 4: To what extent do new firms, firms in other industries, independent inventors, and universities play a leading role as innovators or as sources of the ideas underlying major innovations? The older industries have difficulty in developing and introducing major innovations because they are fragmented into many small firms, they are committed to present methods and machines, and they spend little on R&D.

Section 6: Once an innovation has been introduced by one firm, how rapidly does its use spread? The diffusion of a new technique is generally a slow process. The rate of imitation varies widely.

Section 7: What determines an innovation's rate of diffusion? Four principal factors seem to govern the rate of diffusion: (1) the economic advantage of the innovation over older methods or products, (2) the uncertainty associated with using the innovation when it first appears, (3) the commitment required to try out the innovation, and (4) the rate of reduction of the initial uncertainty regarding the innovation's performance. As the number of firms in an industry adopting an innovation increases, the probability of its adoption by a nonuser increases. The expected profitability of an innovation is directly related to the probability of its adoption. For equally profitable innovations, the probability of adoption is smaller for innovations requiring relatively large investments. The probability of adoption of an innovation is dependent on the industry in which the innovation is introduced.

Section 8: What are the characteristics of firms that are relatively quick or relatively slow to begin using new techniques? (1) Large firms are quicker on the average to begin using new techniques. (2) The higher the expected return from the new technique, the quicker it would be adopted. (3) There is no close relationship between a firm's rate of growth and the rate at which it adopts a new technique. (4) No close relationship exists between a firm's profit rate and the rate at which it adopts new techniques. (5) There is no evidence that firms with younger management personnel might be expected to adopt a new technique more quickly. (6) The effect of a firm's liquidity on its speed of response was not statistically significant. (7) The effects of a firm's profit trend was not statistically significant.

Section 9: At what rate will a firm, once it has begun to use a new technique, continue to substitute it for older methods? The profitability of an investment opportunity stimulates a firm's speed of response. Small firms were quicker than their larger rivals to substitute the new technique. The age of a firm's equipment and its liquidity also determines its response to implementing new techniques.

Section 10: Characteristics of firms which adopted an innovation more quickly are: they were larger than average, the president had more education and was younger.

Sections 1, 5, 11, and 12 were introductory or case descriptions.

7. Major conclusions (and author's recommendations for further research and/or policy considerations):

The author states that no single case study can represent all the new developments that are shaping the nature and evolution of our economy. The examples he presented should provide insight into the process of technological change.

Assessment of significance and relevance of this item:

This chapter covers many significant factors affecting the diffusion of innovations through an industry.

Major recommendations (concerning this item):

The results as stated should be substantiated by other references and used as part of our project report.

1. Author: Utterback, James M.

Title: "Innovation in Industry and the Diffusion of Technology"

Identifier: Working Paper, Graduate School of Business, Indiana University, Bloomington, Indiana

Date: June, 1973.

2. Scope (including process phase and context):

The paper is a state-of-art survey of the factors which influence the process of innovation in organizations including organizational environment, acceptance in the market, and diffusion of technology.

3. Methodology (procedural approach):

The author conducts an exhaustive search of the relevant literature and attempts to summarize and integrate the numerous empirical results and hypotheses regarding

4. Previous work on which based and assumptions:

Since the author conducts an exhaustive literature search, there is no specific previous work on which the article is based. The focus is on innovation flow, particularly the flow of information relevant to innovation. The author's underlying assumption is that the potential of any firm for technical innovation can be considered as a function of the economic, social and political factors of its environment as well as the state of development of technology and information about relevant technology.

5. Major hypotheses advanced:

The article has a number of hypotheses, including: (1) barriers to flows of people and information between the firm and its environment will limit its knowledge of needs, technologies, and policy incentives; (2) the firm's resources, personnel, communication flows, and decision processes will determine the extent to which the firm's innovation potential is realized; (3) as uncertainty faced by the firm increases, its need for specialization to deal with various facets of its environment will also increase; (4) organizational and spatial bonds affect communication and integration among functions and between phases in the innovation process;

(5) the probability that a given firm will adopt an innovation increases with the proportion of competitors already adopting and with the profitability of adoption, but decreases with increase in the required investment; (6) the firm's ability to absorb the cost of a wrong decision is also an important consideration in adoption of an innovation; and (7) in addition to flow of innovative output, skilled persons and technical information flow from the firm and become involved in the creation of additional innovations and spin-off firms.

6. Major Empirical Findings:

Because of the scope of the paper there are a large number of empirical findings reported including: (1) market factors account for 60-80% of important innovations with the remainder accounted for by response to new technologies; (2) short-term profit and market pressures stimulate innovation; (3) most of the ideas successfully developed and implemented by any firm came from outside the firm including innovations wholly adopted from other firms; (4) the cost of the adopted innovation was about the same as the cost of development of the original innovation; (5) larger firms do not seem to develop a greater proportion of innovations, relative to their market share, than smaller firms; (6) there is a substantial lag, 8-15 years, between generation of technical information and use in an innovation and 1-7 years to bring the idea into first use; (7) basic research does not seem to be a significant direct source of innovation but its role is in continual reinforcement and understanding of the implications of applied work; (8) people of all levels of education have generated successful innovations; (9) personal contacts, education, and experience constitute the primary information sources used in originating ideas for innovations; (10) communications which are important in generating ideas are often by someone other than the idea originator; (11) most of the information used in problem-solving comes from "gatekeepers" within the firm; (12) the cost and difficulty of achieving integration increase as specialization increases; (13) project having slack resources tended to have better technical outomes; (14) diffusion of innovations is a multistep flow starting with a few influential individuals; and (15) "spin-off" firms are a function of the environment both inside and in the area of the "incubating" organization and often start from government contract support.

7. Major conclusions:

Past work in the study of innovation in industry and the diffusion of technology has been of a descriptive and non-cumulative nature. As a result, there are difficulties regarding comparability of results and bias in the samples which form the base for the empirical findings. There is a need for an operational model to account for interfirm and interindustry differences and for the development of common definitions. New methodologies such as Campbell's "quasi-experiments" must be applied and utilized.

8. <u>Assessment of significance and relevance:</u>

This is an excellent summary of a literature directly relevant to the project objectives. It should assist us in obtaining a perspective of this area of concern.

9. <u>Major recommendations</u> (research or policy):

Research opportunities abound in the model building, definitional, and methological area relevant to innovation and diffusion. In addition, there are a number of hypotheses which could be tested empirically. Little literature exists involving adoption decisions by firms.

Policy recommendations include: (1) government-held patents and technical reports are seldom used in a commercially or socially important application other than the specific one addressed; (2) channels of communication within and between firms should be encouraged to stimulate innovation; (3) innovation can be encouraged by increasing payoff or by reducing risk; and (4) strategies or policies which enhance market opportunities will be more successful than technology "push" strategies or policies.

1. Author: Salter, W.E.G.

Title: Productivity and Technical Change

Publisher: The University Press, Cambridge

Date: 1960

2. Scope (including process phase and context):

The book is primarily concerned with productivity within an industry and with factors which influence productivity. The particular focus is on the relationship between productivity and technical change.

3. Methodology (procedural approach):

The author conducts his investigation in two sections: (1) theoretical analyses based on assumptions and mathematical analyses and (2) statistical analysis of data published by both U.S. and U.K. government bureaus.

4. Previous work on which based and assumptions:

The work is based on previous work by economists on the question of productivity. The basic underlying assumptions are three: (1) productivity has taken the central place of discussion and investigation in economics; (2) the major problem of productivity analysis is the absence of a suitable theoretical framework in which to organize knowledge regarding productivity; and (3) continuous disturbance and slow adjustment are essential features of technical change.

5. Major hypotheses advanced:

The following relevant hypotheses are advanced: (1) two main forces shape the flow of new techniques (innovations) -- expanding technical knowledge and changing factor prices; (2) the main influences determining movements over time of best-practice productivity in individual industries are rate of technical advance, opportunities for factor substitution, and changes in relative factor prices; (3) in addition to the availability of new technology, the delay in the use of the new technology also influences the impact of the technology on productivity; (4) the higher the rate of replacement investment, the more rapidly are the new techniques brought into use; (5) the rate of replacement in an industry is a function of relative prices of labor and real investment and of standards of obsolescence; and (6) technical advances are more likely in a growing, versus contracting, industry.

6. Major empirical findings:

The following empirical findings are based on 1924-50 data published in the U.K. and supported by 1923-50 U.S. data: (1) unequal increases in labor productivity (within an industry) have not been accompanied by unequal increases in earnings; (2) industries enjoying rapid rates of technical advance and the realization of economies of scale are able to achieve falling relative prices and high rates of increase of output; and (3) structural changes in an industry are a response to the changing pattern of costs and prices resulting from uneven rates of technical change.

7. Major conclusions (and author's recommendations for further research and/or policy considerations):

The major conclusion is that variation between industries in the extent of increases in labor productivity can be explained primarily by the uneven impact of three influences: (i) improvements in technical knowledge, (ii) potential economies of scale and the extent of their realization, and (iii) factor substitution.

Assessment of significance and relevance of this item:

The book is marginally relevant; however, the conclusion regarding labor productivity in an industry could be significant.

Major recommendations (concerning this item):

Two research suggestions can be drawn: (1) there is a lack of knowledge concerning the rate of technical change, relative factor prices, best-practice productivity and the competitive structure of various industries and (2) there is little knowledge regarding causes of increased labor productivity.

The conclusion relating labor productivity and technical change suggests opportunities for policy.

1. Author: W. Rupert Maclaurin

Title: "The Sequence from Invention to Innovation and its Relation to Economic Growth"

Publisher: Quarterly Journal of Economics, Vol. 67 (1953), pp. 97-111.

Date:

2. Scope (including process phase and context):

Deals with propensity to develop pure science, to invent, to innovate, to finance innovation, and to accept innovation. Context - American industry.

3. Methodology (procedural approach):

Case studies and speculation.

4. Previous work on which based and assumptions:

Based on Schumpeter and followers including Bright, Schoville, Bishop, Vandermoulen. Also cites Roston and Gilfillan.

5. Major hypotheses advanced:

That the process of technological advance may be broken into 5 climates which may be more measurable.

6. Major empirical findings:

There are important variations between regions and cultural groups in propensity to accept innovations.

7. Major conclusions (and author's recommendations for further research and/or policy considerations):

Suggests ways in which the 5 listed propensities might be measured. Suggests a need for technological forecasting and attempt to achieve optimum development rates by deliberate intervention. Believes pur science is more important than Schumpeter perceived. Suggests a need to analysze different innovations over time and in depth.

QUARTERLY PROGRESS REPORT NO. 8

Training Project No. T-900227

"Solid Waste Technology"

April 1, 1974 through June 30, 1974

Georgia Institute of Technology

Atlanta, Georgia 30332

General

This report covers the eighth quarter of the extension period of the project originally commenced on July 1, 1972. It coincides with the spring quarter at Georgia Tech during which the second of two formal courses in solid waste technology was presented and the trainees initiated their special research projects.

Program Description

As detailed in the grant application, the program has been devised to provide opportunity for specialized training in the technology of solid waste characterization, collection, transport and disposal. The effort is accomplished with a flexible core curriculum complemented by adjunct courses, seminars and special problems. Students enrolled in the program have completed the formal courses and engaged in their research projects.

Special topics selected by students participating in the program include:

"Sanitary Landfill Stabilization with Leachate Recycle and Residual Treatment" (Supported also by EPA Grant No. R-801397).

"Stabilization of Compacted and Noncompacted Refuse in Natural and Saline Water Environments" (Supported also by a Whirlpool Corporation Fellowship).

"Biological Treatment of Solid Residues from Hatchery Operations"

"Separate Carbon Adsorption Treatment of Leachate"

"Use of Clays for Attenuation of Leachate Pollutants"

"Anaerobic Stabilization of Solid Wastes from Produce Markets"

"Feasibility and Acceptability of Waste Paper Recycle in Large Metropolitan Areas"

Trainee Participation and Program Accomplishments

The following trainees have participated in the program during this report period:

Student	Degree - Date
Mark C. Boner	MSSE - September 1974
Ralph R. Bouton	MSSE - September 1974
Charles N. Crandall	MSSE - September 1974
Edgardo N. Martinez	MSSE - September 1974
Charles L. Simmons	MSSE - September 1974

Through their courses and special problem research, all trainees participate in solid waste laboratory and field projects. The normal student course load is 15 hours per quarter and most students complete their programs in 12 months.

The program director has been involved in several solid waste oriented activities which, including progress on an EPA supported research project on leachate treatment and recycle and assisting EPA on a position paper, has resulted in a presentation at the ASCE Specialty Conference at Penn State and a scheduled presentation at the International Conference on Water Pollution Research in Paris in September, 1974.

Administrative Actions

The program has continued to provide graduates who are functional in the solid waste management area. However, the phase-out of EPA support will probably adversely effect the productivity unless other avenues and sources of support can be developed. Such sources are presently being sought with some limited success to date. With the previous support of EPA, the program at Georgia Tech has developed a strong base which hopefully serve to advantage in the future.

Frederick Pohland
Project Director