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PRIORITIES FOR RESEARCH APPLICABLE TO NATIONAL NEEDS

Report of the
ad hoc Steering Committee for the
Study of Research Applied to National Needs
of the
Committee on Public Engineering Policy
of the
National Academy of Engineering
to the
National Science Foundation

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National Academy of Engineering
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Honorable H. Guyford Stever
Director
National Science Foundation
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Dear Dr. Stever:

I am pleased to transmit to you the enclosed report entitled "Priorities for Research Applicable to National Needs." This report represents the effort of nearly one hundred experts, drawn from many disciplines and empaneled by the Committee on Public Engineering Policy (COPEP) of the National Academy of Engineering. In April 1972, the Academy was requested by the National Science Foundation to undertake a broad study of national problem-oriented research priorities, particularly as they relate to the Foundation's program of Research Applied to National Needs (RANN). This report constitutes the results of the first phase of such a study in compliance with task order 243 of contract NSF-C310.

As stated by the committee:

We emphasize that the understanding and improvement of institutional arrangements, on the one hand, and technical innovations in natural science and engineering leading to improved techniques and systems, on the other, are equally necessary to resolve the problems of society. Each must be treated in context with the other. In recommending attention to the lag in our understanding of people as well as hardware-oriented problems, we are cognizant that the former are not very tractable and that the capacity for research on institutional problems lags critically both with respect to funding and development of multidisciplinary and applied social science capabilities. (Only a small fraction of the funds available for behavioral and social sciences goes to the type of applied research advocated in this report. The same is true for university, problem-focused, research in the natural sciences and engineering.) Nevertheless, we recommend that RANN should seize the opportunity to lead the way in developing effective means of integrating applied social science, physical science and engineering research. An attempt has been made in developing the research recommendations in each problem area to provide, wherever possible, for a coordinated research balance of applied natural science, engineering and applied social science.

The National Academy of Engineering appreciated the opportunity to undertake this important endeavor which addresses the opportunities and

challenges of applying technology, in its broadest sense, to the solution of problems of society. We have noted on many occasions the need for interdisciplinary approaches to such problems. We were therefore particularly pleased to have had the occasion to call upon persons from many disciplines to work with us in this study.

We hope that you will find this report valuable in the weeks ahead as you pursue the difficult challenge of shaping and supporting research approaches to meet the many and diverse needs of our people and our nation.

Sincerely,

Robert C. Seamans, Jr.
President

National Academy of Engineering
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Committee on Public Engineering Policy

Dr. Robert C. Seamans, Jr.
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Dear Dr. Seamans:

I am transmitting herewith the final report, entited *Priorities for Research Applicable to National Needs* prepared and endorsed by the *ad hoc* Steering Committee of the Committee on Public Engineering Policy at the request of Dr. Alfred J. Eggers, Jr., Assistant Director for Research Applications, of the National Science Foundation. This represents the refined and final version of this report and subsumes the interim document of the same name, which we had the pleasure to transmit in November of 1972.

This report reflects our considered conviction that the Research Applied to National Needs program of the National Science Foundation has a significant contribution to make in providing new insights and opening up new alternatives towards the solution of important problems of society and in meeting national needs. RANN's role is essentially to supplement and to go beyond the scope of the applied research of the established federal government agencies. This, and its limited funds, make particularly important the identification of research areas wherein RANN can achieve greatest leverage. We recommend an adventurous approach in the expectation that some failures will be a small price for the greater successes. While the immediate focus of this study was concerned to provide recommendations as input into RANN's planning for FY 1974, we have yet identified certain themes and new thrusts which suggest changes in emphasis and orientation to enhance both the short and longer-term effectiveness of the RANN program.

Two principal themes contain perhaps the most significant statements in this document because they contemplate for RANN an approach to problems which had not previously been widely pursued. They are entitled "institutional functioning" (the role and capability of a wide variety of society's institutions to meet national needs) and "conservation and patterns of consumption" (emphasizing concern for the scarcity of energy, environ-

mental and material resources). These themes suggest that applied social science research, directed at overcoming institutional, policy and legal barriers, is as critical to the solution of many of our society's problems as is technical research. These recommendations do not detract from the importance of applied physical science and engineering research yet indeed they stress a third principal theme of this report: the need for a coordinated and balanced approach.

Within the broader discussion of these themes, two areas for study are emphasized. One is research concerning the efficiency and, perhaps more important, the effectiveness of our largely public and quasi-public systems for delivering human and governmental services (e.g., health, education, manpower development, welfare, urban transportation, fire and police protection, waste disposal). The other is research directed at the "demand" side of the supply and demand equation to provide a balance, particularly in areas such as energy, where the concentration of research by the mission agencies of government and private industry has essentially focused on the provision and supply of goods and services. Nonetheless, energy *provision* is also highlighted, again in the name of balance, with respect to recommendations relating to further development of solar energy.

In order to respond to the needs of the governmental budgeting and planning cycle for FY 1974, 31 areas of research have been identified and recommended in this report. They cover a broad range of problem areas including, in addition to those mentioned above, environmental quality, industrial and production processes, and natural hazards and disasters including weather modification and earthquake engineering. Further, to assure flexibility, we assigned highest priority to the recommendation that RANN set aside 10 percent of its budget for promising "technological opportunities" and "exploratory development" projects not otherwise within its formal planning.

This report recognizes that in planning its program RANN must confront–within priority choices–most of the complexities, problems and dilemmas of modern man and society. The program must deal with "human" problems which are scarcely tractable and which cannot be resolved with the speed and dramatic success possible in such essentially technical areas as aerospace and military weaponry. COPEP and its steering committee appreciate how difficult the task is for this extremely young agency and although we recommend re-orientation and new directions, this independent evaluation confirms the value of much of what RANN has done.

A study of this magnitude, constrained to be completed within the brief period of the summer and fall months of 1972, could not have been achieved without the rare dedication of the nearly one hundred individuals who participated and the helpful co-operation of the RANN staff, including its

division directors and their deputies and, especially, Louis V. Divone, Program Manager, Office of Systems Integration and Analysis; Joel A. Snow, Deputy Director for Science and Technology, and Alfred J. Eggers, Jr., Director. For this final report we are indebted to Daniel Gottlieb, our editorial consultant who helped us organize and refine our materials. Similar assistance was provided by Howard Margolis with respect to the interim report, upon which this report is based. Meanwhile, the intricate logistics associated with convening the six-panel meetings and the workshop and the subsequent preparation and revision of first the panel reports, the interim report and finally this document, could not have been accomplished without the participation and assistance of COPEP's permanent, policy research, professional staff, augmented substantially for this effort and directed by Micah H. Naftalin.

This, then, is the report of the deliberations, recommendations and judgments of our workshop, treating the substantive priorities in research applicable to national needs for the RANN program of the NSF. A further study has been proposed to examine the difficulties and process of managing and implementing problem-oriented, multidisciplinary applied research and to confront the very unique problems inherent in any program reflecting the areas of research and emphasis recommended in this report. COPEP and its steering committee trust that these, together with the principal themes and new thrusts, will make a significant contribution to the continuing development and strengthening of the RANN program.

Sincerely,

Edward Wenk, Jr.
Chairman

Contents

Abstract

The report represents the first phase of a broad study and review of national problem-oriented research priorities. It was requested from the Committee on Public Engineering Policy of the National Academy of Engineering by the National Science Foundation's program of Research Applied to National Needs (RANN).

The report consists of:

A "Preface" which describes the antecedents of the COPEP study.

Part I: entitled "Common Themes" which represents a discussion of the common themes and principal thrusts emerging from the study. It is emphasized that the understanding and improvement of institutional arrangements, on the one hand, and technical innovations in natural science and engineering leading to improved techniques and systems, on the other, are equally necessary to resolve the problems of society. Each must be treated in context with the other.

Part II:

Chapter 1. "Procedure and Strategy" which describes the origins of the RANN program, the review task given to COPEP, and how this study was conducted.

Chapter 2. A Detailed Description of Recommended Research in which each program is discussed in terms of the problem addressed, the research objectives, specific sample strategies for carrying it out, and its priority rating and estimated funding level for FY 1974.

A Conclusion, reviewing the implications of this study and the unique mission of RANN.

Appendix A, containing a comparison of the current (FY 1973) RANN program to the program priorities recommended herein.

Appendix B, representing a series of analytical categories used to develop research recommendations in the area of Community Development and Human Resources.

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Preface

In mid-April 1972, the Research Applications Directorate of the National Science Foundation requested the Committee on Public Engineering Policy (COPEP) of the National Academy of Engineering to undertake a review and study of national problem-oriented research priorities and their relationship to the Foundation's existing program of Research Applied to National Needs (RANN).*

At the outset, both RANN and COPEP were confronted with two fundamentally distinct requirements in this enterprise. One—the ultimate product—was a thorough-going study of national research goals and priorities (albeit in the context of the applied research mission of the Foundation). The second was an intermediate product—a preliminary set of findings and suggestions for use in developing RANN's FY 1974 program, a process which was to get under way, only a few months later, in the Summer and Fall of 1972. The dilemma, of course, was how to meet the demand for immediate, usable recommendations without compromising standards of excellence against which the ultimate product of a

*In 1970, COPEP had furnished two companion studies to NSF entitled, respectively, *Federal Support of Applied Research* and *Priorities in Applied Research: An Initial Appraisal*, which contributed to the initial organizational development and programming of RANN. The current study can be viewed as a continuation and updating of this earlier COPEP activity.

truly deliberative activity should be judged. Clearly the latter responsibility could not entirely be met in the short time provided for the preparation of the intermediate product. This dilemma was resolved, with some compromise, by adopting a mixed strategy. The work was conceived in a number of phases:

- the convening of a highly compressed series of meetings involving six problem-oriented panels during the Summer of 1972, followed by a steering committee workshop in Port Ludlow, Washington, in September, which developed and selected a recommended research agenda from the panel reports and made an interim report to NSF*
- refinement of the interim report into a final report of the first phase effort
- a second phase study of the problems of managing problem-oriented applied research including such issues as the capacity in the U.S. to perform recommended research, the research manager/performer/user relationship, coordination between Federal research sponsors, etc.
- further studies focusing on the longer-term research objectives of RANN

This, then, is the final report of the Phase I study.

The report is divided into two parts, in order to distinguish the broader concerns and conclusions (Part I) from the description of the procedures of the study and the detailed research recommendations (Part II). Entitled "Common Themes," the discussion in Part I describes certain overarching concerns that were reiterated throughout our many deliberations and represents among the most important aspects of our thinking. The two most significant of these themes, which provide the background for many of the research recommendations, are "institutional functioning," focusing on the role of society's institutions in meeting national needs, and "conservation and patterns of consumption," reflecting concern for the use of environmental, energy and other scarce resources. The first chapter of Part II of this report, entitled "Procedures and Strategy," describes in fuller detail the strategy adopted for completing Phase I. An explanation is merited here, however, of the considerations behind the selection of members of the panels and *ad hoc* steering committee (listed in the preceding pages).

From the start, COPEP recognized that the scope of the study's concerns—potentially the fuller spectrum of society's prob-

**Priorities for Research Applicable to National Needs*: Interim Report, COPEP, November 3, 1972 (unpublished).

lems–required broader representation of disciplines and experiences than are reflected in its makeup (five of the 19 COPEP members are non-engineers). Twelve additional members, chosen for their expertise and diversity of disciplines and experience, lems - required broader representation of disciplines and experiences than are reflected in its make-up (five of the 19 COPEP members are non-engineers). Twelve additional members, chosen for their excellence and diversity of disciplines and experience, were added to COPEP to create the *ad hoc* steering committee. Of these, nine were among the members of the RANN Advisory Committee. In addition, three others were added–one of whom had served on COPEP's subcommittee on technological goals which had begun inquiring into related issues at the time of the request for this study.

In the selection of the panels, which ranged from nine to sixteen members each, expertise was sought in the problem areas addressed. Wherever possible, a balance was sought among institutional affiliations, academic, industry, state and local government and other "user" groups, as well as by age groups. From late July through early September, a total of 69 experts participated in six separate five-day, intensive panel sessions.*

The workshop met at Port Ludlow throughout the week of September 11, 1972, and again on October 22, 1972. Panel chairmen reported on the principal recommendations of their panels. From a rearranged and consolidated grouping of approximately 130 panel recommendations, the workshop selected 31 research programs (see Procedures and Strategy for the selection criteria process). The research recommendations themselves are presented in detail in Chapter 2 of Part II.

Thus, despite the time limitations, this report represents the broadly interdisciplinary efforts of nearly one hundred experts as well as the contributions of the staffs of COPEP and RANN.

*The six panels were entitled: Human Resources, Community Development, Growth and Environment, Energy, Natural Hazards and Disasters, and Targets of Opportunity.

PART I

Common Themes

PREAMBLE

In the current* fiscal year, RANN will fund approximately $70 million in support of research intended to contribute to the solution of important problems of our society. These sums constitute approximately 2 to 3 percent of all Federal, civilian (non-defense, non-space, non-nuclear weaponry) R&D expenditures. In the view of COPEP and its steering committee, these facts alone have major implications for RANN's research funding strategies.

The crucial question, then, is how to use these limited resources. If RANN's activities result in no more than a marginal contribution to the solution of these problems, then most people who have been associated with the establishing, managing, and monitoring of the program will be greatly disappointed. The justification for RANN's existence is the capability to undertake research of high leverage—programs that will yield the maximum results from relatively low expenditures. RANN must develop information, methods of problem-solving and technology which can be used by decision-makers in other Federal agencies, state and local government and in the private sector to confront national problems.

At first glance, RANN seems to be faced with an impossible task. One might expect that the many established research

*As this study was performed in the last half of 1972, references to "current fiscal year" should be understood to mean FY 1973.

agencies of the Federal government would have long since seized the most fruitful opportunities for research, thereby making it difficult or impossible for RANN even to make an impact proportional to its funding. Our studies have convinced us otherwise. For reasons of mission orientation, history, or whatever, the aggregate of current Federal research programs leaves gaps in its breadth of coverage in a number of areas where relatively small efforts, on the scale of RANN's capabilities, can have significant leverage in providing new insights or opening up new alternatives towards the solution of important problems.

Although this study involved many varied individuals and experts, and although discussions took place in a series of independent and distinct meetings of different groups (e.g., the workshop and six problem-oriented panels), certain common themes, unpremeditated and unanticipated, recurred throughout the deliberations. The repeated emphasis on these themes by experts drawn from many differing disciplines, professions, and problem-area interests seems to underline their telling nature. They represent perhaps the most significant conclusions of this study, provide the background for understanding the orientation of many of the actual research recommendations and are, in effect, guidance for a redirection of emphasis within the RANN program. We have entitled the two most significant of the themes as "institutional functioning" and "conservation and patterns of consumption."

Yet even before considering these and the other important themes in detail, it is necessary to understand the background philosophy that shaped the charge to the participants in this study. First, broad national goals and priorities, however imprecisely defined, were accepted as the basic starting point of this study, although the constraints of time led to an early decision to rely on intuitive understanding of these goals and priorities rather than to undertake an "in depth" analysis. Secondly, the steering committee recognized that in order for applied research to be effective in meeting national needs, the complex process of the undertaking and application of research must be appreciated to understand towards what ends research can be directed. Accordingly, the notion of a *technological delivery system* was employed to represent the complex processes by which knowledge

in natural and social sciences is deliberately applied to achieve desired outputs of consumer amenities and social values: More than hardware is involved in that the system requires an ensemble of practices and institutions which blends inputs of technical information, capital, natural resources and manpower with inputs of our society's value preferences. The institutional components of such a system include universities, profit and nonprofit research organizations, political components that serve to interpret value preferences through incentives or regulation, public delivery systems at the Federal, state and local levels, and a wide spectrum of private industry components from banking and production to the service sector.

While the interrelationships of these system components are represented by nominal legal, economic, social, political and institutional processes, the structure and conduct of the institutions are animated by and differentiated for each special output desired. This concept provided for the possible evaluation of performance as to the effectiveness of technological delivery systems, gaps in knowledge deserving of research, and the assessment of unintended outputs, or externalities, that may require amelioration if in conflict with other social goals.

With this understanding, the experts, gathered together by COPEP, attempted to evaluate where applied research could most effectively contribute to the meeting of broadly conceived national needs. Society is understood to be, in effect, a dynamic and changing system and also a holistic system in which there are complex inter-relationships and trade-offs, where few problems have a simple solution and effective change is seldom achieved except through the combined effects of a series of independent forces. The recurring themes that dominated so many of the discussions reflect a broad concern that the possibility of researching and finding alternative ways to direct certain of these forces has not been fully realized. The themes are delineated below.

INSTITUTIONAL FUNCTIONING

Perhaps more than any other theme, "institutional functioning" dominated much of the discussion behind the recommendations.

Broadly speaking, the theme concerns the functioning and role of a wide variety of public and private institutions which make policy, directly or indirectly, and implement it. However, such analysis is not intended to imply that RANN should support purely abstract institutional and policy related matters. Indeed, the actual program recommendations reflect a continuing concern for balance and coordination in the research of technical innovation and institutional questions. RANN is rather asked to recognize that previously this important and significant aspect of research has frequently been overlooked and that RANN should, therefore, direct the emphasis of its work to encompass such research and to give due concern to the flaws and failures in institutional functioning that impede the solution of national problems. The theme refers not only to the bureaucracy and the problems of state and local government but to private, nonprofit and profit organizations, particularly in their relationship to delivery systems, and especially in community issues, to the performance of political institutions, legislatures and citizens' groups.

Discussion of institutional functioning, when it arose during our deliberations, arose not as part of discussion of a broad theme, but in the context of discrete problem areas. The research proposed here on institutional functioning is generally specific to the problem areas considered and is therefore limited. However, the pervasiveness of this theme in one problem context after another is a signal that our society may be suffering from problems of institutional performance across the board. It is vital, therefore, for RANN to view the problem in this broader context as well and not be constrained by the forms in which it emerged from the concerns of the six expert panels.* In other words, there is a need

*The picture that emerges must perforce involve some omissions and distortion of perspective relative to that which would come out of a deliberate across-the-board view. An example of such distorted perspective is the view of business institutions. These were not considered in detail and, as a consequence, two simplified images of the business firm appear in the panel reports and in the discussions of the workshop. One image is that of a single rational decision-maker, a decision node in a macroeconomic system. The other is that of a sociotechnical *production* system.

Both images are appropriate for the specific problems which were addressed. Yet neither is an adequate depiction of a business as a socioeconomic technical system. The business firm, seen in its social context, is under marked stress to assume social

for a sharper understanding than we now have of the character and determinants of organizational activity–from the way bureaucracies behave, to the way that advisory committees and citizens' groups behave: What kind of information can organizations assimilate? What kinds of tasks can they perform? What can realistically be expected? How is the public, as well as special interests, represented? These questions emphasize the need for research. It is clear that the gap between the hopes of paper proposals and the discovery of hard facts for practical operations is, on the whole, larger for social systems than it is for hardware systems.

Institutional functioning, more precisely, involves the interaction of a number of processes:

- identification of needs and options
- negotiation and balancing of preferences
- decision making and determination of objectives
- policy implementation
- evaluation of results.

It is in the latter processes–implementation and evaluation–that the broadest concerns were expressed in formulating proposals aimed at achieving a better understanding of *delivery systems.* Certain delivery systems seem, to many people, to have broken down or fallen far short of expectations in terms of intended

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responsibilities which may well affect its behavior as the dominant deliverer of goods and services to society. While broad research into local government has been recommended, as has research on business related topics such as productivity and innovation, very little has yet been proposed by the steering committee on the evolving social and societal role of business although this, in the long run, may well make concerns over innovation, decision making, productivity *per se*, and so on, appear relatively less important than they do at present.

Very little, also, is said about "by-pass" or alternative institutions as a means of overcoming institutional barriers though there are some suggestions under "improving local services" for studies examining the transfer of traditionally government administered services to private profit making organizations and vice versa. Further study of the whole theme might, however, open up a number of useful research opportunities confronting the possibilities of role sharing, and the development of alternative institutions and organizations to serve the public more efficiently and effectively.

results, benefit-cost ratios, and quality of services. The failures appear to have reached a critical level in human service areas such as the judicial process, health care and education. But the delivery system in material areas, such as energy, may be becoming just as critical.

The apparent reasons for the breakdowns or impending breakdowns in delivery systems vary, according to the nature of the institution involved. In general they fail either from:

- faulty definition of objectives and expectations
- insufficiency of resource allocation to make the delivery system work
- lack of incentives to implement policy or misguided application of policy
- inadequacy of organizational arrangements or lack of management ability
- failure to make use of existing technology or to develop new technology to cut costs or improve quality
- lack of consideration paid to indirect policy impacts
- division of responsibility for problems
- attempts to force change within too short a time.

The first reason is frequently cited by program managers but the implication of the research suggested here is that the delivery system requires scrutiny from the point of view of the other stated reasons.

In this area, the pertinent recommendations include those to *improve the measurement and evaluation of the outputs* of delivery systems, implying *accountability* to the clients the systems serve and *incentives* for improved performance.

Data bases and social indicators A vital part of any delivery system is its data base, from which problems may be identified and diagnosed, decisions made, and operations conducted. In virtually all the problem areas considered, there is a need for improved data bases. A special type of data to identify what society wants, trend social statistics called "social indicators," is recognized as relevant and necessary to areas of social concern including but not limited to the problem areas treated in this

study. There is also a proposal for "organizational indicators" which would track organizational changes.

Evaluation and accountability In some respects evaluation can be seen as external to a delivery system. It is a source of feedback on the system's performance both to decision-makers in the system and to persons outside that system who for varying reasons want to evaluate that system and its performance, especially its potential or real impact on them. Evaluation has a dual status in the recommendations. On the one hand there is a recognition of the desirability of an increase in our technical ability to evaluate programs and system performance. Many local governments, for example, lack the data and the technique to be able to merge information about how their services are operating with social data about the population being affected. On the other hand there is a realization that most delivery systems in the public (and private nonprofit) domain need a special type of evaluation, which recognizes their particular political and legal constraints. Though sophisticated tools for measuring cost are available, no equivalent sophisticated indicators have been developed to evaluate performance, especially in the public sectors. In the absence of reaction from the market place such systems can proceed with a very long time lag between malperformance and correction, if any. Frequently, the feedbacks, when they do occur, are greatly distorted.

There are a number of proposals for the design of mechanisms for evaluating delivery systems. A major function of such an evaluation system would be to make the system accountable to the clients as well as the management, whom it serves.

The initial design of a program is a vital determinant of performance and output. Yet theory alone seldom provides a sufficient and satisfactory guide.

Field experimentation With an increasing appreciation of the complexity of social processes and social problems there has come an attendant appreciation of the difficulty of designing social policies and programs with confidence as to the foreseeable consequences. As a result, it becomes apparent that it would be desirable to try out many such policies and programs on an

experimental small scale basis before attempting full scale implementation. With this in view, it was proposed that we increase our willingness and capability to design and conduct such experiments.

Apart from measuring outputs, improving the data base and social indicators, and developing techniques for appropriate utilization and substantial improvement of the technology presently available, the other areas of major concern in the discussions of institutional functioning were research on:

Decision-making It is recognized that, in many problem areas, decision making, both with respect to broad policies and day to day operating decisions, needs improvement. Some of the proposals focus on understanding existing decision institutions and processes. Others are for the introduction of improved formal methods of decision making via such techniques of management science as have had certain successes in business and to a lesser extent in public and other nonprofit settings. Experience has shown that the two approaches are, or ought to be, complementary in that the introduction of "management science" into institutional contexts whose existing processes are not well understood can produce results ranging from useless to disastrous. Related to this also are the problems of institutional malfunctioning that arise from divisions of interest and responsibility between institutional bodies and between different levels of government, Federal, state and local, so that decisions made by one group may not penetrate to another. The absence of couplings and transition mechanisms which would enable separate and diverse institutions to coexist and to reach agreed-upon ends via different yet compatible routes may lead to suboptimal decision making, e.g., they restrict the ability to evaluate the trade-off between cable TV and transportation or mass transit and highways. In other words there are general problems that could be resolved through decision making based on a more holistic view.

Issues of decision making in the environmental area are approached by proposals for problem-oriented modeling and simulation based more on empirical data and empirically established relationships than has previously been the case. For example, it should be possible to develop such policy-relevant

models for regional environmental systems if greater attention is put on data gathering and correlation to provide a sound foundation for such models. Such work, if successful, can give greater insight into the likely consequences of alternative management policies (e.g., costs and benefits, winners and losers), especially since one of the characteristics of existing regional environmental models, and of models in general, is that while they are methodologically sophisticated, they are weak on estimates of the relevant parameters and of the coefficients of interaction of the parameters, because of a weak data base.

*Policy research** Here again there are two sorts of recommendations. One is for substantive policy studies, whether they be a search for better policies, an examination of an existing policy, or both in combination. In addition, there are proposals for studying the processes whereby policies are formed in various areas.

Indirect policy impacts There is recognized a need for better understanding of the ways in which governmental regulation and policies affect national problem areas both directly and indirectly. There is, for example, a need for better understanding of the full implications of the formal regulatory mechanisms which affect the demand for and supply of energy. How, for instance, do the policies of one agency unwittingly impinge on the problems of another agency? More specifically, such matters as land use, population distribution and materials usage are affected by governmental policies which ordinarily may not be directly associated with the issue in question. Thus the regulation, by the Interstate Commerce Commission, of freight rates for scrap and raw materials affect the pattern of materials usage, exercising a strong bias against recycling. There is enough awareness of such anomalies to make this a promising topic to explore.

*Neither common usage nor published literature is entirely consistent in handling the terms "decision process" and "policy process." The distinction we are drawing is between decision making—the mental activity of one man, perhaps with the aid of some prosthetic device such as a computer—and policy process which involves many people and many institutions in a very complex decision process. Policy process includes much of the mental activity of decision making but also group processes such as bargaining and the use of power.

One word of caution is, perhaps, necessary following this discussion. These proposals on institutional functioning and on regulatory processes and policies, while they suggest that many changes may be required to improve the effectiveness of both public and private institutions, should not, however, be read as an attempt to increase regulation, only to improve it. Proposals for research on institutional functioning are not intended to promote wider Federal control over those institutions but to provide better understanding and information to those presently responsible for those institutions. Such research as is proposed may, of course, suggest more control in some instances and less in others. But it is not designed either to increase or decrease controls, but to increase the wisdom of the decisions and actions which are taken.

CONSERVATION AND PATTERNS OF CONSUMPTION

This second, major theme emphasizes concern for the scarcity of environmental resources, available energy and materials, and focuses on the need for research into the "demand" side of issues, an aspect inadequately treated by many of the traditional agencies or private industry despite its potential significance. The theme forms the background for the highest ranked of the substantive areas of research. The proposals in this report seek particularly to provide more adventurous and imaginative research, to begin systematic analysis of problem areas and identify approaches presently neglected or underexploited which are not being undertaken elsewhere. In discussing conservation, or virtually any other problem in this area, the recommendations focus on two interrelated strategies: (a) achieving better management of existing resources, and (b) developing new conservation technology.

The former would include, for instance, development of alternative strategies and institutions for the management of resources, study of present pricing policies and alternatives such as marginal cost pricing, the development of methodologies for internalization of environmental and social costs, and analysis of opportunities for alternative regulation, e.g., the study of the life cycle of materials to locate promising points of intervention either for purposes of preventing pollution or conserving supply. Under

the latter category would fall specific technical research on such possibilities as design of less energy-intensive buildings, development of techniques to utilize solar energy and waste heat from power plants, and cheaper exploitation of underground space through better excavation and tunneling technology.

This theme reflects the need to balance and coordinate applied social science research and applied physical science and engineering research. For example, both "institutional" incentives and advances in technical design and engineering are necessary to achieve buildings, transportation services and urban complexes that minimize energy consumption and deterioration of the environment. Institutional arrangement and technical capabilities must complement each other.

The previous discussion of "institutional functioning" explains the rationale behind many of the research recommendations in this area. The factors affecting decisions by private industry, the formulation of regulatory policy and the complex relationship between Federal, state and local authorities on such matters as land use are examples of institutional questions which have a vital bearing on conservation. Meanwhile, much of the specific technical research is unlikely, in the near future, to be undertaken by private industry, in part for lack of incentives, but also because of cost and sometimes simply because of the nature of the industry, e.g., the construction industry, characterized by small and under-capitalized builders, does virtually no research in the area of less energy-intensive building design, and probably lacks the capability to do so.

Many broad areas of research are involved. Further examples of wide-ranging issues which affect patterns of consumption of fuel and other resources are urban growth, and transportation and communication planning and management.

THE QUESTION OF COORDINATION AND BALANCE

The question of coordination and balance arises in two different contexts—in determining on which perspective of a problem research should concentrate, and in selecting the mix of applied

social science and applied engineering and physical science research to be supported.

In any area of vital national interest in which technology is an important component, the nation's research effort must be *balanced and coordinated.* A number of relevant perspectives should be brought to bear on the problem, each considered in the context of the other. It is not essential, nor is it always desirable, for each agency to strive for this coordination and balance, but the aggregate of such research should have this property. RANN can make an important contribution toward achieving both balance and coordination. The field of energy provides a useful example to illustrate the point:

The national research effort is almost exclusively devoted to the technology of energy *provision.* RANN could and should provide balance by increasing its support for research on questions of energy demand and patterns of consumption, and should coordinate such studies with our understanding of the problems of supply. Such "balancing," however, should not lead to neglect of important areas of research. For example, while the efforts of many agencies are concentrated on energy provision, some vitally important research areas on that side of the energy equation are not being fully pursued. This is particularly true in the area of solar energy research and development. RANN should help redress such neglect.

Balance and coordination are also significant issues in determining the mix of applied social science and applied engineering and physical science research to be supported in pursuit of solutions to national problems. For example, in applied research, where technological advance is the necessary factor to achieve improvement, implementation of that technology may be impeded by institutional malfunctionings. Coordinated research programs designed to identify and treat the interrelationships between the different factors in the system then become necessary.

The use of natural science and engineering research in the social context has created as well as helped to solve the nation's problems. The techniques of such research often are divorced from the existing social and institutional setting. The fact that there are misunderstandings between natural and social scientists and between all such categories with politically-oriented decision

makers is only too well known. What we do not know are the answers to such questions as how to locate the more efficient and politically feasible entry points for natural science and engineering solutions and how to mount a combined research approach which spans the applicable disciplines.

If the recommendations of this report appear to stress institutional barriers and applied social science, it is because these areas are frequently neglected or research is poorly performed in comparison to the study of technological barriers involving applied natural science and engineering research which are better established.

We emphasize that the understanding and improvement of institutional arrangements, on the one hand, and technical innovations in natural science and engineering leading to improved techniques and systems, on the other, are *equally* necessary to resolve the problems of society. Each must be treated in context with the other. In recommending attention to the lag in our understanding of people as well as hardware-oriented problems, we are cognizant that the former are not very tractable and that the capacity for research on institutional problems lags critically both with respect to funding and development of multidisciplinary and applied social science capabilities.*

Nevertheless, we recommend that RANN should seize the opportunity to lead the way in developing effective means of integrating applied social science, physical science and engineering research. An attempt has been made in developing the research recommendations in each problem area to provide, wherever possible, for a coordinated research balance of applied natural science, engineering and applied social science. One cannot, for instance, discuss improving the quality of the environment without considering the need for better technical tools and better institutional arrangements. In the former category would be improved techniques for tracing contaminants. The fact that air basins and bodies of water cut across local, state and sometimes

*Only a small fraction of the funds available for behavioral and social sciences goes to the type of applied research advocated in this report. The same is true for university, problem-focused research in the natural sciences and engineering.

national boundaries suggests the need for jurisdictional authority to manage these resources properly, an institutional element which clearly has been lacking in most parts of this country and the world. As another example, the design of energy-conserving residential buildings requires both engineering and architectural expertise, and also political, economic and legal skills to draft suitable building codes and to estimate cost and social acceptability.

Finally, the workshop included in its highest priority category two recommendations that roughly 10 percent of RANN's total effort should be withheld from preliminary allocation to any of the specific problem-oriented program categories. This funding (recommendations 28 and 29), entitled "technological opportunities" and "exploratory development," should be reserved (5 percent each) for promising projects not encompassed within RANN's formal planning. Allocations of these funds should be independent of (although, of course, not necessarily inconsistent with) whatever formal criteria RANN adopts for project evaluation. Discussion of these two recommendations will be found in chapter 2 of Part II.

USER ORIENTATION

RANN's central task is to develop information, methods of problem solving and technology which can be used by decision makers in other Federal agencies, state and local government, and the private sector in assessing the merits of follow-on programs involving substantially larger investments. It is one of the few research sponsors which is not also the direct user of results. This means that RANN must support research along the R&D spectrum to the point of proving a concept, but normally not beyond into demonstration or ultimate implementation. Therefore, unlike an operating agency, RANN, to be most effective, should not hold on to its successful projects.

Certain subsidiary strategies inevitably flow from this premise. If RANN is to view its "users"—state and local government and the private sector, including profit making institutions—as partners in applying its research results, and if it seeks thereby to influence

industrial and state and local perceptions, goals and practices, then ways must be found to draw these elements expressly, purposefully and effectively into the performance of research on national needs. At the same time, it should improve and preserve the special relationship and capabilities of its current research performers, principally the universities, government labs and nonprofit research institutions. Similarly, if it is to view its sister Federal agencies as partners and future implementers, it must not only work toward a high level of research cooperation, but coordinating mechanisms (e.g., the former Federal Council on Science and Technology) and interagency relationships must be substantially strengthened to ensure successful collaboration.*

THE QUESTION OF INTERNATIONAL ASPECTS

The steering committee was also aware of the international implications of RANN's role. Too often international cooperation is thought of exclusively in terms of managing problems whose elements lie outside national boundaries such as economic aid, space and oceans. Vital as such cooperation is to the well being of individual states, it is to the underdeveloped areas of mutual exploration of common problems within national borders that some of the recommendations of this report address themselves. Thinking in such fields as fuels and the development of pollution standards which does not comprehend the relationship between oil in Alaska and gas in Algeria and the economic consequences for trade of uncoordinated pollution standards for internal combustion engines is doomed to what has been termed the "tyranny of small decisions."

The recommendations of this report are not always explicit on the international aspects of specific problem areas. But the Steering Committee feels that Research Applied to *National* Needs should not be viewed as a program which stops at the U.S.

*To accomplish these goals will require much study by RANN and further attention by the COPEP steering committee in later phases of this study. Moreover, it will require, in part, RANN's determination to review critically its present patterns of activity in each of the above respects.

shoreline. Moreover, the "needs" are national but both the achievement of solutions and the repercussions of decisions may require a broader look. RANN should be open to consideration of investigative proposals on what other governments are doing about their national problems with a view to possible adaptation of successful technologies and institutional processes in this country.

THE RELATIONSHIP OF THEMES TO SPECIFIC RESEARCH RECOMMENDATIONS

Although discussed and emphasized because of their significance, the above themes do not reflect the entire nature or even the overall shape of the research recommendations put forward through this study. Firstly, the research recommendations are in no way intended to make up a program; rather it is these themes which are described here to suggest the program orientation toward which RANN should move. Secondly, these themes represent a longer-term approach to the issues of applied research priorities, while the research recommendations were designed primarily to provide immediate input to the planning process of RANN's FY '74 budget cycle. Thirdly, further criteria are necessarily applied in the actual selection of recommended research reflecting RANN's particular role to fill gaps in the work of the traditional, mission-oriented agencies and to deal with issues too broad for their scope, and to reserve itself largely for efforts in which it can apply significant leverage.

LEVERAGE AND RISK TAKING

The issue of "leverage" is an important one. With its limited funds, RANN must be selective in order to make the most effective use of its resources. Small efforts, on the scale of RANN's capabilities, can have enormous leverage in providing new insights or opening up new alternatives toward the solution of important problems of society. To this end, and because of its distinct freedoms, RANN must be willing and even eager to take some risks in its choice of programs, projects and investigations.

Also many of society's most perplexing and stubborn problems have long failed to yield to the conventional wisdom of the day. In cultivating a taste for risk-taking, RANN must seek out the responsibly unconventional. In this spirit, we are struck by the suggestion of one panel that a small portion of RANN's budget should be reserved for the proposal which, receiving adverse grades by most of the peer reviewers, nonetheless receives several especially high grades. We recommend, therefore, a spirit of adventure, in the reasonable hope that a higher failure rate can be more than offset by a number of outstanding successes.

Support of the RANN program and other similar programs is in no way a guarantee that the important problems of society will be or can be solved. These problems are far too complex and frequently too "human" for research alone to achieve a resolution. Meanwhile the state of the art in many areas of the applied social sciences and applied engineering and physical sciences requires extensive work before the full skills of research can be brought to respond to the needs of society. Yet even while we achieve a greater understanding of society and its needs, "people" problems, unlike those in more narrowly technical fields, will frequently remain intractable, and while the progress from an orbiting satellite to a space lab may take only a decade, human society is unlikely to see such dramatic and speedy development within its own complex and delicate structure.

The research anticipated for RANN is, however, recommended in the belief that research can contribute to the meeting of social needs. RANN's mission is not aimed at seeking knowledge primarily for its intrinsic value and for the edification of the researcher and his peer, but rather to lead to the prompt utilization of the results by a client or consumer who can and does put them to use to solve or ameliorate problems. To this end, RANN's task is to support research applicable to the resolution of national needs, the amelioration of national problems, and, in part, to support the more diffuse purpose of seeking information, insights and new perspectives as alternatives which will provide users, problem solvers and other researchers with new ways of looking at their problems as distinguished from answers or solutions *per se*.

PART II

Chapter 1 Procedure and Strategy

Applied research only became a major activity of the Federal government during World War II and the postwar years. Born in the environment of war, it concentrated almost exclusively on fashioning newer and more efficient weapons and counter-weapons systems. In the cold war period, Federal applied research received a new impulse from the orbiting of the first artificial satellite—Sputnik I. To the large defense research program was added a highly intensive, richly funded effort to put man in space and on the moon. Both the defense and space applied research programs drew heavily on basic research already accomplished or on-going.

Beginning in the 60's and accelerating in the 70's, the lessening of international tensions gradually focused attention once again on national domestic needs. Only then did the nation as a whole become fully conscious of what had been happening to it in terms of urbanization, pollution of the environment, depletion of raw materials, usable land and energy resources, and obsolescence of its institutions.

ORIGINS OF RANN

It was not until 1968—a peak year of domestic violence and crises—that the first centrally-funded, broad-ranging Federal re-

search effort on national problems was launched. A number of mission-oriented Federal agencies, however, were already performing applied research on certain critical problems: *AEC* on nuclear energy, *Transportation* on mass transit systems, *HUD* on modular housing, etc. Yet Congress felt the need for a unified program to serve the broad perspective of national purposes and goals which spanned mission-agency lines and covered the territory between them. The agency it chose for this program was the National Science Foundation. Although the Foundation previously had been restricted to basic or fundamental research, the rationale for this limitation, contained in the National Science Foundation Act of 1950 creating NSF, was that applied research relevant to Federal activities would be supported by mission-oriented agencies.

The 1968 amendments to the NSF Act made two basic changes:*

(1) the Foundation's authority to initiate and support scientific research was extended to include applied research at academic and other nonprofit institutions.

(2) When directed by the President of the United States, the Foundation was authorized to support applied research through other appropriate organizations, including those for profit, as long as that research was relevant to national problems involving the public interest. This authority was delegated in the Spring of 1972 to the Director of NSF.

The Foundation faced many difficult problems in implementing this legislation, since the type of research and relationship with the nonacademic communities were new to it. Aside from the usual start-up problems of defining goals and procedures, and assembling the appropriate professional staff, the potential territory was vast and largely uncharted. The first response was to establish the

*The amendments contained the following wording: . . . the Foundation is authorized to initiate and support scientific research, including applied research, at academic or other nonprofit institutions. When so directed by the President, the Foundation is further authorized to support, through other appropriate organizations, applied scientific research relevant to national problems involving the public interest. In exercising the authority contained in this subsection, the Foundation may employ by grant or contract such consulting services as it deems necessary, and shall coordinate and correlate its activities with respect to any such problem with other agencies of the Federal Government undertaking similar programs in that field.

IRRPOS (Interdisciplinary Research Relevant to Problems of Society) program. In fiscal year 1971, the RANN (Research Applied to National Needs) program was established, incorporating IRRPOS and other applied research activities.

The consolidation of existing programs into RANN constituted a mixed blessing. On the one hand, it gave some substance to the program at an early point. But, it also may have built some rigidity into RANN's activities which could curtail its options at a later point. Nevertheless, the program has experienced rapid expansion. It currently is expending about $70 million annually, slightly more than double its appropriation in FY 1971 when it began operations.

This study of RANN's priorities and objectives comes, therefore, at a juncture which could be crucial to setting the course for applied research development in years to come.

COPEP'S CHARGE AND PARTICIPANTS

In April of 1972, COPEP was aked by RANN to perform a broad study and review of national problem-oriented research priorities, especially as they relate to RANN's own program. Recognizing the highly interdisciplinary nature of that program, COPEP established itself as an *ad hoc* Steering Committee, with the addition of further representatives of disciplines outside the engineering field to give balance. Most of the additional members were also members of the RANN Advisory Committee. Because of the constraints of time, it was felt advisable to have on hand experts with prior awareness and experience with the RANN program. The representation for the study was further broadened in formation of problem-area panels. Experts were chosen from the social and natural sciences, the legal and health professions, the engineering community, universities and nonprofit organizations, private business and the user community (e.g., persons familiar with state and local government).*

*Six interdisciplinary panels were organized with membership of 9 to 16 persons each. Five of the six panels, focused on specific groupings or problems or national needs, were entitled: Human Resources (concerned with issues such as health, education, crime, safety); Community Development (concerned with issues such as housing,

STRATEGY OF THE STEERING COMMITTEE

On the weekend of May 20-21, 1972, the steering committee met with the Director of NSF, Dr. H. Guyford Stever; the Director of the RANN program, Dr. Alfred J. Eggers, Jr., and other senior RANN staff, to develop a plan for the study.

In approaching its task, the steering committee recognized the need to adopt a broad and coherent conceptual framework for planning that would reflect the operational engagement of science and engineering with society. The committee therefore turned to the development of a mixed strategy to cover both the long-term recommendations and an interim "first cut" to be delivered in time for the NSF's fall planning and budget cycle.

The strategy adopted reflected a number of assumptions: *First*, that RANN must address both the problems and opportunities

transportation, communications, municipal systems and services); Growth and Environment; Energy; and Natural Hazards and Disasters (including weather modification). In addition, a sixth panel was created to identify "Targets of Opportunity" which might not necessarily fall within the other five categories, or which warranted special emphasis because of their high or early promise of success.

Each panel chairman was provided with descriptive material about the goals of this study, about the current RANN program, and other, albeit limited, material related to the parallel work of other agencies. Senior RANN personnel were available both at panel sessions and the workshop to provide further insights and background information.

In addition, panel and steering committee members brought to the study all their experience and knowledge of the state of research across the nation. This was vital, since no master inventory of industrial or government-wide problem-oriented research exists. Accordingly, these members, together with the members of the RANN staff, provided our most comprehensive source of information about research programs that are being undertaken elsewhere and made it possible to recommend research for RANN that would interface with the work of other agencies and institutions, avoiding duplication yet filling gaps and treating areas where research is being performed inadequately.

Each panel chairman was largely responsible for the agenda and deliberations of his panel meeting, although the steering committee set the general scope and boundary conditions and COPEP staff provided assistance where desired. Each panel was obligated to prepare, in the course of its meeting, a report of its findings.

In September, the steering committee, augmented by the six panel chairmen and two or three additional specialists, met for a week as a workshop at Port Ludlow, Washington, to review the panel reports and from this work, together with a final day's meeting in October, to reach the judgments presented herein. In most cases, the recommendations of the panels are close to those of the workshop as presented in this report (the panels providing, after all, the principal intellectual input of materials to the workshop), but the workshop both deleted and revised panel recommendations.

amenable to technical amelioration and also the socio-economic, institutional barriers to solutions of national problems (i.e., the hardware and the software); *second*, that it must address research initiatives of both long and short term payoff; and *third*, that the overall study must take cognizance of RANN's needs both for thoughtful long-term policy guidance and for immediate recommendations which could be implemented in the context of the FY 1974 program.

The mixed strategy thus conceived concentrated on five objectives. The first three, which had varying program planning implications, were as follows:

(1) *Immediate priorities for RANN*, yielding ranked recommendations of use in planning its FY 1974 program—*the principal product of this report*;

(2) *Intermediate range goals*, concentrating on a deeper examination of the major technological delivery system, described above, through a more detailed development of selected research recommendations generated in the first phase exercise; and

(3) *Long-range research goals*, in which COPEP would focus on the broad range of human and societal needs and goals in the context of future social and technological horizons.*

(4) *Research Management*, involving an examination of the problem of establishing and maintaining the external research capability and associated internal management priorities and policies at RANN needed for performing interdisciplinary problem-oriented research, especially in universities and other nonprofit research institutions.†

*It is important to stress, in connection with the third objective, that time did not permit, nor did RANN request an examination or study of national needs, goals or priorities; that question being capable of answer only through the overall public and political process. In order to permit an organizing framework, the steering committee merely identified, inferentially, what seemed to be "needs" or problem categories on which there seemed to be consensus. It then quickly turned to the principal task of structuring a study to identify and rank, in light of such problem categories, research tasks which would be deemed appropriate for sponsorship by RANN.

†Substantial applied social science research is emphasized in addressing the deficiencies in the ways in which our society's manifold public and private institutions and public service delivery systems function. Throughout this study the concern has been voiced that our nation's multidisciplinary and applied social science research capability is

(5) *Integrating Research.* Assuming that the No. 4 objective is enhanced through the RANN program, the next step would be to examine the extent to which such capability and policies might be applied to the needs of the broader research sponsoring and user communities, including the civilian mission agencies of the Federal government and state and local governments.

The steering committee elected a taxonomy for panel jurisdictions generally different from the existing RANN organization and program structure. This was done, in part, to ensure that the study *would not inadvertently be bound to existing points of view* and, in part, to emphasize *the problem orientation and organization of the study.* It was recognized that this would create difficulties in cross comparison with ongoing efforts, but the opportunity to examine the spectrum of problems from a fresh viewpoint was considered of prime importance.

THE SELECTION PROCESS FOR RECOMMENDATIONS

In the course of this study the six panels made and considered several hundred recommendations for research in the light of the importance of the national problem addressed, their susceptibility to research within the general scope of RANN's mission, and certain criteria based on those used by RANN* but incorporating some modifications.† The goal of the study thus has focused on

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critically limited. Consequently there have been recurring recommendations that institutional forms of support are required to build up both the social science and institutional capabilities to undertake the kinds of interdisciplinary research recommended by this study. As these kinds of recommendations are within the scope of the next phase of our study, we take no position with respect to them at this time.

*RANN's current criteria consist of: Importance, Payoff, Leverage, Readiness, Capability, Need for Federal Action, and Role of NSF. While used as a guide, the workshop examined and considered variants of these and arrived at its final recommendations by combining individual aggregated ranking preferences, which were then translated into priority categories A, B, and X.

†At first, each recommendation was ranked on a scale of one to five, against five criteria: National importance (but also in RANN context); timeliness or ripeness of the research in relation to the problem addressed; the relationship of cost to pay-off; the

research recommendations to RANN rather than the ranking of national goals and priorities *per se*. Approximately 130 recommendations survived the panel deliberations. The workshop in synthesizing, integrating and consolidating related or duplicative recommendations and, on occasion, deleting proposals, emerged ultimately with an even more compact program and with a somewhat different taxonomy to that represented by the six panels.

What is presented in chapter 2, then, are detailed descriptions of the 31 specific program recommendations which have survived this process, presenting for each a brief statement of the problem and the consequent research objectives, followed by data on the estimated funding levels and the priority category assigned.

GROUPING AND RATING SYSTEM FOR THE RECOMMENDATIONS

The program recommendations have been grouped under six general programmatic headings:

- community development and human resources
- environmental quality and growth
- conservation of energy, materials and land
- new production processes
- natural hazards and disasters
- exploratory development and technological opportunities

likelihood of early coupling from research results to application; and the contribution which the work would make in expanding RANN's ability to meet RANN's purposes.

Three workshop sub-groups were established to review the recommendations of paired panel reports: Each sub-group chairman reported to the plenary session the results of his group's deliberations. A rating chart was prepared showing the recombined recommendations, the proposed budget and the sub-group rating. Each participant then scored every recommended project according to symbols which corresponded to budgetary levels, in order to achieve a sense of relative priority, e.g., the aggregate of recommendations to which an individual could ascribe the letter "A" could not exceed $60 million; a "B" rating meant the next $20 million; and so forth. Each program recommendation score was then totalled and the recommendations were displayed in rank order. Inasmuch as a number of anomalies were detected, it was agreed that a second look at the rankings would be undertaken at the final workshop meeting on October 22, 1972.

This taxonomy is by no means a perfect fit; certain programs could belong in several of the groups; others are not quite comfortable in any. The decision to establish such categories, however, has lent some order to the recommendations and should clarify the main thrusts of the overall program. No inference should be drawn, however, from the order in which these categories or specific recommendations are presented either in the text or in the two accompanying tables.

Table I lists the 31 program recommendations, grouped under the six programmatic headings described above. Also shown are their respective priority categories, A, B, or X and estimated funding levels. The funding levels are designed to indicate only an approximate order of magnitude.*

In order to display the overall trend of priorities developed, Table II presents the same 31 program recommendations by category of priority:

Category A—those programs considered of highest priority

Category B—those programs of next highest priority†

Category X—those programs for which adequate time or proper information was lacking to evaluate for purposes of priority with full confidence and authority.‡

The research agenda outlined in Tables I and II inevitably omitted certain problem areas. Some, to which attention should nonetheless be given, include social ethics and values, violence, resources, marine affairs, transportation, the economic health of

*In essentially all cases it should be understood that support at the level of effort indicated must be contingent on project proposals that credibly promise substantial results. In no case do we recommend *absolute* commitments of funds to an area prior to proposal reviews. In most cases, it seems highly likely that RANN should be able to find well-qualified research teams to carry out the work. But we do not assume this to be necessarily the case, especially in areas where detailed projects, as well as specific research teams to carry them out, remain to be identified.

†There is no ranking within Categories A and B. All of the programs in these two highest categories were identified as of particular importance with virtual consensus.

The reader should be reminded, however, that all programs described herein were regarded by the workshop as particularly promising, since, as noted above, they represent the survivors of a winnowing process which began with consideration by the respective six panels.

‡Thus individual programs within Category X, given more study, might well be moved to A or B.

the nation and international aspects of certain issues. This study also has not examined the cost-effectiveness of research recommended, nor has there been an opportunity to subject any of these recommendations to the rigors of technology assessment. Assessments of possible hazards or risks associated with the performance of the research itself appropriately could be made during the detailed research program planning phase.

MANAGEMENT PHILOSOPHY

Time constraints and the substantive orientation of this study did not permit consideration of RANN's management policies and practices. Nonetheless, each panel felt that its concern for the management implications and implementation of its research recommendations could not be left unregistered. For the most part, these issues have been deferred for later and more thorough consideration because of their importance. Certain elements of management philosophy, however, can be emphasized here: (1) unlike the programmatic agencies with more immediate problems than they can solve, RANN's research program must, in part, be oriented to the future; (2) similarly, it must be willing and, to an extent, eager to take risks in its choice of programs, projects and investigations. In this respect, some reasonable failure rate must be acceptable as an intended cost of program vitality, and (3) recognizing the early stage of its development, and the scope of problems which it must confront, RANN must resist the ever present temptation to seek a neat and tidy program package.

In planning its program, RANN must confront most of the complexities, problems, and dilemmas that confront modern man and society. The steering committee, through its workshop and the panels it convened, has of necessity done the same. In any ultimate and complete sense RANN's mission, whether with $70 million or $70 billion, is impossible, as was this study's broadest goal impossible of achievement in six months or even six decades. The development of a program of research applied to national

needs, like change itself, is an incremental game. But while acknowledging the inevitable frustration, and recognizing that ultimate solutions to society's ills can never be achieved, the committee derives some comfort from the adage: the perfect is the enemy of the good.

Chapter 2 Detailed Program Recommendations

This chapter presents an extended description of the 31 program* recommendations of the workshop. Under each numbered program is presented a statement of the problem, the research objective and a short description of the research effort which, either specifically or by example, should be included in such a program. Each program description is accompanied by an identification of category of priority—A, B, or X—and concludes with an estimate of approximate funding levels. Caveats to the recommendations and concerning the significance of these priority ratings and recommended funding levels were discussed in the previous chapter, "Procedure and Strategy."

For ease of discussion the 31-program recommendations have been grouped under six broad problem areas. The introduction to each of these areas reflects the background of societal concerns which influenced the direction of research chosen in the respective 31 programs.

The recommendations are displayed in Table I. Table II groups them by priority ratings. Because the 31 selected represent only a

*Each recommendation is called a "program" to distinguish it from a project, but none is a program in the broad sense of the term. The term program was deliberately chosen since it provides detailed advice, yet does not cramp the imagination of candidates for RANN support.

small fraction of the proposals forwarded by the expert panels, *all*, including those left in the unclassed category, should be considered important. Within each recommended research area, a number of subareas for research are suggested. The latter were not rated independently so that the committee might have felt some had a lower or higher priority than that assigned to the main area under which they fall.

The range of funding designated for various problem areas is tentative and suggestive at best, and should be determined ultimately by the quality of the research proposals received. In no case do we recommend *absolute* commitments of funds to an area prior to proposal reviews. Because an area is assigned many more dollars than another with the same priority rating does not necessarily mean it has more importance. The dollar amounts are based on rough assessment of the probable amount and type of research work involved and in relation to the amount of funds being invested in the relevant research area by sources outside RANN.

Table I Program Recommendations		Range of Funding FY '74 ($ million)
I.	Community Development and Human Resources	
1.	Improving Local Service Delivery Systems (A)[a]	20-25
2.	Social and Organizational Indicators (B)	1-±
3.	Evaluation Methodology of Social Programs and Services (B)	2-3
4.	Community Growth, Trends, and Forces (B)	5-8
5.	Regulatory Implications and Technological Innovations for Urban Growth and Population Distribution (B)	5-8
6.	Analysis and Implications of Change in the Socio Economic Sector (B)	1-3
7.	Communications and Transportation (X)	5-7
II.	Environmental Quality	
8.	Environmental Effects of Energy Production (B)	3-5
9.	Assessment of Environmental Research Efforts (B)	1-±
10.	Institutional Arrangements and Implementation Processes Related to Environmental Policy (B)	1-3
11.	Research on the Biological and Physical Environment (B)	7-9
12.	Agriculture and Lightly-Managed Ecosystems (X)	4-6
13.	Research on The Social and Economic Environment (X)	2-3
14.	Decision-Relevant Research on Environmental Systems (X)	8-12

Table I Program Recommendations (continued)		Range of Funding FY '74 ($ million)
III.	Conservation of Energy, Materials, and Land	
	15. Conservation and Management of Materials and Land (A)	6-8
	16. Energy Consumption and Conservation (A)	7-9
	17. Institutional and Regulatory Systems as They Affect Energy (B)	4-6
IV.	New Production Processes	
	18. Industrial Processes and Manufacturing (A)	4-6
	19. Energy Provision (X)	10-15
	20. Enzyme Technology (X)	2-3
	21. Excavation and Tunneling (X)	4-6
V.	Hazards and Disasters	
	22. Optimizing the Nation's Adjustment to Natural Hazards (A)	1-±
	23. Management of Hazards Caused by Surface Waters (B)	1-2
	24. Fire Research and a National Fire-Modeling Facility (B)	3-4
	25. Earthquake Research Program (B)	9-13
	26. Weather Hazard Modification (B)	4-6
	27. Experimental Short-Range Weather Warning Service (X)	3-5
VI.	Exploratory Development and Technological Opportunities	
	28. Technological Opportunities (A)	5% of Total Budget
	29. Exploratory Research and Assessment of the Future (A)	5% of Total Budget
	30. National System of Electronic Storage and Retrieval of the Printed Word (X)	.5-±
	31. Product Safety, Marketability, Risk Benefit Analysis (X)	.5-±

N.B. Ranges of funding are only estimates to provide additional insights (see text) and are not to be considered as planned program budgets.

[a]Parenthetical letters reflect category A, B, or X. (See Table II.)

Table II Priority Categories of Program Recommendations	Range of Funding FY '74 ($ million)
Category A	
Technological Opportunities (28)[a]	5% of Total Budget
Exploratory Research and Assessment of the Future (29)	5% of Total Budget
Conservation and Management of Materials and Land (15)	6-8
Energy Consumption and Conservation (16)	7-9
Industrial Processes and Manufacturing (18)	4-6

Table II Priority Categories of Program Recommendations (continued)	Range of Funding FY '74 ($ million)
Institutional and Regulatory Systems as They Affect Energy (17)	4-6
Optimizing the Nation's Adjustment to Natural Hazards (22)	1-±
Improving Local Service Delivery Systems (1)	20-25
Category B	
Environmental Effects of Energy Production (8)	3-5
Social and Organizational Indicators (2)	1-±
Assessment of Environmental Research Efforts (9)	1-±
Management of Hazards Caused by Surface Waters (23)	1-2
Evaluation Methodology of Social Programs and Services (3)	2-3
Community Growth, Trends, and Forces (4)	5-8
Institutional Arrangements and Implementation Processes Related to Environmental Policy (10)	1-3
Fire Research and a National Fire-Modeling Facility (24)	3-4
Regulatory Implications and Technological Innovations for Urban Growth and Population Distribution (5)	5-18
Earthquake Research Program (25)	9-13
Analysis and Implications of Change in the Socio Economic Sector (6)	1-3
Weather Hazard Modification (26)	4-6
Research on the Biological and Physical Environment (11)	7-9
Category X	
Agriculture and Lightly-Managed Ecosystems (12)	4-6
Communications and Transportation (7)	5-7
Energy Provision (19)	10-15
Research on the Social and Economic Environment (13)	2-3
Decision-Relevant Research on Environmental Systems (14)	8-12
Experimental Short-Range Weather Warning Service (27)	3-5
Enzyme Technology (20)	2-3
Excavation and Tunneling (21)	4-6
National System of Electronic Storage and Retrieval of the Printed Word (30)	.5-±
Product Safety, Marketability, Risk Benefit Analysis (31)	.5-±

N.B. Ranges of funding are only estimates to provide additional insights (see text) and are not to be considered as planned program budgets.

[a]Parenthetical numbers reflect recommendation numbers. (See Table I.)

I. COMMUNITY DEVELOPMENT AND HUMAN RESOURCES

The Problem

When people talk about loss of confidence in the delivery of services by government, they do not necessarily discriminate about the level of government. Nevertheless, it is a fair assumption that most of their experience with governmental services occurs at the community level. Their everyday concerns about health, education, law protection and a clean environment are affected most by the delivery of services in these areas at the local level. Public dissatisfaction with these services is now perceived as a problem of failure to fully develop the human resources of the community or at least that of portions of the community. Community development, on the other hand, was considered in developing these recommendations as part of a general consideration of the needs of local and community government and administration.

In the human resource area, dissatisfactions are particularly keen when the main problem no longer appears to be inadequate financial resources but inefficient and misdirected application of those resources. Whether it be health care, police service, the administration of justice, the collection of refuse, or the provision of aid or education and training, people are dissatisfied if they do not know what service is available, who provides it, and how they can obtain the service when they want it. They ask that the service be provided with a tolerable cost in time and dollars, and that it preserves their dignity.

Broad Research Considerations

Understanding of the research needs in this area requires the development of an holistic view of the problem. Where problems occur and public needs are not being well served, there is a need to understand the trade-offs between alternative "solutions" to the problems. For example, decision-makers' priorities are influenced by the trade-off between spending money on highway safety to save lives or reducing highway deaths by providing alternative transportation, or again by the interreaction between programs to

provide welfare and policies to eliminate the need for welfare. These relationships need to be defined and evaluated.

Research to find ways of improvement must also take into consideration the institutions performing the service, the market, the engineering and physical science techniques involved, and the means by which performance can be evaluated. If the institutions are not functioning effectively, either in their decision-making or administrative processes, if the public cannot express its needs and preferences or cannot appreciate the intent of the administrators, if the technology to improve institutional performance is too costly or is less effective than other technology which could reasonably be developed, then the service is in some way compromised.

Research* must take into account all aspects of the problem. For example, new technology, however well designed, will fail unless the decision making and administrative processes support its application. Time and time again, problems which appear due to technical, financial or personnel factors really originate from malfunctioning of institutions or faulty institutional design. Or the problem seems so wholly new or unexpected that the lack of institutional responsiveness is rationalized as a temporary phenomenon during a period of upheaval and change. Nevertheless it is society's institutions–government, quasi-public or private–which have the power to reduce the harmful consequences of new societal forces or demands, to seek out neglected beneficial consequences or, too frequently, to exacerbate the problem.

1. IMPROVING LOCAL SERVICE DELIVERY SYSTEMS (A)

The Problem

Public dissatisfaction in this area does not require great sophistication to understand. Refuse collection, for instance, is

*In order to appreciate fully the research needs in this area, it was found useful to develop a global analytical structure for research questions. The analysis, developed in our community development panel, forms Appendix B and is provided as background for consideration of the limited program recommendations contained in the section on Community Development and Human Resources.

often infrequent and noisy. The criminal justice system is plagued by delay in bringing cases to court, leading to unnecessary anxiety and harm to accused and citizens alike, thus proving the adage "justice delayed is justice denied."

In the area of health care, the public demand for services is leaving a great many people dissatisfied. There are opportunities for improvement both through development of new technologies, and changes in the organizational structure of what we define as health care, but we have yet to determine how best to integrate the two approaches. The fire service operates with equipment much of which is based on technology developed 50 years ago. Many fire departments believe that they lack adequate means to cope with modern needs, e.g., with fires in highrise buildings. There is also lack of sophisticated devices for quickly locating the source of fires. As a result, lives are lost and property is destroyed in situations where harm might have been limited or prevented. A list of similar failings in housing, transportation, welfare, education and other service areas could easily be compiled.

Research Objective

Research in this area has been shaped by the need for an holistic approach to the problems discussed earlier. The research recommendations, however, also reflect the judgment that the behavior of the public sector is particularly significant. Some of the key institutional elements which determine the degree of responsiveness to change in this area (and ones which we know relatively little about in a scientific way) are the performance of legislatures, relationships between the electorate and legislatures or legislatures and administrative branches of government, the incentives for performance in bureaucracies, community participation and organization and the climate of public opinion. "Conflicts in governments" form another set of problems touching on the relationship between Federal and local or regional and local units. Nor should the potential for technological innovation be overshadowed.

The recommended research on improvement of local service delivery systems is grouped below under three general headings:

1. Evaluation of what is known about existing systems
2. Identification of new management methods and institutional/organizational alternatives
3. Technological innovation

The objective of the program should be to discover means of improving performance and/or reducing costs. Aspects of the research should include examination of the source of demand for the service, the range of types of possible services, the problems of manpower deployment, identification of operations amenable to mechanization, study of physical science and engineering technology available for development and application, a consideration of possible alternative organizational structures supporting or providing these services, and the development of organizational indicators comparable to and on similar dimensions with social indicators. Quite frequently, however, the solution to the problem may be found outside of the immediate area of the problem, possibly in some other part of the social system, and for this reason, research must recognize the context in which problems occur.

Specific Research Strategies and Programs

Evaluation of what is known about existing systems A great deal of experience, particularly in the past decade, has been gathered on delivery systems. Very little of that experience has been assembled and much of it lies scattered and, in effect, lost. Analytical surveys are needed which put conclusions into a form which is useable by decision makers. This may prove to be particularly costly if continuous and cumulative updating is undertaken. RANN eventually may have to encourage some other body to take over the work recommended under this heading. Certain programs have been recommended in greater detail below.

- *Analyze existing information and research on municipal systems* A series of analytic surveys should be made which are designed to extract relevant policy propositions from urban research, assess the confidence users can have in such research and identify lessons learned and gaps in such knowledge. The results

relevant for policy can therefore be drawn into coherent form. The surveys should be developed to the state where continuous, cumulative updating is possible.

- *The supply side in urban research* An examination into the capacity of institutions, and their personnel recruitment and incentives policies, for policy-oriented urban research is needed. This should include research into the availability of persons and resources, the ability to maintain large and diverse teams, and the consequences for teaching, finance, governance and other university functions.
- *Demand side of urban research* On the demand side, some governments have capacities to utilize urban research and to incorporate results in policy and administration. Most local governments lack such capacity however, both for administrative and political reasons. The problem is complicated by pervasive inter-governmental processes which render policy change slow and difficult. Studies of both successful and unsuccessful experiences in a variety of local government situations should lead to modification in governments which sense the gap, and to a recognition of the opportunities in governments which lack an awareness of the problem today.
- *Assessment of transfer of knowledge experience–hard sciences to urban issues* There are a large number of systems delivering services for people, e.g., health, justice, education, public safety, etc. Most of these services require improvement. Our state of knowledge with respect to these systems is not such as to provide guidance for intervention, i.e., the consequences of our intervention are elusive to prediction. There are a number of strategies for studying organizations providing social services such as longitudinal studies of a given organization, comparative analyses of one type of organization or different organizations, development of organizational indicators, social field experiments, simulations, surveys of clients' perceptions, etc., or a combination thereof.

Identification of New Management Methods and Institutional/ Organizational Alternatives

Examples of areas where improved management methods and institutional arrangements are critically needed and where investment in research would yield significant, short-run dividends are (a) the criminal justice system; (b) the penal system, and (c) the fire fighting system. Certain other areas recommended for attention, such as health and education, already are the targets of research by many Federal agencies. In these cases RANN should use a "generic" approach, funding a series of projects examining, for example, personnel policies rather than a series of projects each focused on one particular service area. The research under this heading explicitly should include development of organizational indicators and experimentation with and evaluation of new techniques in actual operating systems. To improve feedback in

nonprofit local service systems, research should also be carried out on evaluation and accountability techniques.

Research on *accountability* is taken to mean the development and testing of information feedback processes that enable clients and managers of nongovernmental and governmental systems providing social services to assess their performance regularly and accurately. Such orderly and continuing feedback enables the managers to learn about shortcomings of their operations and correct them. In short, holding officials to accountability standards requires a well-defined set of performance measures and an appropriate set of sanctions and rewards. Those served can use these to influence the system to bring about desirable adaptations to meet their needs.

Research on *evaluation* would include studies of methodology, but the primary focus should be on the development and validation of measures of cost effectiveness.* The proposed projects should also conduct tests and experiments to demonstrate the feasibility and usefulness of these measures in choosing among alternatives for the design and operation of governmental service systems. In brief, the philosophy and techniques familiar to systems engineers in high technology areas should be considered in the development of similar methods useful for designing and operating governmental and other systems providing public services.

Programs recommended in greater detail are:

- *Management methods* Many of the failures in the provision and delivery of local services can be attributed to inefficient and outdated organization and management practices. Modern management methods and techniques which have been successfully introduced into industry have seldom been applied in government, and where they have been introduced they have not always led to improved performance. Research should concentrate on improving the resource management of local governments through analysis that takes into account the political and legal constraints placed on public and social organizations, and through experimentation with and evaluation of new techniques in operating systems.
- *Personnel policies for government* The selection of personnel and the attitudes of individuals are intimately involved in organization and practice. Therefore,

*This term is used in a broad, generic sense here. It includes concepts such as "cost-benefit," "cost-efficiency," and "risk-benefit," which by various writers in various disciplines are used in closely similar, but not identical ways.

before any new management technique can be developed, research is necessary to understand the nature and the impact of the civil service system, the implications of the unionization of public employees and the significance of the increased specialization of the bureaucracy.

Some areas of service, notably those of health and law, suffer from the lack of personnel, which is related to their recruitment policies. Research is necessary in these fields into the relevance of traditional standards of entry.

- *Meeting new and changing municipal service demands* Society is continually changing. New values and new social and structural patterns give rise to a demand for new or different municipal services. Research is necessary to anticipate such demands and to devise how changing needs can best be served. Some can be done by established organizations which, at present, may be doing it poorly, but a significant contribution can be made by RANN. Also a variety of new services are emerging which are candidates for municipal management and financing, possibly with Federal aid. Child care, nursing homes, legal assistance, neighborhood health services are recent examples. There should be a systematic inventory of these emerging demands, of feasible institutional frameworks for them in government or private firms, of the financing systems, public and private, which might support them, together with their distributional, equity and efficiency effects.
- *Distribution of responsibility for government services and costs* Public services for a local community are provided by five or more levels of government and by various quasi-public bodies and institutions, each with different geographic jurisdictions and financed in diverse ways by user charges, special taxes, general taxes, or by State and Federal subventions. The resulting pattern of services, qualities of services, and costs of services generate erratic development patterns, districts with rich and poor services, high taxes and low and have other adverse social, economic and governmental consequences, illustrated dramatically in the California case of *Serrano v. Priest* and similar cases in other states. While there are a number of excellent studies of particular metropolitan areas, or, more rarely, of states, and some generalized national studies in the field, these studies fail to cover the full range of situations, and rarely examine the range of alternatives and their social, economic and political consequences. Further, the scale of the studies has been too small to develop authoritative quantitative data or relationships. As a result, emerging policies tend to be uninformed and to generate unanticipated consequences. Research is therefore necessary to determine the consequences of the present distribution of responsibility for government services and costs, and to analyse how changes in such distribution would affect the cost, the quality and the efficiency of public services.
- *Intercity comparisons* Intercity comparisons of the nature, extent, inputs and outputs of each municipal service would provide a new perspective on the problems and policies of the individual city. The major emphasis should be on quantitative, cross-section analysis of U.S. cities, but studies of foreign cities would also be desirable. The behavior of local governments under very different institutional rules can suggest institutional changes, and also can highlight the relationship between institutional arrangements and the efficiency and effectiveness of individual service delivery systems.
- *Alternative institutions* Research is necessary to determine which services traditionally provided by public agencies and public employees on a monopoly basis could be provided competitively. A public monopoly can sometimes become a captive of the employees, and fail to respond effectively to consumer needs.

Analysis of the cost and effectiveness of the service provided via a competitive market structure has particular relevance for policy decisions.

Related research should also examine the consequences of government support on the demand side rather than the supply side; e.g., government subsidy of the market rather than being the direct provider of food, housing, health care, education, transportation, refuse collection, culture and recreation.

- *Evaluation of processes of policy formation and implementation* New technologies or solutions related to the functions or organization of local services frequently fail to survive the policy making process of government, or else emerge in unrecognizable form. For example, a welfare system is devised and designed to provide particular incentives, but is then funded at a level too low to make those incentives effective; or considerable time and effort are spent devising a mass transportation system for an urban area, yet highway projects continue to impede it from providing a proper service. The problems, therefore, of transfer from research and development to application have become serious and evaluation of the policy making process is necessary if the full value of research and technology is to be realized. Such research might examine the characteristics of both the legislative and administrative branches of government and also the role played by citizens, especially as a "veto" group. The interreactions of the parts of the decision-making process also bear analysis since, again, they have an extensive impact on policy.
- *Evolving representative institutions* The last decade has seen the evolution of a variety of new groups designed to influence or fill functions related to those traditionally performed by local governments, e.g., poverty program councils, and ethnic, consumer and environmental organizations. This evolution is a major change in local government. The initial research goal would be to understand what is happening through an inventory and comparison of the different forms and character of such groups.
- *Pricing policies and their impact on services* Research should examine pricing policies and how they can be used to change the pattern of demand for public services and utilities. The effect of such changes on the cost and quality of services should also be investigated. The initial stages of a theory of public pricing have been well worked out, but there is a need to make the theory operational and to get better understanding about how to implement optimal pricing schemes to achieve local government and societal objectives.

  One particular example that has been recommended for study is waste disposal management. Waste generation and collection problems may be valuably influenced by such policies as depletion penalties, tariffs favoring recycling and by disposal tax on various points in the production/consumption cycle.
- *Incentives: extra legal and informal* Extra legal incentive payments are said to be characteristic of some local governments, yet their nature, extent, and magnitude have never been carefully researched. The demand for special treatment is a signal for a faulty allocation of public services or an attempt to redistribute income privately. In either case, public programs will not function as conceived and adopted. An understanding of the factors giving rise to the payments and their consequences for the behavior of the local public sector will provide knowledge for a better design of a more responsive and responsible set of government institutions. Systematic research on illegal and quasi-legal activities is difficult, but almost any authoritative findings can contribute to the development of more satisfactory operating rules for government.

Technological Innovation

Population and economic growth place pressures on local services in terms of rising costs and new problems which do not fit old technologies. The shift from labor to capital intensive production in industry has produced benefits applicable to local delivery systems which have not been adequately exploited. Municipal governments and other entities concerned with these services frequently lack the expertise and funds to develop and adapt the new technology required, and industry is discouraged from investing in technology and making it available at an affordable price because of the disaggregated nature of the market. RANN can make a significant contribution by supporting applied science and engineering research to bridge the gap between basic research and the development of operational hardware in certain areas of high potential payoff and/or acute need.

- *Solid and liquid waste disposal* The disposal of wastes, both liquid and solid, is an important and costly function of municipalities, ranging from collection to disposal. New technologies are necessary to increase efficiency in the system and to protect the environment.
- *Fire and police protective devices and communication systems* Relatively little new equipment has been developed for the fire and police services during the past decade. Research is particularly necessary into warning and other devices to improve communications for fire and police operations and into protective devices for both firemen and police.
- *Automatic routing and control of traffic* The development of new technologies for traffic direction and control would contribute significantly to the traffic flow in congested cities particularly at peak hours and would speed essential services in case of emergency.
- *Snow removal* For many cities in the snow belt, snow and ice present a serious inconvenience and a hazard. Research is necessary to develop new techniques to remove snow and ice or else to minimize the dangerous effects of snowfall and ice-formation.
- *Education* Research could produce technologies which will assist educational services both to the student in school and to the potential external student. Technology might also be employed to improve the school environment.
- *Access to legal advice* At present, access to legal advice is both complex and expensive. Some form of computerized information system if developed and made accessible would greatly aid the general public particularly in the case of simple problems or in the initial stages of more complex proceedings.
- *Noise reduction* Noise is both a pollutant and at times a danger to health. New technologies directed at reducing noise levels would contribute significantly to the comfort and safety of daily life.

It should be noted that these are only examples and considerable program planning and problem assessment is required to develop specific projects in the light of user needs, research and development in other agencies and detailed resource allocation.

The overall program recommendation represents a recommendation to RANN for considerable expansion in size and scope of its existing work and a close tying together of its technological and non-technological research in this area.

Funding Levels–Improving Local Service Delivery Systems	FY '74 $ millions
Evaluation of what is known about existing systems*	8.0
New management tools–institutional/organizational alternatives†	8.0
Technological innovation‡	6.0
Total	22.0

2. SOCIAL AND ORGANIZATIONAL INDICATORS (B)

The Problem

Government tends to shape many of its social policies according to economic concerns without necessarily providing sufficient weighting for the social costs and benefits of such policies. The highly developed state of economics as a science with recognizable measures and indices tends to encourage this. Until there is some measure for the effectiveness of a service on recipients or for its impact on the "quality of life," government will find it difficult to introduce social factors into policy decisions to balance economic

*Half of this would go to the program: "Assessment of transfer of knowledge experience."

†Program distribution here is fairly even but with slightly larger sums allocated to "personal policies" and "distribution of responsibility for government services and cost."

‡Little can be said about distribution of funds within this heading except to recommend that particular emphasis be given to health care delivery in seeking technological innovations.

considerations. Meanwhile needs are neglected and socially undesirable aspects of policy may not be properly considered.

Research Objective

The research objective is aimed ultimately at designing a sophisticated method for measuring social change. Steps along the way include a complete and coherent program for collecting data, nationwide and locally, and for developing conceptualizations and related methodologies for designing indicators.

Specific Research Strategies and Programs

The following areas have been suggested for research, but they are certainly not exclusive.

- *Social Indicators* There is a developing consensus that social indicators (explication, development, etc.) are an important area for research. The NIH and NSF (Social Science Division) are involved in funding such efforts aimed at essentially different aspects of social change. In addition to proposing measuring social change in a variety of ways and for a variety of purposes, many advocate that social indicators should be used for the preparation of an annual social report. Without commenting on the advisability or the content or uses of a social report, RANN should continue to fund work such as the measurement of perception of the delivery of social services, and other such efforts as may not be funded by other federal agencies. The existence of the Center for Coordination of Research on Social Indicators provides an opportunity for RANN and other federal agencies funding related work to develop a complete and coherent program.
- *National data analysis* Support is provided for problem-oriented analyses of social and economic data from sources such as the census, current population surveys, consumer expenditure surveys, and various special data collection efforts sponsored by the Federal government. These studies are well-funded, but frequently additional scientific or policy implication can be gained with modest supplementary grants.
- *Neighborhood data within cities* There are a large number of municipal information systems which now provide information about municipal operations and activities. But these are not coupled to social data about the populations being affected. The latter data comes from special surveys. By merging operational data with the social data, direct tests of the effectiveness of municipal operations can be made. Research is recommended on the software necessary to merge social data with operational data of organizations and on the techniques needed for relating these varied data–social and organizational indicators–in order to provide sophisticated information systems for policy makers.

Funding Levels	FY '74 $ millions
Social and Organizational Indicators*	1.0

3. EVALUATION METHODOLOGY FOR SOCIAL PROGRAMS AND SERVICES (B)

Many agencies perform evaluation studies, but none address themselves to the basic theoretical questions or measurement approximations. Without more sophisticated methodologies, it remains difficult to learn from past and present experiences or to develop useful designs for social experiments. Evaluation and experimentation are also particularly difficult in the public sector where methodologies must include an appreciation of legal and political constraints. One of the factors inhibiting the widespread use of evaluation studies is their limited authority due to imperfect conceptual underpinnings and measurement techniques. Different practitioners and different agencies employ conflicting approaches.

RANN especially could contribute to the development of techniques of evaluation and the design of experiments, since it has no vested stake in any service and can more easily avoid the danger of evaluation becoming rationalization and experimentation leading to demonstration.

Research Objective

The research objective should be to develop techniques and standards of measurement relevant to evaluation. Present methods need to be studied and evaluated. Some suggestions have been made that the development work should be carried on at specially created institutions or centers. This idea may or may not be

*Most of the money is intended for the study of social indicators, not elsewhere funded. Data analysis should form only a small part of the research, but is included because of its potential useful return for the little cost involved.

feasible, but it highlights the need to study how this work can best be organized and conducted.

Specific Research Strategies and Programs

In addition to work examining present methods of evaluation, the following project areas have also been recommended:

- *Measurement techniques and data processing* Research is directed to measuring outputs of public programs. There is growing awareness that accountability requires both input and output as well as internal measures, but individual cities do not have the research capability to solve the conceptual and measurement problem. Beyond existing programs there is a need for systematic reporting on the state of social problems as attempted by social indicators, as covered in the previous proposal.
- *Improved designs for social experiments and evaluation* With an increasing appreciation of the complexity of social processes and social problems, it is becoming apparent that some or many social programs should be tried out on an experimental basis before full scale implementation is attempted. Research should therefore be carried out on methodologies for designing and interpreting the results of social experiments. Several large scale social experiments are now in the design or implementation stage. They provide unprecedented opportunities to gain understanding about policy alternatives. These experiments also raise unprecedented methodological issues which require solution.
- *Organizing for observing spontaneous or naturally occurring phenomena* Research to predetermine methods for organizing and managing the observation of unique events, for use when the proper situations arise, is needed generally in the applied sciences. The social sciences are not an exception. Without preparation in advance, opportunities will be lost to learn from short-lived phenomena. Consequently the evaluation improvement effort should explicitly include this class of observation.

Funding Levels–Evaluation Methodology of Social Programs and Services	FY '74 $ millions
Evaluation and similar research	0.1
Measurement techniques and data processing	0.75
Improved designs for social experiments and evaluation	1.0
Organizing for observing spontaneous or naturally occurring phenomena	0.5
Total	$2.35

4. COMMUNITY GROWTH, TRENDS AND FORCES (B)

The Problem

As our nation approaches its 200th birthday, the communities where we live continue to change. They lose or gain population. Some are abandoned. Some are absorbed into larger jurisdictions. The character of their population changes. Old industries die or move away. New ones emerge. In fact, change appears, at times, to be the only constant in the places we live. Yet we understand very little about the emerging economic and social forces which will undoubtedly influence how they change in the next decades. Little attempt has been made to prepare for these changes even where their direction has been anticipated. Unnecessary environmental damage has often accompanied new development while the new communities have frequently fallen short or in some cases failed to satisfy the needs that first impelled people to create them. Meanwhile, abandoned and declining communities are left without any comprehensive national or local policy addressed to their fundamental problems which eventually erupt into the well-headlined crises of poverty, racial conflict, obsolescent physical capital, fiscal distress, crime, rigid bureaucracies and declining relative wealth and income.

Many of the government programs which have been deployed in central cities and declining communities and regions have been, in effect, palliatives dominated by immediate problems of human and capital investment. Some of these have been based on unverified and often doubtful assumptions about the present and future of these communities. Until we know much more about the role of the central city within the metropolitan area and of the declining towns in terms of likely future settlement patterns, policy will continue to be based on myth and more than likely will result in ineffectual or inefficient use of resources, to say nothing of lost human opportunities.

Many of the environmental problems of new communities and urban growth are discussed in the Environmental Quality and Conservation of Energy, Material and Land sections of this report. In this section we are concerned with a broader range of effects brought about by particular development and building processes.

These structural changes have not been understood in depth. Policy makers, therefore, have made decisions on premises of doubtful validity. Where this occurs, schools are overbuilt, training programs do not match new skill requirements and jobs, households and public service facilities are poorly located.

Research Objective

Research programs should deal with those community growth trends which seem amenable to public policy control or guidance, if adequately understood. They should include: analysis of the changing structure of the economy and the influence of these changes upon urban societies and governments; analysis of the building processes which affect the form of cities and which generate impediments to urban growth; and an examination of the environmental effects of settlement and of the environmental consequences of second and mobile home communities. Further policy studies are proposed on areas of declining populations, of central cities and small towns in order to devise social and economic policies to ameliorate these changes. It is assumed that part of the research in this area will be related to examination of the various relevant delivery systems, including planning systems, and therefore will include the kinds of management and evaluation studies described under "Improving Local Service Delivery Systems."*

Specific Research Strategies and Programs

- *Social and economic aspects of areas in decline* Migration from rural areas and center cities to the new emerging population centers has caused dislocation in the areas of decline. Research should address the social functions and structure of these areas in order to identify the possibilities of affirmative policy regarding these movements. Specific concerns are the abandonment of rural small towns and the functions and structure of central cities. The central cities are the repositories of obsolescent physical capital and the metropolis' low productivity labor force. These problems interact and inhibit the natural improvement process which may have taken place. A better understanding of these problems and the desirable function would facilitate the design of better and viable programs.

*See page 33.

- *Trends and policies for social, economic and ethnic composition* Neighborhood character has been an important element in political thinking and rhetoric for the past decade at least. An understanding of the communal use of space is needed to formulate policies which will ensure opportunities for a reasonable standard of life and dignity for every citizen whatever his race or social class. Through the market the city has organized itself into neighborhoods. Many public programs seek to defend neighborhoods but just as many are destructive. We have little more than an intuitive understanding of neighborhood decay, the value assigned to neighborhood features or the conditions of viability. Useful experimentation and research in this area should include the development of indicators of change, which assist policy makers, and exploratory research to document new ways in which people show their concerns about their residential environments (e.g., reaction against massive projects, formation of alternative life systems, concern for the "quality of life").
- *Institutional constraints on development and building processes* Some understanding of the conditions which influence a builder are necessary to policy making intended to guide construction of housing or other facilities. These conditions include codes and code enforcement, zoning variances, building trades unions, work rules and wages, technology transfer, methods and costs of financing. Also included is research to understand development patterns and how density and other development variables affect the people. Studies should also review the regulations governing land use. Development imposes costs upon local communities (externalities) which have led to widespread resistance to growth, and the development of elaborate impeding or cost raising codes and procedures. Analysis of these costs in ecological, social and economic terms, and of the incidence and shifting of burdens should lead to a rationalization of development processes which permits needed development, regulates quantity, location, and quality, and assesses costs equitably.
- *Environmental studies in settled areas* An understanding of the ecosystems of all areas where people settle is essential to proper planning and development. Exploratory thinking into all aspects of man's relationship to the environment, including social and economic relationships, are recommended. It is known, for example, that urban climate differs from rural climate, and thereby leads to changes in animal and plant life.
- *Emerging second and "mobile" home development and their policy problems* New areas are being developed which permit more people to use more of the outdoors. Now there are second homes in the mountains or at the shore, and mobile home parks which may or may not be "second" or recreational homes. These have been expanding at a rapid rate with little attention to their environmental implications. An understanding of the socio-economic-cultural reasons for this trend will be helpful for future land-use planning and to ensure that the demands which first gave rise to the new development are satisfied by the new communities. RANN should continue, but at a lower funding level, its present ecological base line studies of new planned second home communities but should also fund similar research, where feasible, in unplanned second home areas and mobile home parks near to metropolitan areas, to understand how the ecology of these areas is affected.*

*This element was considered of significantly lower priority than the rest of the group.

Funding Levels–Community Growth, Trends and Forces	FY '74 $ millions
Social and economic aspects of areas in decline	2.0
Trends and policies for social and ethnic composition	2.0
Institutional constraints on development and building processes	0.5
Emerging second and mobile home developments	0.9
Total	$6.0

5. REGULATORY IMPLICATIONS AND TECHNICAL INNOVATIONS FOR URBAN GROWTH AND POPULATION DISTRIBUTION (B)

The Problem

The growth policies of the nation have major consequences for population dispersal, integration, jobs and the future of the central city. While national forces are at work determining community growth, the main regulatory effort is being exerted at the local level, chiefly through land use controls such as zoning and planning,* and a national land use policy is only now in the process of being formulated. Local regulation, without a national policy, is a haphazard method of trying to influence community growth patterns since the varying and uncoordinated nature of local regulatory policy frequently encourages negative or positive incentives for certain types of land use or construction which may be (temporarily) beneficial to one community but harmful to another. The end result of unplanned growth and decline is one or more of the following: dissatisfied citizens, a damaged environment, and inefficient use of resources. Although the federal government has been prompted by such problems to consider action on a national level many significant issues will not be resolved and, without greater understanding, will continue to be intractable.

*Although there is no national growth policy, the development processes, as with most processes in this country, are enormously influenced by Federal and state policies, many of which were designed with primary attention to another area of concern.

Research Objective

It is important to devise alternative methods of land use regulation and controls which meet new demands by the public for effective regulation of development, the preservation and conservation of open space, and the reduction of scattered development or urban sprawl. The research should examine existing policies and methods of guiding urban growth and analyze their consequences.* This will require development of new and more effective analytical techniques.

Specific Research Strategies and Programs

- *Methods and consequences of existing regulatory controls on development* Growth policies are being determined at the local level in a national policy vacuum. The nature and extent of this process needs charting, and the consequences have to be analyzed and understood, whether the objective is to continue the system or change it. The mechanisms used are varied and often ineffective or canceled out by the policies of other communities. These mechanisms include land use control techniques such as zoning; control of the basic service infra-structure; bedroom zoning; local fiscal policy, etc. The consequences of the policies extend to issues such as population dispersal, integration, employment and job training opportunities. Especially involved is the question of the future of the central city.
- *Federal and state influences upon development* Although there is no national growth policy, the development processes, as well as most processes in this country, are enormously influenced by Federal and state policies, many of which were designed with primary attention to another area of concern. A detailed analysis of the indirect and direct consequences of activities such as: Federal, state and local taxes, direct subsidy programs, Federal and State economic regulatory agencies, loan guarantee programs, etc., should be undertaken with a view toward aiding the consistent application of policy.
- *Urban design methods and institutions* Urban design is an important ingredient in the "quality of life." Improvements are being made using improved design methods including new technological innovations. However, new technologies, such as remote sensing, are valuable only so long as there are capacities to use the information. There is a need to develop modeling skills sophisticated enough to use all the information that can be gathered by conventional methods, and we should concentrate on improving their use.

  Attention should be given to state and metropolitan area institutional arrangements for guiding urban growth, including the protection of flood plains and methods for regeneration of land for recreation, parks, schools and for future transportation corridors, and the funding of development plans.

*Including the unforeseen urban development consequences of Federal and state policies as described below.

Among the important subjects for urban design innovation is the development of energy conserving urban design prototypes. These would include neighborhood, office, and retail districts which consume energy in lighting, heating, air conditioning and the like.

- *Transportation and urban development** Research is needed on relationships between urban design and transportation planning for metropolitan areas, and to examine the possibility that public utilities, transportation and communications corridors are the essential basis for comprehensive planning of neighborhood growth units. Research on properly integrating urban design, access, and transportation corridor design could lead to a much better quality of suburban living.
- *Applied science and engineering research for solar energy systems*† The energy of sunlight for the most part is neither utilized in the heating of buildings during cold weather nor appropriately controlled during hot weather. Applied research is needed to develop additional devices, heating and cooling units, and building systems which, through a proper combination of design, configuration, and new technological devices, will convert the energy to heating in the winter and cooling plus shielding in summer, in an economic manner.
- *Conservation policies and methods for urban fringe and urban affected land*‡ Limitations in the availability of land for development are already in view. Research should be directed along two approaches to mitigate the force of such limitations: (1) to reduce consumption via more efficient utilization or by modulation of demand, and (2) to develop under-utilized space both above and below ground.

Funding Levels–Regulatory Implications and Technical Innovations for Urban Growth and Population Distribution	FY '74 $ millions
Methods and consequences of existing regulatory controls on development	1.0
Federal and state regulatory influences upon growth patterns	1.5
Urban design methods, institutions and innovation	1.0
Transportation and urban development	1.0
Applied science and engineering research for solar energy systems	1.0
Conservation policies and methods for urban fringe and urban affected land	2.0
Total	$7.5

*See NAE report, "Transportation and Urban Development," recommendation 6, and The American Institute of Architects, "Plan for Urban Growth," January 1972.

†Emphasis on solar energy provision received the Committee's "A" priority even though the grouping as a whole received a "B"; see also recommendation 20 re energy provision.

‡See also recommendation 15 re conservation and management of land.

6. ANALYSIS AND IMPLICATIONS OF CHANGE IN THE SOCIO-ECONOMIC SECTOR (B)

The Problem

Technology, economic activity, population composition, life styles, personal values . . . these and other forces have been and continue to change rapidly. Many of our assumptions on the present nature of the socio-economic environment which should be considered for community development are obsolete. One outstanding example is the continued reliance on development of an economic base for employment composed principally of extractive and manufacturing activities despite the fact that capital, in these sectors, is being extensively substituted for labor. The most rapid growth in employment opportunity now, and for the foreseeable future, is in the service sector. Other trends, whose consequences are poorly understood and therefore are not factored into policy, are substantially changing the structure of urban life:

- the declining rate of population increase
- consequent changes in population age distribution
- rising age of entry into the labor force
- the potential for earlier retirement
- reduction of the working year and week

Research Objective

Research should produce an awareness of emerging trends which can contribute to a more intelligent shaping of policies. At the start, research is needed into the assumption (often implicit) about underlying trends and social forces which shape current policies. Though the forecast of the future is bound to be hazardous, a better grasp is possible.

Specific Research Strategies and Programs

The implications of change in the economic structure are so widespread that only a few, hopefully the most critical, have been identified:

- *Implications of changes in economic structure*
  (a) This research should be directed to clarifying the social, economic and employment implications of the structural changes that have taken place in the economy during the past two decades. A foundation for forecasting future directions in this trend should be developed. For this purpose, the service sector should be examined in as disaggregated a form as possible.
  (b) Research into organization of the service sector should be concerned with the process of innovation in services, problems of capital formation, problems of entry, the choice between public and private supply, and related matters.
- *Implications of increased growth of the public sector* It is likely that the public sector will continue to grow relative to the private. The public sector has different patterns of skill requirements, personnel recruitment, tenure, management practices, policy determination and financing. All of these should be examined in the light of emerging trends in demographic and industrial structure.
- *Locational implications of change in demographic and industrial structures* The locational propensities and requirements of these major structural changes are not well understood. Jobs and residences will be spatially redistributed. Some cities and transportation systems will become obsolescent, environmental problems will develop and a redesign of public programs will be necessary.

If research of the type listed above is undertaken, it should produce results of considerable direct value to policy formation. It should also provide a basis for the following additional kinds of needed inquiries:

- *Possible use of changes in demographic and industrial structures and levers to assist in effecting new settlement patterns* No national urban policy, concerned among other things with present and future settlement patterns exists at this time. Such a policy is needed as are studies of the various instrumentalities and levers with which it could be implemented. The emerging structural changes may well provide some points of leverage in moving toward changed and restructured settlement patterns.
- *Technology and economic structure* The value and desirability of technology has generally been estimated solely in terms of cost or economic efficiency. Research is needed to define new measures that relate to broader issues such as distributive effects, likely benefits to and demands on the public sector, likely subsidy requirements, and locational effects.
- *Evaluation of experience in mobility facilitation* This is a slightly more distant program which yet falls within the general area and is of particular concern. Poverty and inefficiency may often be attributed to structural defects in the mobility of people, capital, technology or management, which prevent full usage of a resource. Areas, industries and occupations decline and the resources which respond sluggishly become less valued. Systematic review of the various remedies and programs previously attempted in this area should be valuable for design of future programs and strategies, e.g., dissemination of information, training programs, subsidies, etc. Studies of factor mobility in the service sector, such as those recommended above, will be especially relevant in this context.

The initial FY '74 funding figure is for fundamental research to lay the foundation for a wide range of exploratory programs in the following years.

Funding Levels	FY '74 $ millions
Analysis and implications of change in the socioeconomic sector	$2.0

7. COMMUNICATIONS AND TRANSPORTATION (B)

The Problem

Modern broad-band communication facilities have the potential to provide more, better and more diversified services to households and businesses. Microwave, satellite and cable systems linked to computers can transmit high-speed data over many channels simultaneously to link centrally located storehouses of information with many outlets. Broad-band systems also open up new possibilities to individuals or groups who wish to experiment in television art forms or simply to air their concerns or promote a cause. Although the technology for such systems is available, neither the potential demand for access nor the probable viewer response is known. Because investment costs in mass producing broad-band communications are so high and operating costs, characteristics and social acceptability or need unclear, a substantial and successful demonstration experiment is probably required. RANN is at present funding a research program on the effective uses of communications technology which gives particular attention to public uses with respect to the disadvantaged. This should be continued. But very little research has been conducted on the social and policy implications of wide-band telecommunications, particularly broad-band (cable) T.V. transmission. There is urgent need for such work to be performed before broad-band systems are introduced extensively.

In the field of transportation, the public need is simply not being properly met. Part of the failure is due to lack of coordination among the different modes of moving people and

goods. Another contributing factor is the traditional preoccupation with mobility within the metropolis to the neglect of that within the large region and small locality.

Research Objectives

Communications The objective should be to evaluate the policy options for alternative uses of channels on existing cable and broad-band television systems, investigate the barriers to developing new capacity for public service communication and explore methods for effectively utilizing such new capacity in the public interest. The ultimate goal is to improve not only public services but also the general quality of community life. The determination of the social impact of emerging possibilities is, therefore, a key factor in this work.

Transportation Research should chart the existing transportation system, identify new needs and examine alternative institutional arrangements with a view to providing information for designing a more responsive system.

Specific Research Strategies and Programs

Communications . . .

- *Policy research* Special emphasis should be given to the development and understanding of policy alternatives which affect local policy decisions on such matters as cable television franchising, the regional inter-connection of cable television systems, and the financing of local educational and civic programming.
- *Access to cable television channels* Pilot projects should test alternatives for the creation of markets in leased cable channels and to encourage the provision of new services.
- *Interconnection and sub-division of cable television networks* Pilot projects should be conducted to evaluate the effects of offering a wide range of market sizes and locations. Both the content of the services offered and the institutional arrangements for such experiments are important variables.
- *Interactive cable television* Experiments are needed to test cable television systems with the capability for real-time audience response by means of push-button response coupled to computers. This is a new communication medium which is likely to exist on a large scale within the next decade.
- *Patterns of information flow in cities* New communication technology is likely to alter significantly present patterns of information flow in our cities. In order to

realize the social benefits of this new technology, we need to present patterns of information distribution and the alternative ways in which these patterns can be altered.

One program has been separated out from the main research group because it involves both communications and transportation:

- *Communications/Transportation Interaction* Modern communication technologies, particularly broad-band cable television, provide opportunities for the use of communications either to enhance the effectiveness of or to replace certain transportation services. Studies and experiments are needed to investigate these opportunities and their potential benefits, especially to the emerging service economy.

Transportation policy The research recommended is almost exclusively policy-oriented.*

Three main areas have been defined:

- *Government and policy issues relative to metropolitanization and regionalization of transportation* The changing patterns of demand and distribution of origins and destinations require a consideration of interactions among transport capacities on a metropolitan and regional basis. Research should examine the competing jurisdictional interests, and the effectiveness of institutional arrangements.
- *Identification of transportation requirements* The varied transportation requirements of different sectors of the community need to be analyzed in order to provide appropriate services. The need for mobility within communities and relatively small neighborhood space has in particular been neglected in the past despite its significant implications for community coherence and development. Part of the existing RANN program such as the relationship between transportation and the access of minority populations to jobs would come within this research.
- *Matching capacity to local, urban and regional needs* The changing demands and needs for transportation services as outlined above effect a whole series of systems that are private as well as public, regulated as well as unregulated. All interact in largely unknown ways. Research should aim to delineate the need for regulation or deregulation, additions or consolidations, or development of alternative ways of providing equitable distribution of goods and services. Aside from research into the diffuse network of transport services, experimentation with alternative forms of services is needed.

*See recommendation No. 16 re "Energy Consumption and Conservation."

Funding Levels	FY '74 $ millions
Communications	4.0
Communications/Transportation Interaction	2.0
Transportation Policy	0.6
Total	$6.6

II. ENVIRONMENTAL QUALITY AND GROWTH

The Problem

The restoration and maintenance of an environment of high quality is a goal that has achieved widespread acceptance in our society. This has been evident in opinion polls conducted by reputable national organizations, by electoral results, by a long series of court decisions, by the passage of legislation, and by the involvement of many citizens in the environmental movement.

Questions remain, however, concerning the degree to which growth of different kinds is compatible or incompatible with the desired environmental quality. Some kinds of growth will tend to enhance environmental quality (e.g., growth in the development and application of technology to abate pollution, or growth in efficient and attractive systems of public transportation in urban areas, or growth in the use of recycled materials). Other kinds of growth will tend to make it more difficult, perhaps even impossible, to meet the desired goals (e.g., growth in population, growth in energy-intensive or material-intensive industry, or growth in size of urban areas).

Clearly, the pattern of growth is a key determinant in environmental policy options and, in turn, will be affected by certain government interventions. Yet up-to-date information on the relationship between growth and environmental factors is not available in a form which can be used by decision-makers. An example would be patterns of mobility with respect to environmental changes such as whether air pollution levels influence relocation decision by families and industries.

The conservation aspects of growth are explored more fully in the next group of program recommendations (Conservation of Energy, Materials and Land). The focus of the environmental quality programs described here is on filling the wide gaps in our knowledge about the impact of man and his products on the life support system.

Broad Research Considerations

A number of factors which should be borne in mind in setting up any research program on environmental quality and growth:

1. Neither the price mechanism nor any other existing institutional mechanism alone will operate automatically to keep potential limits to growth from being approached or exceeded. Externalities, uncertainties, ignorance and non-competitive market conditions are rampant in areas where environmental and resource commodities are concerned.
2. A partial substitute for the price mechanism may be found in policies based on comprehensive models. These must include physical, biological and behavioral relations so that all the relevant consequences of a change in growth patterns or policy with respect to the environment can be evaluated.
3. The few comprehensive models that have been built so far on an experimental basis inspire little confidence because so little is known about the responses of physical, biological and social systems to changes in the environment. We do not even know, for instance, what concentrations of effluents are present in many of our local environments, let alone how these concentrations change with changes in emissions.
4. Conflicts between regions and between countries for resources are likely to arise with increasing intensity. A diverse range of analyses at the regional, national and international levels is, therefore, required.
5. The kinds of problems we will face in the area of growth and the environment involve long lead times. By the time the necessity for a change in current direction is recognized, it may be too late, in many cases, to take effective political action. Conflicts between short-term "needs," such as economic growth to provide jobs and

distribute wealth, and long-term "imperatives," such as the avoidance of ecological breakdowns, complicate the search for solutions. Our thinking must project farther into the future than is normally done. This requirement places a premium on insight and imagination. In this regard, the current RANN program may be too "safe," i.e., overly devoted to projects that maximize the chances of a productive payoff in the conventional sense. Some of the research programs suggested below try to correct this natural bias.

In brief then, the broad research objective is to discover the likely long-term tradeoffs which must be factored into environmental policy and the means of maintaining a safe distance between pressures of growth and the limits of resource availability and the absorptive capacity of our environmental systems.

8. ENVIRONMENTAL EFFECTS OF ENERGY PRODUCTION (B)

The Problem

The production, conversion and transportation of energy are major causes of environmental degradation. About 80 percent of the degradation of air quality, for instance, is associated with the direct transformation of energy, according to responsible estimates. Yet the need for a healthy environment must be balanced against the demand for energy. Even individuals have difficulty assessing preferences between clean air and cheap energy. It is not surprising, therefore, that national energy policy is inconsistent and shifting.* Neither the market mechanism nor economic theory help the policy maker. Specification of an efficient strategy begs the question of the relative value of our resources; yet the very problem is how to value the resources involved.

*Present energy policy is an amalgamation of decisions by numerous agencies and groups with diverse criteria. The ICC, for instance, has promoted an energy-intensive transportation system without ever confronting its impact on national energy policy. See discussion of this under No. 17.

Research Objectives

Since we cannot predict the future value assigned to various resources, we cannot be certain which technologies will be "economical." This calls for a research strategy that, in the first instance, emphasizes diversity, providing a variety of strategies and assessments of their comparative advantages.

Specific Research Strategies and Programs

The following recommended areas for research do not fit in neat categories. They are important examples but are meant to be suggestive only:

- *Utilization of waste heat, especially from electric power generation* The waste heat from this source alone would be about enough to satisfy every space conditioning need in the country: about 17% of the national energy budget. To be sure, many of these waste heat sources are at inconvenient places, and/or the exhaust temperature is too low, for the heat to be utilized very well. But the benefit achieved by utilizing just a modest fraction of presently wasted exhaust heat would be great.
- *Environmental effects of energy extraction* Although much research is already underway, much more work of an integrative nature is needed on the problems of (a) coal mining, both underground and surface, and also in relation both to ecological and human damage. (b) Proposed technologies for extracting, processing, and disposing of oil shale deposits.
- *Major accidents in transportation of energy* Neither the supertanker nor LNG (Liquefied Natural Gas) tanker problems have been properly assessed in terms of engineered safeguards versus probability and cost of accidents.
- *Auto emissions* While standards have been established, it is by no means clear that they are societally optimum. For example, the social cost/benefits of adopting different technologies are unworked, lost in a flurry of effort to implement a set of existing decisions.
- *Epidemiological studies of the consequences of long-term exposure to low levels of pollutants, many of which come from the direct transformation of energy*
- *Environmental effects of industrial energy use* This area, comprising 40% of all energy use, is very much under-explored, even in terms of planning the work.

In addition to these examples, it is expected there will be other significant projects in this area which, for various reasons, will not be funded by EPA, DOT or other mission-oriented agencies.

Funding Level	FY '74 $millions
Environmental Effects of Energy Production	$4.5

9. ASSESSMENT OF ENVIRONMENTAL RESEARCH EFFORTS (B)

The Problem

Although millions are spent by diverse agencies on environmental research, not enough is known of the way these programs are managed, the priorities and projects selected, the results disseminated, or even whether the research products are fully evaluated. The lack of this knowledge makes it impossible to determine where national needs are not being fully met and where RANN can best contribute its efforts.

Research Objective

The objective should be to survey and analyze the existing research programs in this area to identify the gaps in research and leverage points for innovative pressures from an outside agency such as RANN. The ultimate goal is to achieve a better direction and balance for the total research effort. While such a review would help ensure that important national needs are being met, RANN must meanwhile carry out its research functions and set priorities for itself.

Specific Research Strategies and Programs

Review both intramural and extramural research efforts of governmental agencies, including NSF, in the environmental field. The funds needed for such an assessment are small and the study need not extend beyond two years.

Funding Level	FY '74 $ millions
Assessment of Environmental Research Efforts	$0.5

10. INSTITUTIONAL ARRANGEMENTS AND IMPLEMENTATION PROCESSES RELATED TO ENVIRONMENTAL POLICY (B)

The Problem

As population and urban growth exert increasing pressures on local ecosystems, the needs for integration of water, air and land waste disposal systems and land-use control for large areas will become increasingly urgent. This will make increasingly intolerable the mismatch between existing institutional arrangements and implementation processes of state and local jurisdictions. It is becoming clear from some of the regional environmental studies that proper environmental management will require new, regionally oriented institutions with responsibilities and powers quite different from those in existence today.

Research Objective

Research should, on the basis of the knowledge developed concerning existing institutional arrangements and their implicit constraints as they affect the environment, develop and evaluate possible alternative arrangements.

Specific Research Strategies and Programs

- *Institutional and policy mechanisms* Evaluate existing institutional arrangements and develop alternatives which might better respond to the needs of the future. Because research in this area which is reasonably rigorous (especially quantitative) is new and rare, small selective programs might be preferable in the initial stages.
- *Social experimentation* Among the methodologies that might be applied to this problem area is social experimentation. Information is required not only on the way people have behaved when faced with different environmental circumstances but on the way they are likely to behave when faced with new situations in the future, in particular, new policy and institutional regimes. Opportunities to gather such information may be provided by working closely with local authorities in the process of effective policy changes. The methodology is one that is being used effectively in areas as diverse as the guaranteed income, education and birth control. In this particular area, an obvious and important candidate is experimentation with various ways and means of implementing the effluent charge approach to environmental management.

Funding Level	FY '74 $ millions
Institutional arrangements and implementation processes related to environmental policy	$2.0

11. RESEARCH ON THE BIOLOGICAL AND PHYSICAL ENVIRONMENT (B)

The Problem

Although a good deal of work is being done on free-standing models of individual, biological and physical processes influenced by the environment, data sources still remain poor. Lacking, for instance, is accurate information on the flows and concentrations in a number of environments of many potentially harmful elements and compounds. Little is known, for instance, about the pharmaceutical, viral and bacteriological content of the effluents poured daily into municipal sewage systems from homes, hospitals and pharmaceutical concerns and less yet about the final destination of these materials in food chains of concern to man.

Neither have the pollution concentration levels of "inner environments" (i.e., public rooms, homes and work places) received sufficient attention. Mercury levels in rooms painted with latex paint, for example, may exceed safe levels after long periods of time. Large quantities of synthetic organic chemicals are also emitted in high density areas by small businesses and dry cleaning establishments. Little is known about their sources and destinations.

At present there is a lack of adequate measuring devices and continuous monitoring procedures to gather data on such emissions and effluents.

Research Objective

The objective is to obtain a better understanding of biological and physical processes in connection with the introduction of certain materials to the environment in order to identify environmental hazards. Research should also develop methods of removing these materials.

Specific Research Strategies and Programs

- *Source-destination-concentration studies** There is an imperative need for a systematic review and evaluation to determine which potentially harmful elements should be the object of priority research and to close the more critical data gaps.
- *Instrumentation and monitoring* The search here should be for more economical measuring devices and monitoring procedures. Current methods for detecting polychlorinated biphenols, for example, are time consuming and expensive. One promising area is multisubstance analysis through use of spectroscopy or spinflip laser spectroscopy.
- *Processes for removal of pathogens, heavy metals and nutrients from waste streams* Low level, exploratory work in this field should be sufficient in FY 1974.
- *Recycling and recovery technology* An exploratory study, which includes economists and industrial engineers, should be initiated to determine whether funding of technological development in this area is warranted, and if not, what institutional changes are necessary to induce the proper amount of research expenditures on the part of private business.

Funding Levels	FY '74 $ millions
Research on the biological and physical environment	$8.0

12. AGRICULTURE AND LIGHTLY MANAGED ECOSYSTEMS (X)

The Problem

Present agricultural practices result in: substantial damage to wildlife; the entry of pesticides and other agricultural chemicals into ground water, streams, and rivers; and increased vulnerability to catastrophic crop losses arising from specialization of gene types ("monoculture"). Also, problems have arisen in the management of recreational forest and lake areas and regional grassland systems.

Research Objective

To study, on a regional basis, alternative methods of management of agricultural lands and lightly-managed ecosystems, so as

*The present RANN trace contaminants program would form part of these studies.

to understand better how to restore and maintain diversity, stability, wildlife populations, high-quality water supplies, and other desired attributes of such systems. Development of effective non-chemical methods of pest management is one important objective of this work.

Specific Research Strategies and Programs

- *Regional environmental effects of the leakage of nonpesticide agricultural chemicals into ground and surface waters* This should be an assessment of the nature and magnitude of the problem and elucidation of the processes involved. RANN should not become involved in supporting the development of corrective measures.
- *Intensively-managed monocultures* Research on long-term stability of intensely managed monocultures will be difficult but reliable knowledge is essential as a guide to agricultural policy. Since quality proposals which go beyond academic speculation will be difficult to obtain, expenditures in this area should be limited to planning grants for the time being.
- *Management of recreational forest and lake areas and regional grassland systems* This research should investigate the ecological impacts of alternative management strategies and the physical, social and legal processes influencing land use and allocation in rapidly urbanizing rural areas and coastal zones with a view toward minimizing environmental deterioration.

Funding Levels	FY '74 $ millions
Agriculture and lightly-managed ecosystems	$5.0

13. RESEARCH ON THE SOCIAL AND ECONOMIC ENVIRONMENT (X)

The Problem

We know very little about the social and economic costs of environmental changes, especially about those costs that are small in the short run but potentially large in the long run. Information on the way people behave with regard to the environment is vital for the determination of these costs and the establishment of policy.

Research Objective

Among broad research program areas, the emphasis should be shifted from comprehensive model building exercises and physical and biological relations to the estimation of relationships pertaining to social and economic aspects of growth and the environment.

Specific Research Strategies and Programs

- *Mobility studies* No one is likely to consider air pollution levels in deciding whether to migrate to or from the United States, but an increasing number of persons and industries are likely to consider such levels in deciding whether to locate in such areas as the Los Angeles basin. Analysis is needed relating mobility to changes in the quality of the environment.
- *Consumer and producer responses to new environmental policy* Research in this area could test likely responses, for instance, to policies aimed at reducing consumption of environmentally deleterious commodities and to effluent charges and other devices for internalizing external costs, economic costs and benefits of changes in environmental quality, and sensitivity analysis to determine, e.g., what it takes to induce use of substitutes for environmentally detrimental products (social field experiments may be useful in this area and warrant consideration).
- *Sample survey panels* A feasibility study in depth should be funded on the development and use of sample survey panels to measure behavioral responses to policy and environmental changes over time.*

Funding Levels	FY '74 $ millions
Research on the social and economic environment	$2.5

14. DECISION-RELEVANT ANALYSES OF ENVIRONMENTAL SYSTEMS (X)

The Problem

The tools for analyzing the long-term consequences of alternative patterns of growth are inadequate. Since population, per capita income, urbanization, etc., will continue to experience substantial growth in the United States for the balance of this

* Such a panel using households as principal units of observation could provide micro-economic and sociological information of benefit to all the social as well as the medical sciences.

century, such analysis will be essential if new developments are not to overtake us before it is too late to take effective political action.

Research Objective

Improve the capability for providing credible, timely, policy-relevant information to decision-makers. In part, this program represents a continuation of RANN's present regional environmental systems studies with some re-direction to make such research more policy-relevant.*

Specific Research Strategies and Programs

- *Empirically-oriented, interdisciplinary model-building for specific regions*
- *Benefit-cost analysis*
- *Development of better signaling devices* Research should establish just how much reliance should be put on the price system as a device to provide timely information on long term resource adequacy questions; can its reliability be improved; and where it cannot, can some other system or technique provide continual monitoring, domestically and internationally, of consumption and production trends and the changing possibilities for technological substitution. Simple planning projects in this area should go ahead immediately.

A good bit of RANN's regional environmental systems program, in particular the part involving computer simulation work, falls in this category. It should be reviewed with the above criteria in mind and funding continued at approximately current levels at least until this review is completed.

*Projects in these areas should meet the following criteria:

(a) Any computer or simulation models developed should be goal or problem-oriented in the sense of social benefit-cost analysis; that is, the consequences of alternative actions that have relevance for policy makers must be included.
(b) Emphasis should be placed on the development of empirically sound relationships rather than on model-building implemented with illustrative relationships. Where any dubious quantitative relations must be used to complete the implementation of the model the user should be made fully aware of such limitations.
(c) In choosing the geographic focus of the efforts, whether it be on coastal regions, lakes, or administrative units such as states, high priority should be given to units about which environmental or economic policy decisions are relevant. This may mean, for example, more emphasis on urban areas and less on isolated lake or desert regions.

Funding Levels	FY '74 $ millions
Decision-relevant analyses of environmental systems	$10.0

III. CONSERVATION OF ENERGY, MATERIALS, AND LAND

On almost any set of assumptions about national policy, the United States can be expected to experience substantial growth in both population and real output per capita over the next 25 to 50 years.

In more concrete terms, the demographic composition of the present population suggests that about 1.5 million new households will be created each year during the mid 1970's and early 80's, the rate then slackening somewhat. The labor force will increase by some 3.5 million *per annum* during the 70's, dropping back to roughly a 2.8 million *per annum* level after 1980. This growing population and labor force can be expected to be more heavily concentrated in urban areas; by the year 2000, 85 percent of the population will be living in cities, an increase of roughly 50 percent over today. Problems of housing, urban blight and unemployment will clearly be with us for some time to come. While a substantial increase in real output is insufficient by itself to resolve these problems, most social scientists believe it is a necessary condition. In any case, assuming trend increases in productivity and full employment, real output will at least double, and quite possibly treble by the year 2000.

This growth will not occur without costs to the environment and the scarcity of land, materials and energy. While the rate of increase in raw materials and energy consumption may be constrained by economic, logistic and political factors, internationally and domestically, there is no reason to suppose that the absolute amounts of these resources will impose a fixed limit on growth during this time period. But the ecological disruptions involved in acquiring, transporting and ultimately disposing of these resources are likely to impose serious pressure on natural and social environmental systems. Indeed, pressures on these systems are likely to rise more rapidly than increases in real output.

Although technological innovation can mitigate these pressures, they can still affect human welfare directly and, if large enough, can seriously restrict growth itself.

We should not think of such limits to growth in general as being fixed broad research considerations. Technological and institutional changes have pushed back potential limits in the past and will continue to do so, to an extent not entirely predictable in the future. A useful way to view the situation is as a competition between growth rates–between the rate at which technological and institutional advances push out the limits to growth and the rate at which increases in population and production move us towards them.

Quite obviously, we should attempt to maintain a safe distance between pressures and limits, by pushing out the limits and by slowing down the growth in pressures on the limits. But it is equally obvious that attempts to maximize this distance by restrictions on population and economic growth that are too hasty, coercive or inequitably administered could lead to serious social disruptions. What then, is the appropriate distance, and how can we achieve enough control over our destiny to maintain that distance? Which of the various potential limits are most threatening in the short, medium and long terms? What specific actions can society take to avert or accommodate itself to these threats? Should these actions include changes in life style which make it possible to live without growth? These are the basic questions about growth and the environment which underlie the selection of research areas on environmental quality.

15. CONSERVATION AND MANAGEMENT OF MATERIALS AND LAND (A)

The Problem

Among all the substantive programs recommended, research in this area and in energy conservation were ranked highest because of the urgency of the problems and the potential for the development of more appropriate policies, programs and technologies. We have not answered such vital questions, for example,

as given certain actions in the interim what energy supply will be available in the future at what cost? Further, as the population of the country increases, cities spread, and new agricultural and mining practices evolve, how will pressures on the resource of land develop, and patterns of land use change?

Research Objective

Initial RANN support in the materials area should be focused on finding ways of reducing our demands for irreplaceable natural resources by recycling or by use of substitute materials. The emphasis should, in general, *not* be on new technologies to achieve these results, since other agencies are equipped to work with industry to promote such technologies. Instead, RANN should strive to identify situations in which new techniques would be likely to pay off, to identify industrial incentives for adopting them, to foster public acceptance of substitutes, to assess new markets here and abroad, and so forth.

On a land-use policy, the consideration will be complex and will impinge strongly on other non-land-use matters (e.g., whether to allocate a large area of potential grazing land to solar energy generation, whether to restrict the spread of urban housing and to build more high-rise buildings, whether it is necessary to authorize extensive strip mining of coal or to import it from abroad). Study and analysis of these interactions is needed. To make such complex decisions rationally, it will be necessary to create the required analytical tools and resources.

Specific Research Strategies and Programs

Conservation of Materials

- *Sources, routes and sinks* A raw material is gathered, refined, processed and ends up as a useful product—a car or stove or coat or something to eat. Eventually it is outmoded, worn out, or eaten, resulting in waste of some sort. Disposing of this waste is one of the major problems of our cities, whether it be old car bodies or sludge from a sewer plant.

  Projects would be devoted to delineating, in aggregated terms, how much of each raw material is used in each production process, where it goes, how long it remains in the system, and where it finally ends up. Such an effort would produce an invaluable data base that could serve others studying the flow-through of

materials in our society, and could be used to flag areas in which intervention would most likely be profitable by recycling material.

- *Biological waste conversion* One vast source of waste is a sewage treatment plant, where sludge is one product and water (still containing some dissolved and suspended solids) is the other. The purpose of this project would be to identify the possible uses of such biological "wastes" as raw materials for conversion into foodstuffs, topsoil, and other useful materials. All forms of such conversion should be studied, their relative merits evaluated.

  A first step toward the goal would be to select research organizations which have already made some progress in this work and to compile a state-of-the-art report. Then RANN would arrange to analyze the basic problems involved, such as the possible need to remove heavy metals from biological sludges and to design alternative pilot processes to accomplish this removal. The next step would be to test the alternative pilot processes in laboratories, select the more promising ones, and proceed to the pilot plant stage. Throughout this process, RANN should continuously report its findings to industry in an attempt to interest a profit-maker to take over the work at the earliest possible stage.

- *Life cycles of products* A producer of a piece of hardware, once the warranty period is passed (if there is a warranty at all), is no longer concerned with what happens to the product–except for the effect on his reputation if it fails too often. In almost no case is he responsible for the final disposal of the piece. In only a very limited number of cases is he interested in retrieving it for re-use.

  RANN should explore the possibility of making the producer responsible for the disposal of his product. This involves many questions. If he were responsible, how would he change his product? Would it have longer life? Would it be more convenient to salvage? Would he have chosen different materials? What would be the incentives to industry for such a major recycling practice–would it result in a more satisfactory product as seen by the consumer? Since industry is unlikely to initiate such a move, RANN should investigate the various considerations, with the idea of seeking incentives for such a change in practice if it seems to achieve the goal of conserving materials.

Conservation of Land

- *Patterns of land use* RANN should assure the existence of a comprehensive data bank from which essential inputs to policy decisions can be drawn, such as the character of the land in question, the history of its past use, the economic and demographic factors that must be considered for the region, and a knowledge of the pertinent environmental and ecological interrelationships. Much of this data may already exist, though in dispersed places.
  Concurrent with the assembling of the data bank, land use models should be developed to provide at least a crude ability to forecast the effects of policy, e.g., how patterns change, given a scenario for population growth, resource exploitation, and government intervention. Current land use reflects the history of the land rather than any optimum pattern.

- *Recycling of land/multi-purpose use of land* Individual farmers or industrialists have little incentive to take a very long-range view of land use, with the possible

exception of parts of the lumber industry. However, it may be advisable to cycle land in and out of useful production on a time scale much longer than that of ordinary crop rotation. In addition, there may be novel ways of using land. Examples would be the use of sewage sludge from cities to fertilize and enrich large areas of farmland; or, in quite a different vein, setting aside large areas for solar power collection but retaining other uses for the same land. Multipurpose use of land for waste-water reclamation, crop production, waste-heat conversion, and isolation of generating stations is an example of land conservation by making one acre do the work of several.

- *Management of the use of underground space* A process is needed for deciding the most productive use of underground space, particularly for cities. The suggested approach would be to start with an inventory of location and extent of suitable rock formations under each major U.S. city. A logic should be developed for the selection of functions which could better be located underground than on the surface. A logic for allocating space in three dimensions for each of the selected functions would follow. The goal would be to make recommendations to urban regional planning commissions, the federal government, and to industry concerning this presently under-exploited resource.

Funding Levels	FY '74 $ millions
Conservation and Management of Materials and Land	$7.0

16. ENERGY CONSUMPTION AND CONSERVATION (A)

The Problem

Energy is exclusively an intermediate good. Its work and heat contribute to our well-being. As the price of energy rises, and shortages and even rationing occur in some areas, it behooves us to concentrate on the demand as well as the supply side of the problem. We may be able to achieve the same level of well-being with substantially less energy. This possibility is suggested by the frequently energy-intensive (in most cases a by-product of subordination to other criteria) design of buildings, cities, household appliances, transportation modes and other facilities. In addition, many of the present economic incentives encourage consumption of energy for wasteful or marginal uses.

Research Objective

The objective is to learn more about how energy is being used and study alternative and less energy-intensive ways of carrying out the necessary and desirable functions in our society. This will require interdependent research approaches. For example, energy demand studies require not just economic analysis, but the engineering capability to assess the present and potential ability to find substitutions for the more energy-intensive technologies; studies in energy-conserving residential construction and design require both engineering and architectural expertise, and also the economic and legal skills to draft suitable building codes and to estimate the impact of the increased cost on home buyers.

Specific Research Strategies and Programs

- *Patterns of energy use in alternative transportation systems* Transportation uses 25 percent of the nation's energy. Some work in this area is being pursued by DOT, but more is required, perhaps in collaboration with DOT.
- *Energy consumption in residential and commercial structures* The construction industry, characterized by small and under-capitalized builders, does no research in how to minimize energy use in building design.
- *Industrial use of energy (by process)* Industry accounted for 37.2 percent of the nation's energy consumed in 1970. Much of this use is concentrated in the primary metal and chemical industries, each of which uses 7 percent of the national total. Particular industrial processes should be examined to determine possible substitute technologies that would be less energy-intensive.*
- *Regulation of energy utilities* Traditionally, regulation has concerned itself with price and convenience criteria only. Research is needed on energy effects of regulation with particular attention to the influence of alternative rate structures and peak power metering and pricing.
- *Alternative urban designs* The energy implications of alternative urban designs should be probed as a system incorporating all factors such as spatial distribution, transportation modes, construction materials and design, etc.†

Funding Levels	FY '74 $ millions
Energy Consumption and Conservation	$8.0

*See recommendation No. 20 on Enzyme Technology for related research.
†See recommendation No. 5 for related research.

17. INSTITUTIONAL AND REGULATORY SYSTEMS AS THEY AFFECT ENERGY (B)

The Problem

Many policy decisions (for example, to keep well-head prices of natural gas low or to charge higher freight rates for the transportation of scrap metal than for ore) result in actions which are undesirable from the point of view of energy conservation. In particular, we have little knowledge of how the action of one regulatory body affects the decisions of another, and coordination is only possible if the interrelationships are understood and specified.

Research Objective

The objective is to assess the side effects and long range consequences of alternative institutional designs and regulatory policies, particularly to provide policy makers with the option of considering energy-conserving factors.

Specific Research Strategies and Programs

- *Data base* The main recommendation in this area is for a large integrated data base on energy flows through the economy. This is required if we want to begin to ask how various energy policies will affect economic growth. Only in this way can we get a handle on how various industries will react and on the comparative impacts of changes in the supply and use of energy. We would expect the collection projects to continue for a number of years, and a custodial function of data maintenance and updating to last indefinitely.
- *Resource availability* There should be three studies on resource availability: one each for coal, oil and gas, to estimate available reserves at various price levels. Such information does not exist. Again, the relative magnitude of expenditures does not indicate the importance of the project, but our estimate of the relative difficulty of obtaining information.
- *International impact of energy* This is one of the most overlooked aspects of the energy problem. The specific research suggested involves the disposal of radioactive waste, and transport of fuels, especially supertankers. Only $500,000 for FY '74 is recommended for this work. But more researchable issues and problems will be recognized as this progresses. It would be hard to overestimate the amount of good relevant research that could be done in this area.
- *Effects of regulatory activities* Numerous agencies, courts, and other public bodies make decisions that affect the supply and use of energy. A number of these are not primarily concerned with energy (for example, the Interstate

Commerce Commission) and may not realize the energy implications of their decisions, such as the imposition of higher freight rates on scrap iron than raw iron ore. A series of explicit studies on the impact of various regulatory decisions is needed. To a major extent, national energy policy is the result of many small decisions by these agencies, and the agencies themselves may be unaware of their role in the creation of such policy. The research recommended should clarify the impact of their decisions and the relationships among the various groups with a view to achieving more explicit recognition of energy conservation factors.

- *Economic welfare as a function of energy growth* A major question for public policy is how much energy growth is necessary to assure economic well-being. A number of the projects specified above help to answer this question. A small amount of money should be provided for research that would specify further subsidiary questions that are researchable and policy relevant in this area.
- *Environmental goals and attitudes* A national survey on attitudes toward the environment and energy would be useful. The structures and correlates of public attitudes on these issues have been both taken for granted, and not researched. It is appropriate for some research to be undertaken at this point to understand the underlying dynamics of public opinion on these issues. Legislators and regulatory agencies, in response to the developing ferment in the energy field, would be receptive to better information on the energy consequences of their decisions than is now available; thus research of this type should have considerable leverage.

Funding Levels	FY '74 $ millions
Institutional and regulatory systems as they affect energy	$5.25

IV. NEW PRODUCTION PROCESSES

The Problem

This area groups together a variety of programs which did not fit neatly under any of the preceding rubrics. The major one deals with concern over productivity, reflecting awareness of the need to reduce the cost of goods in order to meet foreign competition and the need for goods in the United States.

Broad Research Considerations

The process and management of innovation and diffusion of technology is key. Two recommendations concern substantive industrial processes in two areas where research gives promise of encouraging significant breakthroughs: solar energy, which was

rated very high even though the broad area of energy provision in which it was grouped fell in the X category, and enzyme technology.

18. INDUSTRIAL PROCESSES AND MANUFACTURING (A)

The Problem

Despite a generalized national awareness that the United States faces a problem of productivity, its full extent and ramifications are not known. Some of its manifestations are:

- problems in the management of innovation
- impediments to market aggregation
- rigidities in industry reaction to technological innovation
- anomie and worker dissatisfaction
- increased ability of foreign producers to match United States performance, even in "high technology" areas
- the failure to reduce production turn-around time in order to exploit delivery time advantages for domestic markets.

Research Objective

A better understanding of industry practices and policy with a view toward increasing productivity.

Specific Research Strategies and Programs

The first projects undertaken should be:*

- *Analysis of the interaction of management and technical innovation*
- *Study of the effects on anomie and worker dissatisfaction* of new fabrication technologies and new varieties of assembly line design
- *Investigation of determinants of long turn-around time.*

The following projects are some of the programmatic attacks:

- *Management of innovation: System response of manufacturing systems to technological process change* Innovation and diffusion of technological process

*These problems are best studied in the context of specific operating settings.

change have created much of our past rise in perceived productivity. A better understanding of these systems may permit a more focused use of government incentives or informal action by business executives to raise productivity. Some of the questions needing study include: (a) the impact of changed tax policy on the adoption of new processes; (b) the impact of government-supported R&D on industrial innovation rate; and (c) the impact of demonstration projects on the new-process diffusion rate.

Market Aggregation Strategies

- *Technology gaps in intermediate product/processes* A large proportion of the technological innovation in processes that takes place in this country arises from the spread of new processes by suppliers of tools, materials, and methods used in the manufacture of goods. These intermediate goods suppliers are frequently fragmented or are faced with fragmented markets which prevent an effective level of investment in the product development. In those foreign governments where the government has taken a role in the aggregation of either the supplier or the supplier's market, such investment by industry is considerably higher. The U.S. supplier/user system for intermediate products should be examined to determine those characteristics that affect the diffusion rate.
- *Intermediate product R&D* Many significant developments in intermediate products are beyond the scope of aggregated supplies. Such general problems as the development of job shop information systems that will support automated factories are non-specific to a particular supplier or industrial group and should be undertaken by a broad systems study.

Industry Reaction Strategies

- *Inter-industry comparison of technological reaction* The reaction of segments of our industry to technological change has varied widely. Some of this variation can be explained in terms of demand and of classical economic theory, but much of it cannot. Very valuable data comes from industry-to-industry comparisons. The work of manufacturing policy researchers should be combined with the work of capable psychologists and technologists in order that data from one industry can be used to disclose possible structural alterations in other industries that would render them more amenable to technological change.
- *Industry response to worker dissatisfaction* Industry responses to expressions of worker dissatisfaction have ranged from relocation to labor displacement to social experimentation. Needed is a program (parallel to the industry-specific program on technological change) dealing with inter-industry comparisons of responses to worker dissatisfaction. It is likely that many common characteristics will be identified and that many of the structural modifications suggested by one study will appear relevant to the other.
- *Anomie and worker dissatisfaction* Experiments indicate that worker dissatisfaction is very subtly related to the technology of the tasks being performed and the "range" of the individual worker's task. Little systematic study has been done in this area. Because it is a high pay-off problem which cuts across agency lines, RANN appropriately should undertake funding of investigations in this area on an urgent basis.

Appreciation of New Technological Approaches

- *Automation and anomie* Contrary to conventional wisdom, recent experience in industry indicates that worker dissatisfaction does not necessarily increase with increased level of automation, but that rather, the converse is often true. Automation makes range changes possible and frequently can increase the worker's identification with the product. RANN should start a program in this area.
- *Assembly line re-design* RANN is the appropriate agency to sponsor actual demonstration/engineering projects on worker involvement with assembly line re-design. This work must be done in collaboration with industry.
- *Foreign ability to catch up: Cross-industry study of the process life cycle* While solid data is currently available for the petro-chemical industry and for a few of the other commodity industries, no one has done a systematic study of the process life cycle as a function of time for various industries upon which the economic health of the United States depends. This study should be undertaken in order that the characteristics of technological change be identified as a function of the rate at which such change is copied by foreign competitors.*
- *Turn-around time reduction* A major investigation is needed into computer-aided manufacturing. Under this rubric is included the rapid emergence in this country of computer-aided job shops in which orders are processed by computers that act directly on the set-up and operation of various cutting and assembly tools. Often, it is the turn-around time which affects the ability to compete with foreign competitors.

Funding Levels	FY '74 $ millions
Industrial Processes and Manufacturing	$5.0

19. ENERGY PROVISION (X)†

The Problem

Because of limits to the supplies of certain fuels and unacceptable environmental impacts arising from a number of current technologies, energy supplies from current sources will fall short of demand not very far into the future, if demand increases at projected rates.

*This study should not be restricted to the United States, but should explore the nature of foreign competitors and the role of foreign governments and extra-industrial forces (such as the educational institutions) in foreign producers' copying behavior.

†See also Recommendation No. 5.

Research Objective

RANN can speed the development of new, more environmentally acceptable technologies for the provision of energy by funding carefully selected research in areas where market and mission forces appear to be inadequate to bring about desired developments. RANN support for applied research to improve prospects for the practical utilization of solar energy seems particularly attractive.*

Specific Research Strategies and Programs†

- *Energy resources* Development of solar and geothermal energy as well as energy from other unconventional sources.
- *Energy conversion* More efficient and environmentally acceptable methods of conversion of fuels now in common use (coal conversion technology and fuel cells).
- *New ways of transporting energy* Research is needed on electric transmission and control systems and synthetic fuels.

Funding Levels	FY '74 $ millions
Energy Provision	$11.3

*Solar energy and initial research into other unconventional power sources as distinct from the rest was rated very highly.

†Since any RANN investment would represent only a very small fraction of the R&D expenditures on energy provision in the U.S., the need for selectivity is obvious. Leverage is a particular criterion in this area.

In view of the feeling of the Workshop that the most important and effective role to be served by RANN in the energy field was research on energy consumption and conservation, the environmental effects of energy production, and institutional, regulatory, policy, etc., aspects of energy, the category of energy provision as a whole received a low priority rating. However, certain elements of the proposed energy provision research should be given special consideration because of their national importance and the inadequacy of present market and mission forces which might support such research. Research on solar energy is the paramount example of this. Research on control systems and power transmission should be given relatively less attention because of the likelihood that it can be adequately supported from private sources of funds.

Assessment of the side effects and long range consequences of alternative developments in the energy field should receive high priority attention from the RANN program.

20. ENZYME TECHNOLOGY (X)

The Problem

There are many new industrial processes which could be developed if the state of the art of enzyme technology were further along. The RANN description of this effort comments:

"The use of enzymes to break down various molecules is well established, and new technology such as immobilization that permits reuse is making enzymatic processing more economically attractive. A rearrangement using an enzyme to convert glucose to the very sweet fructose should lead to the production of a sweeting mixture using starch or perhaps even cellulose as the raw material. These spontaneous enzymatic reactions take place at moderate temperatures and pressures and with high specificity to give few by-products. In comparison, conventional chemical reactions schemes produce polluting by-products and often present considerable waste heat to the environment. Until now, the use of enzymes to carry out synthetic steps could be carried out only on a very small scale because non-spontaneous processes must be coupled to and driven by energetic reactions such as ATP going to ADP. There are good indications that ATP can be regenerated economically so that a great many enzymatic syntheses of interesting biochemicals now may be feasible. As this research reaches fruition, enzymatic synthesis is likely to become the dominant portion of the Enzyme Technology program. The chances are excellent that this program will be able to demonstrate a very favorable return on the funds invested in research and development."

The COPEP steering committee agreed with RANN's assessment of the promise of this work, but felt that this program could better be in another area of NSF or could be picked up by industry.

Research Objective

The objectives of the current RANN program are to:

- provide enzymes of reasonable costs in sufficient purity
- obtain enzyme-like activity through organic chemical synthesis (a break-through in this area would cause a major change in research emphasis)
- test and develop new processes using enzymes in mass production and with analytical devices
- develop practical processes for driving non-spontaneous enzymatic reactions which will lead to novel synthesis and an entirely new type of unit operation.

Funding Level	FY '74 $ millions
Enzyme Technology	$2.45

21. EXCAVATION AND TUNNELING TECHNOLOGY (X)

The Problem

The cost of underground excavation and tunneling is generally high and often uncertain at the time bids are taken. This cost in turn reduces or curtails the use of underground construction or excavation for various purposes including urban developments or systems and the development of new or cheaper mining techniques without environmental impact. Lower costs and/or faster underground construction techniques could lead to conservation of both land and materials. The current program and the ongoing program discussed herein excludes the use of nuclear explosives (Atomic Energy Commission) and the general use of mining (Bureau of Mines).

Research Objective

Reduce the cost of underground excavation and tunneling in order to expand land resources, reduce costs and reduce environmental impact of mining and underground construction.

Specific Research Strategies and Programs

- *Geophysical exploration methods* Better location of tunnels and methods to provide knowledge of what will be encountered ahead of the face of the cut during the excavation process.
- Improved methods for soft rock tunnelling.
- On-site instrumentation of tunnels, under construction and in-use, in order to provide engineering data that will lead to improvements in current tunneling technology and design.
- Systems studies of the entire excavation process, including the economic and societal factors.
- Improved methods of advancing through hard rock.

The current RANN program on underground excavation and tunneling technology should be continued for several years with the objective of reducing the cost of underground construction which would in turn result in various national advantages.

Special emphasis should be placed on the development of new technologies having the potential to decrease the cost of underground excavation and tunneling in both soft and hard rock and to increase the speed of such excavation. This research will include a small number of major efforts on the most promising new methods that may be exploited for disintegrating and drilling through hard rock such as water jets, rock melting, and advanced technologies employing laser and electron beams, and also a number of projects related to soft rock. Particular emphasis on soft rock is noted since the problems of excavation through soft rock are more significant for many cities and that cost of such excavation, at present, is substantially greater than that for hard rock. Federally supported research in this field was previously co-ordinated by the former Interagency Committee on Excavation Technology of the Federal Council on Science and Technology.

V. NATURAL HAZARDS AND DISASTERS

The Problem

Floods, earthquakes and a range of other disasters cost the nation on the order of $10 billion and thousands of lives annually. Yet research for hazard related problems is inadequate and seldom is much needed scientific and engineering research output integrated with efforts to increase understanding of the basic social, economic and political processes which determine the nation's response to hazards during the planning era before an occurrence, during the occurrence, and in its aftermath.

Four basic functions in connection with adjustment to hazards and disasters are currently performed inadequately:

- *Monitoring* In the absence of a systematic method of monitoring, there is no adequate data base upon which to make significant policy decisions.
- *Dissemination* Many solutions (adjustment mechanisms) have been developed or are available but are not being utilized. This may be due to the possibility that

local policy makers and administrators on the one hand and scientific-technical personnel on the other see the disaster-hazard problems in quite different terms. Until the scientists and engineers understand the needs of the user as he sees them they are not likely to produce many solutions that will in fact be used.

- *Effectiveness assessment* Without research, prototype demonstration and pilot projects, state and local officials may lack confidence in scientific and engineering solutions.
- *Decision-making and adjustment to natural hazards* One of the striking aspects of technological efforts to cope with hazards in nature is that the well-intentioned program often yields results contrary to those desired. For example, the partial protection offered by a dam apparently gives residents a false sense of security and thus may promote intensive development of vulnerable lower sections of the flood plain. When a rare flood exceeds the capacity of a levee, damage is extensive.

Research Objective

To reduce the social and economic losses to the nation due to natural hazards by a better understanding of these losses, by examining the function of monitoring, dissemination, assessment and decision-making, and by increased knowledge of the response of individuals and organizations to hazards.

22. OPTIMIZING THE NATION'S ADJUSTMENT TO NATURAL HAZARDS (A)

The Problem

The term "Natural Hazards and Disasters" connotes an extremely broad range of phenomena to which an even larger range of potential human responses and adjustments are applicable. A significant portion of losses due to hazards and disasters occur in their aftermath and could be averted.

Research Objective

In order to approach an optimal adjustment to the range of hazards, knowledge on four basic functions must be improved for adequate performance on a continuing basis: (1) monitoring the hazards and the array of adjustments; (2) specifying the unmet needs and utilizing the available scientific, technical and social

knowledge (the dissemination process); (3) assessment of the effectiveness of the adjustment mechanisms being used; and (4) decision making and adjustment to natural hazards. It should be noted that the many disciplines required for a long-term integrated study as is recommended here suggests the need for a possible hazards research organization or center.

Specific Research Strategies and Programs

- *Monitoring* studies are essential to:

  (a) develop and then utilize comprehensive, standardized measures for economic losses and social impacts since only when we have comparable indicators of the impact of each hazard on our society can we make intelligent decisions regarding the allocation of money and manpower for coping with the hazard;
  (b) develop and utilize standard measures for assessing costs and payoffs for the various adjustment mechanisms in order to estimate the relative utility of these mechanisms;
  (c) determine where in the nation improvements in hazard adjustment mechanism are most needed, and for this to develop a valid record of the geographical distribution for any given adjustment mechanism;
  (d) develop measures for documenting trends in hazard vulnerability, including population shifts and the rise and fall in popularity of certain adjustment mechanisms;
  (e) develop a "hazard history" for specific locales to which an individual can turn to inform himself of the frequency and magnitude of the full range of hazards to which a particular locale has been subjected. Undertaking to develop this register is a job of great magnitude and difficulty, and we recommend a *pilot project* to develop the register for a restricted number of locales in order to determine the feasibility of a broader program.

- *Dissemination* To significantly improve the dissemination function, there should be *two-way communication* and education which often requires that someone perform or assist in the *linking* function between the experts and the potential users. There would also be international exchanges of information where appropriate.
- *Assessment of effectiveness* Research, prototype demonstration and pilot projects can increase the *relevance* of scientific and engineering research output for hazard-related problems as experienced by policymakers and administrators at all levels by placing "what the users need" first. Included are:

  (a) research on the effectiveness of adjustment mechanisms designed to reduce the hazard impact potential (e.g., engineering works);
  (b) research on the effectiveness of adjustment mechanisms designed to reduce human vulnerability (e.g., detection and warning systems, wind proofing, land-use controls);
  (c) research on the effectiveness of mechanisms designed to insure adequate relief, reconstruction and compensation (including insurance). There appears

to be an interactive effect regarding the use of various adjustment mechanisms. Where one is widely used (e.g., flood control works) other relevant adjustments seem to be neglected (e.g., land-use control and the flood proofing of structures). There appears then to be a need for the use of a systems perspective in the assessment of the effectiveness of adjustment mechanisms. We need to determine the full range of both the advantages and disadvantages of increasing use of any particular adjustment as it relates to the use of other adjustments.

- *Decision making and adjustment to natural hazards* Overall, we need to develop a technology for applying scientific methods and formal analysis to problems of decision making in the realm of natural hazards. Measures designed to increase understanding of decision processes and provide opportunities for improving it require a combination of theoretical, laboratory, and empirical approaches. Determining a rationale for optimal behavior in the face of a capricious nature requires theoretical development. The basic modes of assessing probabilities of rare natural events and of assigning values to consequences involve cognitive processes that may be discerned most clearly in controlled laboratory experiments. The recognition of ways in which cultural and situational factors may influence decisions calls for observation in field settings.

Funding Levels	FY '74 $ millions
Optimizing the Nation's Adjustment to Natural Hazards	$0.75

23. MANAGEMENT OF HAZARDS CAUSED BY SURFACE WATERS (B)

The Problem

Major and minor floods occur in all 50 states and, in recent years, have produced damage estimated at between $1 to $2 billion. These flood losses continue to mount despite the substantial efforts at flood control and prevention. Warning and modification (emergency dikes, etc.) of floods are useful in flood protection and control, but sound water management practices are essential to reduce the damaging consequences of flood hazards in a way which is economically efficient and socially justifiable.

Research Objective

Research programs should evaluate present knowledge, existing control mechanisms and develop new strategies and criteria for flood control.*

Specific Research Strategies and Programs

- *Extensively survey, analyze and critically evaluate Federal, state and municipal flood control projects*
- *Coordinate an interdisciplinary attack on the social, political, economic and environmental problems related to flood hazards*
- *Develop practical criteria and procedures for:* (a) designing flood control and flood fighting structures (b) implementing flood plain zoning and managing the flood plain (c) developing more useful and efficient schemes for flood insurance, and (d) improving the national flood control policy.

Funding Levels	FY '74 $ millions
Management of Hazards Caused by Surface Waters	$1.5

24. FIRE RESEARCH AND A NATIONAL FIRE-MODELING FACILITY (B)

The Problem

In the U.S. each year about 12,000 persons lose their lives due to fire, the highest per capita loss in the world, and at least $5

*Numerous research activities on flood hazards are being carried out by many universities and governmental agencies including the United States Geological Survey, the National Weather Service, and the Department of Housing and Urban Development. Practically all of these research activities are dealing with certain physical aspects of floods, and very little is done to apply this knowledge to mitigate the impact of floods. Accordingly, no new research is recommended for RANN to investigate the physical properties of floods; rather, research is recommended on how to apply knowledge currently available and on how societal mechanisms, such as flood plain zoning and flood insurance, can best be instituted to minimize losses. In applying the available research results, it is most important to coordinate and integrate the fragmented research efforts now being conducted by universities and government agencies.

Since floods are subject to great uncertainty, investigations should emphasize the use of modern stochastic analysis (in contrast to deterministic techniques) and other methods that incorporate the probabilistic nature of the phenomena.

billion worth of property destruction occurs, not including indirect and unidentified losses. Despite the magnitude of the loss due to fire, research undertaken into the problems of fire is miniscule. Of the approximate $36.5 million spent by NSF in disaster and hazard research during the 5-year period (1969-1973) less than one-eighth was expended on fire research. New approaches are necessary to some very old questions.

Research Objective

Understanding fire, and testing and planning a fire modeling facility.

Specific Research Strategies and Programs

- *Propagation velocity* The study should seek means of predicting a fire's propagation velocity over a combustible surface of specified characteristics.
- *Movement of fire* The study of fire in an enclosure or along a corridor by the combined action of natural convection and radiation.
- *Synergism between paired combustible sub-units* Which, when understood, would provide a technique for assessing the hazard due to the combustible contents of a room.
- *Measurement of the flammability of structures, solids, liquids, and fabrics* Recent progress on clothing burns has included the development of these methods which promise to permit assessment of burn injury to be expected from any given exposure.
- *Physiological effects of smoke, tar acids, heat and carbon monoxide and study of synergism among these*
- *Mechanism of decomposition of cellulose to produce tars* The effects of additives to cellulose (smoke and other pyrolysis products of cellulose decomposition cause more fire deaths than heat).
- *Testing and planning a fire-modeling facility* Starting first with a design study, the objective would be to construct and validate a small scale modeling tank, and then finally to construct a full scale National Fire-Modeling Facility located possibly at a federal laboratory. Since full scale testing with multiple replication of the experiment is the only way to gain knowledge about a combustible unit, such a fire-modeling facility is very necessary. Encouraging progress this past year on the use of a pressurized modeling tank make the concept of a large central model facility very important to pursue.

Programs were also recommended by the Natural Hazard and Disaster Panel in the two remaining fire problem categories of "fire department equipment and operations" and "transfer of information." These were considered more appropriately part of

the delivery services program and are discussed in that recommendation (# 1).

Funding Levels	FY '74 $ millions
Fire Research and a National Fire Modeling Facility	$3.25

25. EARTHQUAKE RESISTANCE, PREDICTION AND CONTROL (B)

The Problem

At least one great earthquake can be expected to occur somewhere in the United States before the end of this century. The losses could well amount to tens of billions of dollars in property damage and tens of thousands of lives. Lesser tremors can be expected more frequently. These also could be major disasters if metropolitan areas are involved. No major part of the country is without earthquake hazards of serious dimensions. The potential for occurrences is especially high in Alaska, California, Hawaii and Nevada.

Research Objective

To develop practical means for ameliorating the destructive effects of earthquakes, particularly improved design capability for earthquake resistant structures; long-term–greater knowledge of earthquakes to provide prediction and control.

*Specific Research Strategies and Programs**

- *Earthquake-resistant design code*† A comprehensive, up-to-date, dynamics-oriented design code should be developed, including but not limited to: dynamic loading and response, consideration of site and local soil conditions, influence of

*These recommendations represent considerable expansion of RANN's current program, with some new thrusts and slight redirection.

†This program is considered to have particularly high leverage and payoff at this time in view of the facts that existing codes are many years behind the state of knowledge and that new buildings continue to be built in seismic areas.

foundation types, structural dynamic analysis, soil-structure interaction effects, ductility and energy absorbing characteristics, provisions for design to best utilize the reserve energy properties of the structures, resistance of mechanical and electrical items, and probabilistic aspects of the problem.

- *Critical structures* Improved design criteria should be researched to ensure the continued functioning of critical structures whose services are especially necessary for public welfare in earthquake disasters. These structures would include, among others, hospitals, police and fire stations, transportation systems, utility and communication centers. Similarly improved design criteria should be researched for certain types of structures, such as nuclear power plants, dams, and highrise buildings whose failure would threaten the safety of large numbers of people. Although some research is now being conducted more is needed, directed at improved design of earthquake resistant structures of all types.
- *Expanded knowledge and improved understanding of earthquakes** Five interrelated areas are of especial importance:

  (a) Geographic and depth distribution of earthquake sources, and characteristics of earthquake mechanisms.
  (b) Locations and geometric features of active and potentially active faults, and their individual patterns of behavior through geologic time (with special emphasis on Quatenary time).
  (c) Earthquake recurrence rates and their variations with time, general location, fault behavior, and tectonic setting.
  (d) Influence of earthquake mechanisms and geologic environments on seismically-induced ground motions.
  (e) Influence of local soil conditions on seismically-induced ground motions and ground response.

- *Earthquake technology transfer* Study should be directed at decreasing the time-lag between development of new research information and its incorporation into design practice. Improved technology transfer between the research groups and the design professionals is the objective. Means to study include centers for the dissemination of information on earthquake effects, analysis procedures, computer programs, recent publications, and seminars or courses for practicing engineers and architects.

Funding Levels	FY '74 $ millions
Earthquake Resistance, Prediction and Control	$13.0

*These aspects of earthquake research, such as prediction and control, are important and necessary, but are considered to be of a somewhat lower order of importance and urgency at the present time. They require a much longer research time span and have a greater risk relative to payoff.

26. WEATHER HAZARD MODIFICATION (B)

The Problem

Losses due to weather hazards amount to several hundred lives and several billions of dollars annually in the United States. Hail alone, for instance accounts for annual losses amounting to $300 million, primarily to agriculture.

Research Objective

To continue, at moderately increased funding, the existing RANN program in weather modification research, aimed at reducing weather hazard losses.*

Specific Research Strategies and Programs

- *National hail research experiment* The former Federal Council for Science and Technology designated NSF as the lead agency for this experiment, now in the third year of a five-year period.
- *Ice nuclei counting* This program focuses on the development of the capability to measure the type and number of ice-forming and condensation nuclei present and the type and number and efficiency of those introduced artificially. The ability to produce the desired effect from cloud seeding, be it rain or snow augmentation, fog dissipation, or hurricane modification depends strongly on this knowledge.
- *New concepts and mathematical models for weather modification*
- *The effects of pollution and urban growth on local climate and weather*
- *Social, economic, legal and ecological impacts of weather modification* The purpose of this effort is to attempt to isolate the impacts of modification measures and the distribution of benefits, costs and risks to society.

Funding Levels	FY '74 $ millions
Weather Hazard Modification	$5.7[a]

[a]This figure compared to RANN/modification programs of $4.7 and $5.7 million in FY '72 and '73, respectively.

*Serious questions concerning national policy in weather modification remain to be answered, including decision as to operational programs in snow augmentation and reduction of hurricane winds, regulations and procedures, and methods of assigning weather responsibility.

27. EXPERIMENTAL SHORT-RANGE WEATHER WARNING SERVICE (X)*

The Problem

Several hundred lives and several billion dollars annually have been lost due to short-lived violent weather phenomena such as hurricanes, tornadoes and floods. These losses can be substantially reduced if preventative measures are taken.

Research Objective

To conduct a pilot test of the feasibility and effectiveness of a short-range warning service. The program should be tested in an urban and a rural area to (a) provide real-time detailed information on local weather hazards to the general public and to special users; (b) determine the usefulness of the information; and (c) design improved systems if initial results warrant. The program should be designed so that the users can interact effectively with the observation and dissemination systems. An integral part of the test should be to study thoroughly the value of the system to each major user group, its economic benefits and costs, secondary and nonquantifiable benefits or possible disadvantages, and the degree of public interest and confidence in the products.

Specific Research Strategies and Programs

- *Pilot short-range weather warning system*† This system would utilize high resolution satellite cloud images, ground-based weather radar pictures and

*This area was ranked very low, largely because of concern over possible conflicts with the existing programs of other agencies. This assumption should be investigated further by RANN staff and, if no such conflicts exist, the priority should be upgraded.

†This program is sensible only in close collaboration with existing concerned agencies. The National Oceanic and Atmospheric Administration (NOAA) has a central interest as the agency primarily responsible for providing warnings of weather hazards to the public. NOAA should participate in the planning, direction, and evaluation of the pilot project, and would be the logical responsible agency if an operating program should result. RANN's role in this program is visualized as (a) insuring that the interdisciplinary aspects are fully and effectively incorporated in the design, operation and evaluation and (b) providing access to high quality talent available in universities, nonprofit institutions, and industry.

automatic station observations. These would be processed and coordinated by computers providing any television screen in the region, with a pictorial and/or graphical display of properties such as the movement of convection cells, change in air pollution levels, development of a sea breeze, rise of water level or the detailed distribution of temperature or wind. Specialized radio or television presentations would be provided for specific sectors of the population such as those primarily concerned with transportation, agriculture and other businesses, as well as timely warnings of imminent weather dangers for the general public.

Funding Levels	FY '74 $ millions
Experimental short-range weather warning service	$4.0

VI. EXPLORATORY DEVELOPMENT AND TECHNOLOGICAL OPPORTUNITIES

No preconceived research plan can anticipate all the emerging trends in the society which produce new problems and new opportunities or those unsolicited research proposals which have the potential for important, near-term payoffs. In order to recognize unanticipated opportunities for exploratory development and technology application, however, RANN's role in the research community must be carefully considered.

The committee found useful an analogy, drawn by its Targets of Opportunity panel, between RANN and the central research and development laboratory of a large and diversified company: "Central laboratories do not conduct technical work in direct support of the firm's operating divisions. Instead, they seek new ways to fulfill the functions of the products or services that the divisions supply, or they work to suggest new functions appropriate to the firm.

The National Academy of Sciences Committee on Atmospheric Sciences estimated that the program would require $4 million in new money for research operations and $8 million for satellite and ground-based communication links, data processing, and the data transmission system. Since it is recommended that the project not get under way with less than this approximate level of funding, RANN's share of the cost would depend upon what the other agencies contribute, but would not exceed the $4 million amount.

"Generally, the central laboratories support activities which are vitally important to the overall objectives of the company but which, because of the limited horizons and the immediacy of concerns of the decentralized components, would otherwise be ignored. Concentrating on the development of new products or services needed to meet growing or shifting customer demands, central laboratories look to the marketplace for new opportunities when there are gaps in the operating components' abilities to satisfy particular classes of customers. These organizations normally are expected to monitor the advancing frontiers of science and technology, continually assessing the applicability of new knowledge to the business. In order to make this assessment, it is usually necessary to perform technical work at least to the point of demonstrating the technical feasibility of new ways of fulfilling the functions for customers.

"The successful pursuit of this central mission frequently requires new concepts for the management of the delivery of new products and services, as well as new research and new technology. Thus, while a central laboratory can initiate and support exploratory activities, only the company's central management can commit the firm to the major expenditures and organizational changes required for the successful completion of exploratory projects."

Analogous to the role of the central laboratory, as described, RANN must be able to identify the deficiencies in the operating systems of the U.S. society aimed at meeting national needs and to specify the types of activities required to correct those deficiencies. Projects can then be initiated to capitalize on new opportunities for work which would not be carried out by the established system and which may in some cases lead to the establishment of new institutions for meeting specific national needs. Projects of an exploratory nature can and should be initiated independently by RANN. Some of the overall guidance for major projects must come from the Executive Office of the President, where determination can be made as to whether or not the consequences are desirable or can be supported within the framework of the priorities and resources of the nation.

This view of the future of RANN does not imply that either the assessment of opportunities or the performance of the work

should be located wholly within RANN itself: these capabilities must be established in many places throughout the country.

However, it is obvious that deficiencies in both political and economic market mechanisms preclude a totally effective evaluation and support of activities addressed to national needs by existing institutions; for one thing, both explicitly and implicitly recognized national needs change more swiftly than institutions can adapt themselves. RANN must identify these "market" failures, deriving from them the major criteria for its own programs.

28. TECHNOLOGICAL OPPORTUNITIES (A)

The Problem

It is, in principle, and far more in practice, impossible to identify in advance all those research areas containing potential projects which should rank very high in RANN's priorities. Funding must be set aside to exploit these opportunities as they arise. The committee has recommended 5 per cent of the RANN budget.

Research Objective

To allow RANN to be receptive, and flexible beyond its usual planning and criteria constraints, to exceptionally promising proposals with near term payoff. (A similar category for longer range conceptual work is described below under the heading, "Exploratory Development.")

Specific Research Strategies and Programs

The term "technological" here is used in the broadest sense of any application of technique or expertise. The category consequently is not limited to "hardware" proposals. A single, hypothetical example may suffice to give the flavor of what is proposed here. Under both the existing RANN program, and the priorities recommended in this report, transportation ranks low,

even though it is, of course, one of the most important factors in our economy and society. No one would argue that there are no important, researchable, but unsponsored transportation issues, yet few opportunities seem to have arisen for RANN to play an important role in this area. However, one can say with reasonable certainty that there are potential projects which are at once very promising, within RANN's capability to handle, and for one reason or another not likely to be supported by the Department of Transportation. The technological opportunities category provides a place for such a proposal should one be submitted to RANN. In general, the reservation of funding here assures that no sound project need be ruled out of eligibility for RANN by rigid adherence to program plans. Programs funded under this heading should have exceptionally good expectation of payoff, the chief reason for the absolute priority accorded here to this area. But "expectation" here is used in the sense of mathematical probability: projects suitable for this category may be *either* well-defined proposals with near-term payoff which, for any reason, do not fit within the balance of the RANN or operating agency research plans or support criteria; *or* pilot projects (which may be high risk) with the potential of demonstrating important practical applications of radically new technology or software.

Funding Levels	FY '74
Technological Opportunities	5 percent of total RANN budget[a]

[a]This should be regarded as an upper limit for the total technological opportunities package, where actual allocation must be contingent on the availability of important projects and exceptionally well qualified researchers to carry them out.

29. EXPLORATORY RESEARCH AND ASSESSMENT OF THE FUTURE (A)

The Problem

Societal movements and trends produce a continuous surfacing of new problems and new opportunities. Insufficient capacity

exists for detecting these basic changes and studying their potential impact before their effects become unmanageable.

One example of an area which is changing, and where applied research could contribute to understanding basic trends, is nutrition. The manner in which people are fed and the foods they eat determine the demands made on the agricultural system and affect the health even of a substantial fraction of affluent Americans. Millions of the nation's people are hungry, and malnutrition affects the health and the future prospects of a large proportion of young Americans. Finally, the United States exports agricultural surpluses to meet the nutritional needs of the world at the same time it imports beef for a carnivorous people. Many such fundamental aspects of our society might well be amenable to research proposals under this program.

Research Objective

To create the capacity within RANN to pursue such opportunities which are deemed highly important but which may fall outside the purview of other Federal agencies and to promote flexibility in responding to unplanned for proposals. As with Technological Opportunities, the committee has recommended an allocation of 5% of the RANN budget.

Specific Research Strategies and Programs

The following are to be regarded only as examples.

- *The unaided poor* A striking feature of the present welfare system is the number of eligible recipients who do not receive welfare. A rough estimate is 10 million (derived from the difference between the 25 million members of households with family incomes under $3,000 and the nearly 15 million individuals currently receiving federal and state welfare aid). Since they are not now on welfare, these citizens are not the direct concern of either federal or local social agencies. They endure poverty and its related problems. They present a major potential liability for the welfare system. Indeed, this may be a case in which timely governmental intervention will show a high return to society.

Research questions appropriate for support by RANN include:

(a) What are the characteristics of the unaided poor in terms of their social integration, job skills, attitudes, etc.?

(b) Why do the unaided poor fail to take advantage of welfare?

(c) Why are we beginning to find more white ethnic groups turning to welfare?

- *Disposable time* The amount of disposable time controlled by individuals is steadily advancing. Curiously much study is devoted to the uses of disposable income while the use of disposable time is virtually ignored. It is often incorrectly assumed that "disposable time" and "leisure time" are synonymous. RANN should support the study of present trends in the use of disposable time and examine the new societal opportunities implied by the growing availability of time for new activities. (For instance, the impact of disposable time on educational institutions would be appropriate for RANN study.)
- *Design of alternative futures* A more imaginative and systematic consideration of technological and institutional patterns lying outside our customary range of experience is needed. Situations that might lead to future disasters also need some investigation. The careful building of scenarios to investigate the feasibility of arriving at different future points is a useful tool for this purpose.

  This technique might prove useful in investigating the risks, costs, and likely responses to an agricultural failure arising from monocrop culture. Another possible application is the investigation of the consequences of more extreme compartmentalization between where people work, live and recreate. Yet another study of environmentally neutral forms of growth in economic output and welfare would be of interest (for example, how to package leisure and disposable time, how to increase productivity in the provision of public services and education).

  As this program would identify opportunities for technological development and specific research needs, pieces would likely be transferred to other program areas. For this reason and because this area is exploratory and experimental, funding levels need not be too high.

Funding Levels	FY '74
Exploratory development and assessment of the future	5 percent of the RANN budget[a]

[a]This should be regarded as an upper limit for the total exploratory development package, where actual allocation must be contingent on availability and important projects and exceptionally well-qualified researchers to carry them out.

30. NATIONAL SYSTEM OF ELECTRONIC STORAGE AND RETRIEVAL OF THE PRINTED WORD (X)

The Problem

The information explosion continues and the amount of knowledge recorded in print is doubling every ten years. For institutions which attempt to maintain an exhaustive collection of printed works (e.g., the Library of Congress), as well as college, university, industrial and public libraries, this means an increasing and often overwhelming stress on facilities and a continuing need for new buildings and more staff. The problems of efficiently managing large libraries seem to have swamped present systems while the cost of expansion and storage are becoming exorbitant.

Research Objective

To develop a system of electronic storage and retrieval which could supplement the nation's present collection of books, and improve the cost-effectiveness of the country's information services to all segments of the society.

Specific Research Strategies and Programs

- *National storage and retrieval system* The contents of storage should, through new technology, be made available to anyone in any part of the country, perhaps through a system of machine readable storage of full texts. RANN might undertake a feasibility study of the project and produce some estimate of its cost. Any further research by RANN should be determined only after the results of such a feasibility study, and then, hopefully, passed on to another body more suited to bear the presumably heavy cost of developing the system. This project is a possible example falling under No. 28, "Technological Opportunities."

Funding Levels	FY '74 $ millions
National system of electronic storage and retrieval of the printed word (feasibility study)	$0.5

31. PRODUCT SAFETY AND USEFULNESS (X)

The Problem

The safety and the usefulness of many products ranging from food and drugs to motor vehicles and industrial machinery is cause for heightened public concern. At present the responsibility for ascertaining risks and benefits is fragmented between private industry and organizations and government agencies at the Federal, state, and local levels.

Research Objective

To achieve a comprehensive understanding of the present system and, secondly, to investigate more effective forms of product safety surveillance systems.

This project is considered an example falling under No. 29, "Exploratory Research and Assessment of the Future."

Funding Levels	FY '74 $ millions
Product safety and usefulness	$0.5

Conclusion

In the Spring of 1972, when this study began, the RANN program was approximately two years old. In many senses its mission to confront a broad range of society's problems and dilemmas through the support of interdisciplinary, problem-oriented research is both unique and relatively untried. This study, then, has been undertaken at a time when the direction and scope of the RANN program are still evolving, and at a juncture when RANN, having had two years' experience, is examining itself in order to develop and shape its future.

The report is essentially a working document, in which timeliness was a far more important criterion than perfection. It is intended to contribute to the thinking of those who guide and review the RANN program at this stage in its development, and is in no way a study of national goals or needs *per se*, nor is it a long range blueprint for RANN. Furthermore, the report should not be viewed as a detailed evaluation and critique of the FY '72 RANN program; at no time were the individual research projects supported by RANN subjected to detailed scrutiny. Rather, those who undertook this study addressed the more limited, yet infinitely complex, task of identifying and recommending priorities among the many potentially profitable areas of research where RANN support could contribute to the meeting of the nation's needs.

One particularly important concern was raised throughout the study and should be mentioned here. The unique nature of the RANN mission poses problems related to the very functioning of what we have termed the applied research delivery system. These are the issues of how to organize, manage and utilize applied, problem-oriented, multidisciplinary research in such a way as to achieve the most fruitful relationship between research sponsors and managers, research performers and potential users. Concern has been expressed, for instance, that the nation's capability to undertake and utilize the kind of research recommended herein may be limited, particularly where there is dependence on the use of interdisciplinary teams and by the alleged insufficiency of social science personnel for work in problem-oriented areas of research. These and many related questions were discussed during this study, but deliberations reinforced COPEP's initial assumption that such problems are so extensive and important as to merit a study of their own. Few conclusions in these respects, therefore, are included in this document; rather attention to these issues, in a planned, subsequent study, will be an essential complement to the work of this study.

Many of the most significant conclusions of this study lie not in the discussion of the overall balance of the research recommended, nor even in the 31 specific areas recommended for programmatic emphasis, but rather in the discussion of two themes, "institutional functioning" and "conservation and patterns of consumption." Here we recommend that the RANN program undertake applied social science research and research into the "demand side" of certain national problems to an extent that would make it virtually unique among mission agencies. These recommendations do not overshadow the need for RANN support of engineering and applied physical science research and, indeed, a coordinated approach to research is deemed imperative; yet these themes contemplate for RANN an approach to problems which had not been widely pursued nor broadly supported before. It is in this way also that this report, while affirming much of the previous work of RANN, recommends some shifting in the emphasis of its overall program.

Other themes significant to our discussion are also delineated in this report: The need for a coordinated and balanced approach to

research has been mentioned above, and is a vital element in RANN's supportive and gap filling role. We also make particular reference to the "user" orientation of RANN's mission. The follow-up and application of information, methods of problem solving, and technology developed through RANN programs depend not upon RANN but others, most usually Federal agencies and state and local governments and also upon the private sector. The recommendations in this document, therefore, rest on the assumption that potential "users" both exist and have been identified. By and large this report concludes that research funded by RANN, and therefore committed to a problem orientation, should be planned and conducted in close association with the potential "users" so as to assure an understanding of their needs and to increase the likelihood that recommendations will be usefully implemented.

One further significant theme reflects our appreciation that RANN is an agency with greater opportunity to be adventurous and innovative than the more established agencies. The recommendations are influenced by our conviction that such a freedom can and should be used to great advantage. Taking some wise risks in the selection of program elements would not be capricious in the case of RANN, but rather a realistic recognition that RANN was created partly as a response to the limitations of traditional concepts and responsibilities in funding problem solving research. The particular challenge implied in the formulation of the RANN program is to achieve more than a supportive or gap filling role in relation to the research work of Federal mission agencies but also to engage the full imagination of the research community, often looking beyond the immediate to the fundamental and long-term problems.

The 31 research areas recommended in this report range widely through the substantive areas identified for consideration: community development and human resources; environmental quality and growth; conservation and management of energy, materials and land; new production processes; and natural hazards and disasters. Furthermore, to assure flexibility, we assigned highest priority to reserving 10 percent of the RANN budget for promising "technological opportunities" and "exploratory development" projects not otherwise encompassed within RANN's

formal planning. The scope of these last research areas is therefore not defined in detail, yet we presume that many of the projects falling within these categories would either be particularly opportune and timely, or else innovative and oriented to opening up little explored aspects or areas of thought and research.

Of the individual, specific research recommendations, significantly heavy funding is proposed for research related to solar energy. Our deliberations indicated that RANN should be most concerned to provide a balance within the nation's energy research effort and should focus essentially on research oriented towards the *conservation* of energy, yet also on research into the less conventional forms of energy *provision*, particularly into solar energy. The urgency of potential energy scarcity yet the relatively limited national commitment in the solar area led to a strong judgment that RANN's contribution in the area should be expanded.

The program recommendations within this report, taken together, are consistent with much of the work undertaken at present by RANN, but also call for considerable redirection, reorientation or combination of certain elements. COPEP deliberately chose for its study a different taxonomy from that by which RANN is organized in order to benefit from the light of a new approach. Because of this, a comparison of the recommended program to the current RANN program is provided in Appendix A.

RANN's mission demands that it be both ambitious and innovative, requires that it take risks at the frontiers, and mandates that it contribute to the resolution of national problems and dilemmas. Yet while applied research in the social sciences, or engineering and the physical sciences, can provide new insights to decision makers and new opportunities for change and improvement, it can achieve neither the perfect state nor that society where difficult choices need not be made. Even in the narrower sense of achieving more limited and specific goals, applied research confronting human complexities can seldom have the dramatic impact of a space program that reaches the moon in ten years. Perplexing "people problems" and the consequent inadequacies of social institutions are susceptible only infrequently to the technical "quick fix" popularly hoped for.

As expressed earlier in this report, however, we have recommended specific areas of problem-oriented research, and new program thrusts, in the belief that research sponsored by RANN can indeed contribute to the meeting of many of society's critical, unmet needs. RANN's mission is not aimed at seeking knowledge primarily for its intrinsic value and for the edification of the researcher and his peer, but rather to lead to the prompt utilization of the results by a client or consumer who can and does put them to use to solve or ameliorate problems. To this end, RANN's task is to support research applicable to the resolution of national needs, the amelioration of national problems, and in part, to support the more diffuse purpose of seeking information, insights and new perspective as alternatives which will provide users, problem solvers and other researchers with new ways of looking at their problems as distinguished from answers or solutions *per se.*

COPEP believes that the value of the RANN program is proving itself. Especially because of its relative infancy, it is a dynamic, evolving program; its essence cannot wholly be captured in a snapshot view which was frozen a year ago. The recommendations of this report, initially communicated to RANN in the Fall of 1972, were intended as a guide to the identification of immediate priorities and intermediate range goals for the support of research applicable to national needs.

Appendix A

The recommendations of this report display considerable parallels with the existing* RANN program but also suggest considerable redirection or reorientation of certain elements and areas of emphasis, as would be expected from a review which concerned itself with unlimited possible goals and was conducted early in the history of a new program.

The differences in taxonomy between the COPEP ad hoc committee recommendations and the existing RANN program preclude a simple one-to-one correlation of the individual programs. Suffice to say they would require shifts in emphasis both between and within programs.

For convenience of discussion, the present RANN program is presented in Table III which can be compared with the recommendations of this report.

*As noted elsewhere, except where otherwise indicated, the discussion of "existing" or the "present" RANN program refers to FY 1973.

TABLE III Current RANN Program

	Actual FY 1972	Estimate FY 1973
Advanced Technology Applications		
Energy Research and Technology	$ 8,900,000	$12,700,000
Disaster and Natural Hazard Research	4,200,000	6,950,000
Urban Technology	1,500,000	5,900,000
Technological Opportunities	4,300,000	5,650,000
Total	$18,900,000	$31,200,000
Environmental Systems and Resources		
Regional Environmental Systems	$10,100,000	$10,100,000
Weather Modification	4,400,000	5,300,000
Environmental Aspects of Trace Contaminants	5,000,000	6,400,000
Total	$19,500,000	$21,800,000
Social Systems and Human Resources		
Municipal Systems and Services	$ 7,100,000	$ 8,700,000
Social Data and Community Structure	3,600,000	3,300,000
Total	$ 6,700,000	$12,000,000
Exploratory Research and Problem Assessment		
Exploratory Research	$ 2,900,000	$ 3,100,000
Problem Assessment	1,800,000	1,900,000
Total	$ 4,700,000	$ 5,000,000
Total RANN Program	$53,800,000	$70,000,000

ADVANCED TECHNOLOGY APPLICATIONS

1. ENERGY RESEARCH AND TECHNOLOGY

The energy research and technology program is RANN's single largest program representing over 15 percent of RANN's current budget. It is composed of four subprograms: energy systems, energy resources, energy conversion, and transmission systems. Because of its size, it is best addressed in parts.

The workshop, as one of its principal themes, recommended that research and studies of energy consumption and conservation be accorded great importance because of RANN's unique position in reference to the currently limited and fragmented work (and responsibility) existing across the Federal government and elsewhere with respect to the demand side of the energy problem. While RANN now sponsors some activities on demand and consumption questions in both the energy systems and energy resources subprograms, the report recommends that these areas considerably be strengthened and expanded. It should be noted that RANN's research on increasing conversion efficiency in the energy resources subprogram also contributes to the same end.

A similar conclusion applies to the institutional and regulatory systems as they affect energy consumption. Some effort in this regard now exists within the RANN energy system subprogram, but considerable expansion is recommended.

With respect to energy provision, the report recommends reinforcing RANN's present concentration on unusual or advanced non-nuclear power sources while leaving research on more conventional forms to other agencies and industry. The workshop especially proposed stronger emphasis, within this area, on solar energy and, while the overall recommendation for energy provision falls within the "X" category, the solar energy research component should be included in Category "B."

The report also concludes that, given RANN's limited funds and the relative priorities, RANN's current subprogram on transmission systems and power networks should receive comparatively lesser emphasis, especially if one assumes the efforts of other agencies and industry.

Research on the environmental effects of energy provision and use was also considered important. At this time RANN's energy program has only limited funds tied specifically to this area. The activities underway on the related area of new or non-polluting energy sources, however, contribute to this theme.

2. DISASTER AND NATURAL HAZARD RESEARCH

The disaster and natural hazard research now in RANN concentrates on earthquake engineering and fire research. The

report recommends, in addition, construction of a small scale fire modeling test facility to exploit new concepts, and the eventual construction of a full scale testing facility. Further, stronger emphasis was recommended for work in determining and disseminating design information for earthquake-resistant buildings. Three new areas were recommended by the workshop: research into the management of hazards caused by surface waters, focused particularly on developing practical applications of existing research results in this area; the investigation of an experimental short-range weather warning service; and (partly new) research to better understand the economic, social and political responses to natural hazards, with the objective of optimizing the nation's adjustment to such hazards.

3. URBAN TECHNOLOGY

The current RANN urban technology program encompasses a very large excavation and tunneling technology program and a smaller urban systems technology program focused primarily on research needs for housing design and integrated utilities functions.

From an urban or community development standpoint the present tunneling program was considered to concentrate too narrowly on hard-rock tunneling to the exclusion of softer ground tunneling problems. Strongly emphasized was the topic of conservation under which rubric research on the legal and institutional problems and the development of better understanding of subterranean land use were highly recommended.

RANN's present urban systems technology program is not treated as a discrete entity in this report; rather it is included, where appropriate as part of the concern for improving local service delivery systems (e.g., the technology related to disposal).

4. TECHNOLOGICAL OPPORTUNITIES

The RANN technological opportunities program contains the instrumentation subprogram and the advanced industrial process-

ing subprogram which currently covers enzyme technology and extractive metallurgy.

Industrial processes and manufacturing, as a program area for research by RANN, is emphasized; however, this recommendation is directed toward the functioning of business, labor and institutions, and therefore represents a call for redirection of emphasis. The current enzyme technology activity is not given high relative priority within RANN. RANN should try to determine whether its support could be obtained from industry.

The extractive metallurgy activity was absorbed into the conservation and management of materials program here, but with a suggested emphasis toward the conservation of materials. The instrumentation subprogram received no specific attention but was absorbed into the opportunities program as an example of projects which might fall within that category.

ENVIRONMENTAL SYSTEMS AND RESOURCES

5. REGIONAL ENVIRONMENTAL SYSTEMS

The present RANN regional environmental systems program addresses four types of environmental regions: coastal areas, river basins, urban-rural environment, and primitive areas. These are further subdivided into 14 project areas. The range of the research includes data gathering; determination of cause and effect relationships; and modeling and analysis. This overall program is considered under a set of areas including recommendations 10, 12, 13 and 14.

A significant portion of the present RANN program consists of advanced modeling activities. Modeling in these areas should be redirected for a greater stress on developing empirically sound relationships, rather than on model building *per se*, to provide maximum impact within available funds. In addition, this research area should stress relationships to specific goals, tied to the examining of the consequence of alternative actions that have relevance for policy makers, and having clearly planned interfaces with such policy makers. Emphasis toward areas in which

important decisions will have to be made in the next several years, choosing among a number of management alternatives, is recommended.

6. ENVIRONMENTAL ASPECTS OF TRACE CONTAMINANTS

Recommendation 11, research on the biological and physical environment, encompasses RANN's current trace contaminants program.

7. WEATHER MODIFICATION

The weather hazard modification recommendation corresponds generally to the current RANN program in this area.

SOCIAL AND HUMAN RESOURCES

8. MUNICIPAL SYSTEMS AND SERVICES

This program area represents a relatively large RANN activity and falls within the recommendation here for improving local service delivery systems. Considerable expansion of the applied social science aspects of this program and a greater variety of research areas are called for, however. Presently, this RANN program is concerned almost exclusively with applied social science research while related physical science and engineering oriented efforts are administered in other divisions of RANN. A significant modification is therefore implied by the recommendation for closer ties between research into the physical science and engineering and applied social science aspects of the delivery of municipal services.

In addition, the developing and maintaining of close coordination between researchers and users especially at the state and local level, is particularly stressed.

An additional recommendation is for RANN to broaden the boundaries of the program from "municipal" to a broader context of "local systems" which include both quasi-public institutions such as hospitals, as well as larger geographical jurisdictions such as counties or school districts.* Included in this broadened scope are investigations of problems caused by the mismatch and overlapping of jurisdictional boundaries and the boundaries of the problems which are many times far from coincident.

Included within RANN's municipal systems and services program area are new programs on the impact of broad-band cable tele-communications.† The FY '74 budget also contains funds for activity related closely to the recommendations under Improving Local Service Delivery Systems.‡

*In this connection it is interesting to note RANN's decision in its FY '74 program to pay particular attention in research into the problems of non-metropolitan areas to those with declining populations which is in harmony with the specific research topic "Social and Economic Aspects of Areas in Decline" recommended in this report.

†The FY '74 budget presentation also states:

One of the major effects of modern telecommunications technology will be in the area of service delivery, as ways are sought and found to substitute technology for human effort in this largely non-mechanized area. It is also expected that it will prove possible to make available a range of services which have up to this point remained impractical because of the difficulty and expense of communicating using present systems.

During FY 1972 work began on evaluating the use of telecommunications technology to improve citizen-government relations, and this work will have been continued and expanded somewhat during FY 1973. During FY 1974 this work will be evaluated, and design work will begin on projects seeking to test the more promising systems on a large-scale basis.

‡c.f. p.52 recommendations on distribution of responsibility for government services and costs, and personnel policies, and compare with the following extract from RANN's FY 74 budget presentation:

Government services are financed by the most diverse forms of user charges, special taxes, general taxes, and from state and Federal revenues. The resulting pattern of services, quality of services, and costs of services generate erratic development patterns, districts with rich and poor services, high and low taxes, and other undesirable social and governmental characteristics. In *Serrano v. Priest* and similar cases, the courts are now finding that traditional ways of financing services are inequitable. While there are a number of excellent public finance studies of particular metropolitan areas, or more rarely of states, and some generalized national studies, they fail to cover the full range of alternatives and their social, economic, and political consequences. In FY 1973 several studies examined the benefits and costs of alternative forms of taxation and finance. In FY 1974 the financing of a representative set of services will be examined systemically with attention to the implications of recent legal decisions on equity in finance.

9. SOCIAL DATA AND COMMUNITY STRUCTURE

Within the existing RANN program, this area contains three sub-programs: population baselines, social data utilization, and evaluation methodology for social programs. Expansion of research on evaluation methodology for social programs and services is recommended. Both research on evaluation methodology and the improved design of social field experiments, yielding potential means for measuring public accountability, were considered important for RANN. It is suggested further that RANN should actually design such field experiments and evaluation methods to tie on to selected existing programs or the social experiments planned by other agencies, and thus provide a greater insight into the consequences of alternative plans, strategies and programs.

The two RANN analysis subprograms form a significant part of workshop recommendation No. 2 considering social indicators. This effort is also considered very much worthy of continuing emphasis.

EXPLORATORY RESEARCH AND PROBLEM ASSESSMENT

This RANN activity supports studies to define and analyze specific national issues, identify alternatives and to assess the consequences of present or proposed applications of technology. (The growing field of technology assessment is one in which COPEP has maintained a continuing interest* and to which it expects to return in later phases of this study.) Research is also

A large and growing issue in municipal government is the structure of their personnel system. Municipal personnel management is becoming increasingly complex because of the unionization of public employees and the growing feeling of "professionalism" within bureaucratic ranks. In FY 1974 a comparative analysis of municipal organizations and procedures will be initiated to determine practices that are internally efficient and equitable and that provide a proper amount and an appropriate mix of public services.

*See COPEP's report, *A Study of Technology Assessment* (1969), published as a committee print of the Committee on Science and Astronautics, U.S. House of Representatives.

supported in formative stages to determine the opportunities and utility for research in specific areas prior to the development of major RANN divisional program. Efforts within this activity are grouped under three subject areas: technology and the economy, individual well-being and human development; and alternative futures and institutional innovation.

Efforts under technology and the economy include preliminary studies exploring the enhancement of private sector innovation and productivity, including investigating research opportunities in automation and consumer durables, and analyses of consumer product safety and efficiency. Additional preliminary research on the impact of telecommunications is also included in this activity.

The individual well-being and human development heading includes analyses on the impact of changing work and leisure patterns, the legal system in an increasingly technological society, and consequences of alternative population patterns. The alternative futures and institutional innovation heading include both general studies examining the future and developing techniques for such examinations.

This program area represents a number of diverse research activities under the category of exploratory research. Many of these are representative of subject areas encompassed in a broader or larger sense by the program recommendations of the workshop. These efforts can be considered as the nucleus of "pre-start" activities for related programs and, in a sense, the emphases attributed to the program recommendations may be applied to this "program within the program." Of more significance, this area is consistent with, in part, the philosophy of one of the highest priorities of the workshop to reserve a small percentage of the total budget for program flexibility in the areas of technological opportunities and exploratory research (recommendations 28 and 29).

Appendix B

The Community Development Panel created an analytical structure for research questions about substantive issues on community development. This formed the basis of many of the research recommendations in both the Community Development and Human Resources areas. This is only one of many alternative and perhaps more useful listings of categories. It is, in this case, peculiarly weighted towards research on public policy formation, implementation and evaluation–reflecting a judgment that the behavior of the public sector should bear a heavy weight of research resources. We have appended it here merely as valuable source material.

I. The Government

A. Positivistic Analysis–how government behaves and the process of policy formation and program implementation.

1. Responsiveness

a. State and local legislative behavior. Though the role of legislatures is critical, their performance, as a whole, is poor. Typically the legislator is a part-time official without the information, background or resources to act wisely. Illustrative research questions are: How well do they perform?

Under what conditions has legislative initiative been fruitful, counter productive? What changes should be made to increase their effectiveness? Could research carry weight?

b. The Electorate and the Legislature. The legislator is very poorly informed of his constituency's preferences and the citizenry are ignorant of the legislator's behavior. What are the incentives which motivate the legislator and how can we improve the information flow between legislator and citizenry?

c. The Legislature and The Administration. The administration has the resources and information for policy formation and implementation. The legislature has authority but the administration has the resources. The checks and balances work very unevenly, but more important is the random effect on policy and programs. How well does this system work, and how can it be improved?

d. The Administration. The bureaucracy has been blamed for the sins of omission and commission of public policy. The incentives to resist innovation and any evaluation of behavior are strong. Can we develop incentives to adopt innovation, to alter policies, to be more responsive to consensual needs?

e. Community Organization. Characteristically, citizen groups tend to focus on protection of disaggregated interests. Often they act as a veto power to frustrate change which appears to threaten interests. How can they be convinced to accept change—e.g., a park which may attract distant residents, for instance?

f. Community Participation. The initiatives of community organizations are recent and sporadic. Can they be structured to create a users market for public output?

g. Incentives for Decision Makers. Seniority, rather than productivity, is too frequently a criterion for promotion. Can incentives, linked to social productivity, be invented and adopted?

2. Conflicts of Governments

Urban policy and programs are formulated and administered through a complex of governments which are imperfectly related. Taken as a whole and separately, the question is to study the existing relationships and their policy effects to see if reforms are possible.

a. Federal vs. Local. Federal procedures often badly fit local preferences. Federal objectives are not the same as local ones, and a national consensus can be frustrated by local behavior, e.g., urban growth policy.

b. Regional vs. Local. Regional government ranges from weak to non-existent. But there is a consensus on the need for regional governance for interdependent activities like transportation, land use, pollution control. How can "regional objectives" be achieved, and with what degree of local autonomy?

c. Local vs. Local. Externalities between neighboring cities are widespread. Policies designed to aid one city may have unfortunate effects on another, e.g., new development in one may draw off vitality from the other. How can these be internalized?

d. Special District vs. General Government. Education, for example, has long been a special government. In recent decades "regional" problems have been solved by the formation of special districts which are poorly related to other public agencies and to the electorate.

e. Operations of Intergovernmental Systems. The public sector is dominated by a division of financ-

ing, rule-making, administration, and evaluation among levels of government. Authority and responsibility is diffused and uncertain.

3. Production Processes

Many improvements to the performance of the government may be suggested by a careful analysis of their operating characteristics.

a. Scale economics—can tell us about the level of policy.

b. Interdependencies—can tell us about the scope of authority.

c. Distribution and use—can tell us about pricing possibilities and equity features.

B. Normative Analysis

1. Instruments:

a. Budgeting

b. Cost-benefit and benefit-risk analyses

c. Planning

d. Land-use controls

e. Regulation

f. Decision making

g. Taxation

2. Policies:

The possible set of policies are numerous. The ones to be chosen for emphasis are those on the political agenda today or are likely candidates for consideration in the near future.

a. Growth and density of metropolitan places

b. Residential segregation by race and wealth

c. Environmental consequences of development decisions

d. The spatial distribution of population by size of place and location

e. The distribution of income, wealth and the benefits of the public services

f. Movement and transportation

g. Residential and work mobility

3. Implementing Strategies:

a. Consensus Generation. One of the most difficult tasks for the policy maker is the generation of an agreement among groups with differential incidence of benefits and costs. The bargains struck in legislation are the outcome of the process. More desirable outcomes might be forthcoming if we knew more about the process and how to improve its operations.

b. Incentives to Innovation. Unfortunately the incentive structure encourages public officials to continue an unsuccessful routine rather than search for better performance. How shall innovative proposals be generated?

c. Adoption of Innovations. The resistance to new ideas is small compared to the resistance to new practices. Adoptions often take decades. We know too little about the process and the levers which could accelerate it.

d. Evaluative Procedures. Evaluation too easily can become rationalization when left in the hands of the administrators who are implicitly to be judged. More rigorous and systematic operational tools have to be developed.

e. Social Experimentation. Large scale social experiments can reduce the uncertainty about con-

sequences, and thereby increase confidence in a program. But care must be taken to ensure scientific rigor if experiments are not to become demonstrations.

II. Market

How the behavior of people, households and firms respond to governmental policy and vice versa is new to community development trends and forces. Research on improvements in market institutions is as needed as research on the market responses to policy instruments. Many objectives can be achieved by market improvements rather than public actions.

A. Migration–spatial shifts in activity

1. Rural to Urban
2. Intra-Metropolitan Residential
3. Intra-Metropolitan Jobs

B. Externalities–imperfections in market operations

1. Real Estate Market Imperfections
2. Environmental Spoilation
3. Scale Economies in Production
4. Scale Economies in Consumption
5. Mobility of Human Capital
6. Aesthetic
7. Group Decision Making

C. Land Use and Development

1. Inhibitives of the Market
2. Distributional Consequences

D. Privatization—Too often solutions to problems are sought by replacing private market operations with public administration. A reversal sometimes may be more fruitful. Many sectors of the local public services could be made more subject to the discipline of the market.

III. Knowledge, Science and Technology

A. Information Systems—Research on optimal information systems for the following groups seems desirable:

1. Legislators
2. Administrators
3. Citizens

B. Technological Developments and Transfers—Though institutional constraints are the greatest inhibitions to innovation, new technology can sharply reduce many problems. Transportation, communication, waste treatment, construction are all obvious candidates, though in each of these cases we have been unable to absorb the current technology.

IV. Values

The debate about needs, values, goals are as old as society. Though no resolution appears to be in sight, there are substantial improvements in how to debate the issues and how to relate the debate to public choices. Rigorous analysis at the theoretical and measurement levels is called for. Social indicators are one controversial effort to zero in on the problem. It is easy to fault the product but yet it is these efforts to analyze and measure welfare changes other than income which will help us clarify the study of goals at an operational level.

A less normative approach is to study changing life styles. The phenomena of the suburb, the automobile, the second home provide clues of the revealed preferences of the citizenry. They are rejecting old patterns and choosing new ones. Their behavior, and its implied values, should be studied for indications of changing goal structures.