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M. ATHANS

Dear Colleague:

Enclosed you will find a draft of a report that summarizes the activities of the ad-hoc committee on the National Systems Center. The committee members are currently reviewing it so that the final draft can be prepared and submitted to the National Science Foundation.

I would welcome your reactions, suggestions, criticisms, and comments in writing before December 15, 1978.

Thank you for your interest in the idea of a National Systems Center.

Sincerely,

Michael Athans
Chairman
Ad-Hoc NSC Committee

Enclosure

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THE NEED FOR A NATIONAL SYSTEMS CENTER: AN AD-HOC COMMITTEE REPORT

by

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ABSTRACT

The purpose of this report is to outline the findings and recommendations of an ad-hoc committee of systems engineers and scientists with respect to the establishment of a National Systems Center (NSC). The chairman of the ad-hoc NSC committee is Michael Athans of M.I.T. The committee concluded that there is a national need for a high-quality, independent, non-profit National Systems Center and that steps have to be initiated to establish such an entity. This report summarizes the developments to date (November 1 1978) and is written for the purposes of (a) information dissemination to the engineering and scientific community and (b) to solicit criticism and suggestions for future plans.

CONCLUSIONS AND RECOMMENDATIONS OF THE AD-HOC NSC COMMITTEE

The nation faces, and will continue to be confronted by, a sequence of critical decisions with respect to current and future complex technical, economic, and social issues. Limited resources require the coordination and optimization of complex interconnected, large scale, dynamic, stochastic systems. Existing methodologies are not sufficiently advanced, nor are they holistic enough, to provide understanding of key issues so as to advise planners and policy-makers at either the strategic or tactical levels. A significant national need exists for basic and applied research in complex large scale dynamic and stochastic systems. The available intellectual resources of the country are too fragmented by existing departmental and institutional barriers to address the complex and long range national needs and to provide technical advice to decision makers in matters of policy analysis and synthesis.

It is recommended that a high-quality, independent, non-profit National Systems Center be created to conduct both basic and mission-oriented research in the general areas of large scale systems analysis, modeling methodologies, and tools for planning, design, and policy analysis and synthesis. Such a center would constitute a national asset which is urgently needed.

To fully utilize the existing centers of excellence it is recommended that the NSC be a distributed entity, with an administrative office in Washington D.C. and several geographically distributed satellite centers. In addition to in-house research the NSC should sponsor, organize, and carry out annual multi-week institutes to formulate relevant problems with close interactions between decision makers and research scientists.

These institutes will be held at different locations each year.

The initial funding for the NSC should be provided by the U.S. Government, but should not be limited to the National Science Foundation (NSF). Stable long-range funding mechanisms for both the NSC and the annual institutes have to be found. Eventually the NSC should also be funded by foundations and private industry. The funds for the NSC should be in addition to those currently provided to universities, non-profit organizations, and private industry.

The caliber of the permanent NSC staff should be of the highest quality. Appointment to the NSC senior staff should involve the same scrutiny as, say, tenure appointments at major universities.

A HISTORICAL PERSPECTIVE OF NSC DISCUSSIONS

An NSF sponsored Workshop on New Directions in Systems Science and Engineering was held in Ann Arbor in 1972. One of the outputs of this workshop was a recommendation to the National Science Foundation to consider formation of a National Systems Center. This idea was further discussed in 1975 at a NSF-sponsored Workshop on Modeling Large Scale Systems at National and Regional Levels at the Brookings Institution in Washington, D.C. In 1976, NSF sponsored another workshop on the National Center for Systems Analysis at Ann Arbor. At that workshop an ad-hoc committee was elected to conduct further studies into the desirability of a National Systems Center. Funding for travel and publication expenses was provided by NSF under grant NSF/ENG 77-07777 to the Massachusetts Institute of Technology. Several successive meetings occurred and the membership of the ad-hoc committee was enlarged to include a broader spectrum of system scientists and engineers. On May 12, 1978, a workshop was held at MIT and a presentation was made which is essentially identical to the present report. About 80 people attended this workshop and still additional input was obtained concerning the viability and feasibility of a National Systems Center.

Following the M.I.T. Workshop it was decided that further exposure of the findings of the Ad-hoc NSC Committee was desirable. On July 19, 1978 Professor Athans of M.I.T. and Mr. Crosby of DOT briefed the House Committee on Science and Technology. In October 1978 Dr. Levis of SCI organized a panel discussion at the Second Lawrence Symposium on Systems, Berkeley, CA. Prof. Kahne of Case Western Reserve organized a similar panel dis-

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discussion at the Allerton Conference on Communication, Control, and Computing in Urbana, Illinois, and Dr. Chestnut of GE presented the idea at the Joint Automatic Control Conference in Philadelphia, PA. An editorial appeared in the September 1978 issue of the IEEE Spectrum. A panel discussion will be held at the IEEE Conference on Decision and Control. Plans are underway to make additional presentations at scheduled conferences of ORSA and several economic conferences.

THE SYSTEMS NATURE OF NATIONAL PROBLEMS

The systems problems at the national level may be divided into physical systems, socitechnical systems, and information systems. Energy, food, material and environmental issues present problems of finite resources and environmental capacity that must be dealt with while taking into account economic and political issues. As constraints become more binding, local subsystems become more interconnected. Local optimization can lead to very inefficient operation and even instability if the degree of interconnection and interdependence is increased. Interconnected large scale systems can exhibit poorly understood behavior of a dynamic and stochastic nature. Although systems science and engineering has begun to make contributions to some of these areas the systems science discipline has not matured to a level where it can be used routinely to contribute to the solution of these complex problems. In addition, there is a need to increase public awareness so that these large scale problems are viewed in their appropriate broad context.

A number of major problem areas exist at the national, regional, state and local levels which would benefit from the existence of such a systems center. These include power systems, communications systems, national defense systems, energy, pollution, resource management, food, health care systems, transportation systems, urban systems, industrial productivity and national-regional economic systems. Policy and decision makers in the various sectors of our society need better ways to enhance their policy making abilities.

Models play an important role in the analysis of large scale systems. Short term and long term decisions depend on the notion of a systems model. This may simply be a mental model in the mind of a decision maker or a simulation model stored in a computer. It may be an analytical model, which is a mathematical expression whose solution exhibits characteristics of the system in question. Several thousand models exist today which are designed to help make specific decisions. Attempts to interconnect them in various ad hoc ways to give more global insight generally lead to unreliable results. One consequence of this is that the credibility of models as an aid to decision making suffers. The world is dynamic and stochastic. Unfortunately, most existing models are static and deterministic. Thus the real value of current models is limited. There are competing methodologies which presently exist and which have little connection between them. The typical example is system dynamics based models versus econometric based models.

The availability of computers has focused attention on the use of models for the analysis

and understanding of possible results from various decision alternatives. In this process of using large scale systems models a number of difficulties have been revealed which have limited the effectiveness of these studies. Many of the models are not global enough and show only one area of region and do not provide any realistic feedback or interaction with the rest of the world. To date there are no adequate ways of demonstrating the range of validity of large scale systems models. Many of the models are inherently static, as mentioned before. Databases have generally proven to be inadequate. The data are not timely, they are not up to date, they are of questionable validity, and data formats are incompatible from one model to another. Although different models are nominally related they are generally unable to work together. It is difficult to obtain adequate documentation on how the models are built and how they are intended to be used. Many stochastic effects and uncertainties are simply not incorporated in these models. It is perhaps due to this fact that experienced decision makers do not trust the forecasts of available models and the outcomes of alternate scenarios. Yet, during the past five years some significant theoretical results have been obtained in the area of decision-making under dynamic uncertainty. Thus there exists a gap between the state of the art in system theory and the models available for planning and decision-making.

NATIONAL NEEDS

The committee has found ample evidence that all sectors of our scientific and technological society are becoming increasingly aware of the sad state of affairs. These sectors include government, industry, and universities.

As a specific example of government awareness that something should be done, we cite the formation in early 1978 of an ad hoc committee, chaired by Dr. J. Fearnside of D.O.T., with broad representation from several executive and legislative agencies, to address the formation of a government interagency committee for "Large Scale System Analysis". It is hoped this committee will encourage and guide the evaluation and assessment of existing and planned models and methodologies illuminating those areas most in need of development. The committee plans to recommend research in systems analysis, modelling and decision making to the National Science Foundation and other governmental agencies and organization. It intends to foster the development and maintenance of a facility for the exploration of policy alternatives. This facility will include a library of simulation models, decision tools, and data banks.

Another specific area in which complex systems arise, can be found by examining military command, control, and communication (C³) networks. The effectiveness of such C3 systems has not been analyzed in depth. Key questions such as the "force multiplier concept", i.e. what is the improvement of a particular weapons system when backed up with a timely and reliable C3 network have not been answered.

Industrial productivity is another area which certainly could benefit from the development of a national systems center. Problems of coordination,

planning and expansion are not well understood. Such issues are affected by technological advances, tax policies, regulation policies, subsidy policies, balance of payments issues, tariffs structures, and others. As mentioned earlier, several complex engineering systems are not well understood. These include power networks, communications networks, food processing and distribution networks, transportation systems and many many others.

Universities also have needs which could be filled by a national systems center. Much of the intellectual capital of universities is wasted on irrelevant and trivial research. No mechanism exists for handling research in directions of meaningful and pressing problems except through very indirect mechanisms of funding agencies such as the National Science Foundation. Generally, mission oriented agencies produce RFP's which are short range in nature. Industrial support for university research has been limited and there seems to be a continuing decline in university/industry cooperation. Information dissemination seems to be fragmented; specialists talk to specialists, publication of new research results is often delayed. There is a lack of appropriate educational materials, not only for university students, but also for the public at large. Universities do not have access to most existing models of complex systems, either due to a conscious effort on the part of the model builders or simply incompetence in the area of documentation and validation. High quality software for model estimation, specification, and optimization generally is not available and not easily transferred.

PROPOSAL FOR A NATIONAL SYSTEMS CENTER

We have seen that a national need exists in the area of complex interacting and interconnected physical, engineering, social and economic systems. The National Systems Center would have a number of major responsibilities which could help alleviate some of these problems. There is a need to evaluate existing theories and describe in detail the need for additional basic and applied research in the area of complex, large scale systems. It could examine limitations of present models of large scale systems and delineate in an objective fashion the areas of applicability. It must identify education and information dissemination needs and provide the user with the most appropriate tools and best information for policy and decision making.

The National Systems Center would have four basic activities: (1) basic and applied research on large scale systems, (2) basic and mission-oriented research on complex models, (3) education and information dissemination, and (4) unique institutional services.

Let us look at the research which may be conducted at a National Systems Center. Such a center would identify and encourage new areas of research with particular emphasis on large scale systems. Furthermore, basic research in model validation which addresses this issue across different modeling philosophies and methodologies could be carried out. This is a task which is greatly needed and is inadequately handled by existing institutions.

The salient characteristics of basic research to be carried out by the National Systems Center include; breadth of scope addressing methodological issues that cut across alternative approaches, a pluralistic attitude which searches for fundamental underlying principles in the study of large scale systems, an exploratory philosophy with the objective of generating interest in research topics both among researchers across the country and in funding organizations, and a focus toward future needs as opposed to review of past problems. Research at the National Systems Center would strive to develop interplay between realistic problems and basic research. These are vital for the development of relevant theories.

It is dangerous to enumerate at this large scale systems research topics which would be appropriate for NSC. However, let us try to name several as examples of large scale systems research appropriately handled at the center. (1) Credibility of information, distributed data bases, incomplete information, delayed information. (2) Characterization of uncertainty-impact on policy analysis and synthesis. (3) Methodology for cause and effect relations. (4) Identification of systems with changing parameters and structure. (5) Theories of aggregation and decomposition including fast versus slow phenomena and weak dynamic coupling. (6) Organizations under multiple criteria, dynamic teams and games. (7) Conflict resolution theory. (8) Stability of large scale systems, interconnection properties of stable subsystems and global stability. (9) Selection of planning horizons and discount factors. (10) Coordination, centralization and decentralization. (11) Group decision making theories.

Although NSC should undertake both methodological and model development, emphasis should be placed on methodological development, not on the building of models which carry NSC labels. Methodological development may proceed in four areas.

- 1) Established methodologies including assessment, evaluation, and guidelines for applicability.
- 2) New methodologies including assessment, evaluation, identification of current and future needs, and formulation of research objectives.
- 3) Model verification including definitions and procedures.
- 4) Model validation including research into precisely what constitutes validation and how it can best be carried out.

NSC will need to have access to models in support of methodological research and should be encouraged to take a pluralistic attitude toward modeling approaches. It should provide a forum for creative dissent concerning alternative modeling approaches.

It will be important that NSC maintain models for use by its staff and others and therefore would end up serving as a "foster parent" for models developed elsewhere. This implies that models residing at NSC must be transferable and adequately documented. NSC would provide opportunities for adequate testing and verification and these models will be generally accessible for research. It will be possible to provide some sort of peer review as with most other scientific contributions.

A systematic accumulation of experience, development of data on the models, and set of case studies will be undertaken. NSC can serve as the focal point for the development of educational programs in the field of modeling. It can provide the environment for constructive interaction between proponents of alternative methodologies, as well as providing a forum for decision makers and model users and the means for constructive interaction with modelers. It seems that NSC could contribute to increasing the credibility of modeling and models with users and decision makers by evaluating models, providing an independent verification of model results, helping to develop standards for model documentation and developing standards for the documentation of results of studies incorporating models.

Issues relating to modeling research include model building and testing, model choice and linkage, model verification and validation and model utilization. Under the topic of model building and testing one should look at questions of complexity which relate to how comprehensive large models are constructed, model dynamics which questions the most effective ways for capturing dynamic system behavior, and uncertainty in which problems of objective versus subjective probabilities must be studied. It is important to consider limitations and pitfalls of linking models of the same kind, and also linking models of different types. This is especially true when model development has been carried out for different purposes (e.g. technological forecasting and economic forecasting). One would like to develop operational guides for model verification and validation for both users and modelers. Finally, model use has tended to be rather ad hoc. We must begin to design controlled experiments of model utilization and thus begin to turn large scale systems modeling studies into an experimental science.

The committee has found little opposition to the need for more basic and applied research in the area of complex physical systems, which is the main area of research of the proposed National Systems Center. Most of the scepticism has been directed toward the activities of the proposed NSC with respect to complex national models.

Many systems engineers tend to view both systems-dynamics based models and econometric based models with a great degree of scepticism. They freely admit that the available tools are inadequate and that more basic research on the methodology of modelling has to be done. The committee feels that the NSC can indeed contribute to the development of such a modelling methodology. However, the right mix of staff (modellers, economists, decision makers, mathematicians, systems engineers etc.) is not enough; one has to have a suitable "laboratory" to reconcile available philosophies as reflected in the models.

Some, but not all, modellers view the modelling activity and "model peer review" with concern. The committee appreciates their vested interests and concerns. Nobody will be forced to provide his model to the NSC. For those who do, there will be a mutual benefit since their ideas will contribute to the development of a future "modeling science" even though their models have certain shortcomings.

INSTITUTES, INFORMATION DISSEMINATION, AND EDUCATION

The unique educational function of the NSC will be carried out at various levels. The NSC will provide the environment for constructive interaction between members of different constituencies such as government, academia and industry, who are all concerned with addressing specific national needs. It will provide a forum for decision makers and model users through which they could address the research community, and also provide a means for constructive interaction with researchers. It can foster constructive interaction between proponents of alternative approaches to large scale system problems and can develop educational programs and aids for government, industry, universities, public consumption and private organizations.

One specific suggestion for a research and educational activity of the NSC is the development and operation of annual institutes. The purpose of these institutes would be to set directions for future research that would contribute to the solution of future mission-oriented problems. This is particularly important because researchers who are establishing tools relevant to future problems are not necessarily aware of what the future problems will be. Policy and decision makers in government and industry can contribute to specifying the future problems, but they are not always aware of the capability of the systems researchers. Although conferences and symposia tend to be helpful in disseminating current results, they generally provide little guidance to future needs.

The institutes are envisioned as being annual affairs in which the key problems are studied in depth by a selected group of researchers, decision makers and users. They would be multi-week meetings including a staff of core participants and part-time participants. The location of the institute could change from year to year. The idea of an opportunity for in-depth study on both tutorial and research levels in an environment conducive to uninterrupted in-depth discussion is an appealing one and is a format for education and information dissemination not presently used in the systems community.

The fact that these institutes will alternate locations can be very useful to both state and local government officials, local industry, and neighboring universities. Local decision-makers and researchers alike can only benefit from the occurrence of such well organized colloquia.

(The committee feels that in the short-run (3 years) the organization and funding of such institutes, carrying the NSC label, will be the easiest and most appropriate way of getting started. However, a great deal of thought and preparation has to be given to the organization and agenda, as well as to the staffing of such institutes; at least a year of careful planning and preliminary research has to be carried out.

The committee feels that these annual institutes are necessary but not sufficient. The outcome of an institute will be a report that "may gather dust". The existence of the NSC will provide the necessary follow-up to the institute findings through additional research, so that influence upon the public and private sector is maximized.

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ORGANIZATIONAL ASPECTS

The committee devoted very little time to the proper organizational aspects of the NSC. It recognized that a much more in-depth study of the organizational design is required. However, such an organizational design requires time commitments and funding not available at this time. Due attention to the organizational design of the NSC is necessary because

- (a) initial funding should not be solicited only from NSF but from many government agencies
- (b) the desirable geographical distribution of the satellite research centers will create organizational problems
- (c) the further geographical distribution of the annual institutes will require careful management

In the remainder of this section we outline the preliminary committee recommendations.

The charter of the NSC would be established as the result of a proposal solicitation effort. The organizations submitting proposals for establishing the NSC would develop its charter as part of their proposal effort. The Ad Hoc committee expects that the best institutional arrangement will be a non-profit organization of a somewhat unusual sort. A Board of Governors will be necessary to provide overall policy guidance for the operation of the NSC. This board, made up of eminent systems research specialists from a variety of disciplinary backgrounds, would ultimately be responsible to funding agencies for setting NSC policy and directions. The Director of the NSC will be the key staff member of the entire operation. He should be an individual experienced in the development and management of research with a background which insures that he understands the nature of the research as conducted in various types of institutions in the United States. His offices should be in a facility in Washington, D.C. where he will have access to mission-oriented agencies and decision makers who have need for the results of NSC research. At this Washington facility there should be a small staff including people responsible for maintaining a library of models and related materials. The Washington staff must maintain close communication with Associate Directors and their staffs and must include liaison people who will maintain contact with policy makers and others.

The Associate Directors of the NSC will be geographically distributed at the various centers of excellence of systems research throughout the country. It is envisioned that large scale projects will be maintained at appropriate geographic centers, such as universities, research institutes and industries where the expertise currently resides or could be conveniently collected. Directors of these large scale projects would be Associate Directors of the NSC and would be phased in and out of NSC operations as their research was phased in and out. These large projects, presumably funded over an extended period of time, would be selected by the NSC Director and Board of Governors in response to proposals submitted by these research groups. Once such a project is funded, the Director of the project becomes an Associate

Director of NSC. The committee felt that it would be only inappropriate but also impossible to try to establish these projects in a single location such as Washington, D.C. The U.S. does have several existing centers of excellence and their expertise should be utilized. Such a geographical distribution and active participation of existing centers of excellence will minimize the probability of failure of the NSC and will guarantee access to the latest state of the art.

As has been indicated before, significant systems research projects require substantial funding for extended periods of time. It seemed unreasonable to expect that major contributors in the systems field would be prepared to spend extensive periods of time away from their home institution, and therefore this distributed geographical structure is proposed. With this structure in mind, it is seen that the NSC Director and Board of Governors attract the money to NSC and distribute it to these rather large scale projects being conducted in the various geographically distributed centers. The continual process of program evaluation and guidance for future work must emanate from the NSC Director's office. Programs which are recommended by the Director and approved by the Board of Governors should have a mix of funding styles. We envision approximately 50% mission-oriented, 40% funded by the Director to facilitate the technical growth and user-response capability, and 10% of a rather blue sky nature. All of these efforts would be funded in the same way: namely, NSC would obtain outside funding and solicit proposals in the areas of interest. Unsolicited proposals submitted by outside organizations could, of course, also be considered if they fit into the program objectives set by the Director and Board of Governors.

Initially, the NSC should be funded by the U.S. government. It is essential that long-range funding be committed. The National Science Foundation should fund only part of the research effort; the remainder of the funding provided by different government agencies. All funding should be sole source. Industrial and foundation funding should be actively solicited in addition to government funds necessary for the annual institutes can be provided by the agency or agencies in competitive bidding. It should not be viewed as being competitive with universities or industry.

SUMMARY JUSTIFICATION

Let us again review why we need a National Systems Center and why the proposed center is better suited to address the needs and problems discussed when there already exist numerous universities, research institutes, government agencies and industries attacking various subsets of these problems.

Current research in the United States resides in narrowly focused disciplines. Existing institutions in general do not have the appropriate organizational structure, stability of resources, and incentive system to effectively analyze complex dynamic interdisciplinary systems. Government agencies are beginning to recognize the need for a sustained long-range high quality effort to create necessary interaction among relevant disciplines and large scale systems and to extend

current methodologies and develop new ones to address major national problems and ultimate policy issues.

One might ask about existing alternatives to the proposed structure. Government research laboratories carry out research and monitor research in support of the mission of the department with which they are associated. This includes the Department of Defense, Department of Energy, Department of Agriculture, National Aeronautics and Space Administration, etc. They are not chartered to address issues that cut across department boundaries. These are often common issues that are not well understood.

Extramural research is carried out through RFPs that are problem specific with a narrow focus and mostly short range in nature. Time constraints, in the preparation such RFPs, and in the responders to these RFP's, often preclude the use of state-of-the-art technology.

The National Academies of Science and Engineering have the ability to bring together, for short periods of time, top experts on a wide range of disciplines to make analyses and recommendations on current issues. They often identify research needs, but do not conduct the research. This is quite different than the Academies of Science of other countries. There is nothing to suggest that the American Academies will change this mode of operation.

An individual university, which conducts substantial research activities in a variety of areas, cannot command the resources of the national systems community for a sustained long-term effort. Existing university departmental organizational structures do not provide necessary incentives for the faculty to engage in serious long term interdisciplinary research of the type needed for the resolution of large-scale systems problems. Industry also conducts research, but its goal is related to a profit motive. One could argue that there is indeed a profit motive in the solution of large-scale systems problems. Nevertheless it is very difficult to justify a particular company spending the kind of money and effort that would be required to resolve such problems.

Professional technical societies rely on volunteers. Committees can be organized to draw upon existing knowledge and make recommendations, but certainly cannot be expected to conduct research and development.

Non-profit organizations, as they presently exist, cannot command the resources of the national systems community for a sustained long-term effort. Moreover, they are not geographically distributed in a way needed for the success of systems research efforts.

The above discussion points out the conclusions which must invariably be drawn when examining the existing alternative institutional structures for conducting the work of a National Systems Center. The National Systems Center would essentially be a non-profit organization with an organizational and geographical structure that would permit this type of work to be carried out. It is envisioned by the Ad Hoc NSC Committee that the eventual institutional design for the National Systems Center would indeed be some form of a non-profit organi-

zation.

The National Systems Center, as conceived above, would be a unique national resource because of the combination in one organization of two key attributes. 1) A primary emphasis on development and documentation of methodologies for decision making and policy analysis and syntheses in those national and regional problem areas where a comprehensive systems description or knowledge is essential. 2) A sustained, stable, and sufficient effort by top researchers and experts in the systems and policy fields cooperating with universities, industry, government and other research organizations to affect a major advance in the state-of-the-art of this field.

The distinctive attributes of the proposed National Systems Center include the accumulation of models and case studies, peer review which will contribute to experimental evidence of data essential for defining methodological research, development of guide lines or standards for model documentation, and availability of advice to decision-makers on the state-of-the-art in large-scale systems studies. Strong interactions with the systems community will ensure that the most appropriate methodology is used in any mission oriented study. At the present no such institutional structure exists in the United States. The National Systems Center will address issues in more depth than the Academies of Science or Engineering and with a more holistic flavor than is carried out by existing research institutes, industry and universities. NSC will have a focused basic research component and the research findings will be accessible and widely disseminated.

Thus the Ad Hoc NSC Committee believes there is an urgent need in complex national systems which requires a new organizational structure to carry out basic and applied research in the areas of large-scale systems, modeling, policy analysis and policy synthesis. The proposed National Systems Center represents a unique match between national needs and organizational design.

FUTURE PLANS

The ad-hoc NSC committee feels that its recommendations should receive wide dissemination before any additional formal steps are taken. Since we propose an extremely high quality staffing, such a proposal must have the active support of the scientific and engineering community. For 1979 several presentations are planned at conferences, government agencies, industries, and universities.

Even in the initial phases, funding is necessary to

- (a) define and carry out the first annual institute, probably in the summer of 1980
- (b) carry out a more detailed organizational design.

Comments are solicited from all concerned. Please transmit them to Professor Michael Athans, Room 35-308, M.I.T., Cambridge, MA, 02139.

ACKNOWLEDGEMENTS

It was mentioned that, the work that has gone into the development of the proposal contained in this report is the result of efforts by a large number of people. The following individuals have been the active contributors to the Ad-Hoc Committee for a National Systems Center. Travel support was provided in part by National Science Foundation Grant NSF/ENG77-07777. The nature of the discussion in this report and the conclusions should be attributed largely to the work of this committee, and do not necessarily reflect those of NSF.

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