

**U.S. DEPARTMENT OF COMMERCE  
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AD-A025 791

APPLIED MATHEMATICS

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APRIL 1976

176066

Technical Report JSR 75-13

April 1976

ADA 025 791

# APPLIED MATHEMATICS

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Contract No. DAHC15-73-C-0370  
ARPA Order No. 2504  
Program Code No. 3K10  
Date of Contract: 2 April 1973  
Contract Expiration Date: 30 August 1976  
Amount of Contract: \$2,062,014

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ARPA ORDER NO. 2504

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| REPORT DOCUMENTATION PAGE  |                       | READ INSTRUCTIONS<br>BEFORE COMPLETING FORM                 |                        |
|--|-----------------------|---|------------------------|
| 1. REPORT NUMBER<br>JSR 75-13  | 2. GOVT ACCESSION NO. | 3. RECIPIENT'S CATALOG NUMBER                               |                        |
| 4. TITLE (and Subtitle)<br><br>APPLIED MATHEMATICS   |                       | 5. TYPE OF REPORT & PERIOD COVERED<br><br>Technical Report  |                        |
| 7. AUTHOR(s)<br><br>K. M. Case    A. Despain    F. J. Dyson    J. Katz   |                       | 6. PERFORMING ORG. REPORT NUMBER<br>Project 3000            |                        |
| 9. PERFORMING ORGANIZATION NAME AND ADDRESS<br>Stanford Research Institute<br>333 Ravenswood Ave.<br>Menlo Park, California 94025  |                       | B CONTRACT OR GRANT NUMBER(s)<br>DAHC15-73-C-0370           |                        |
| 11. CONTROLLING OFFICE NAME AND ADDRESS<br>Defense Advanced Research Projects Agency<br>1400 Wilson Blvd.<br>Arlington, Virginia 22209   |                       | 10. PROGRAM ELEMENT, PROJECT, TASK AREA & WORK UNIT NUMBERS |                        |
| 14. MONITORING AGENCY NAME & ADDRESS (if diff. from Controlling Office)  |                       | 12. REPORT DATE<br>April 1976                               | 13. NO. OF PAGES<br>41 |
|  |                       | 15. SECURITY CLASS. (of this report)<br><br>UNCLASSIFIED    |                        |
|  |                       | 15a. DECLASSIFICATION/DOWNGRADING SCHEDULE                  |                        |
| 16. DISTRIBUTION STATEMENT (of this report)<br><br>Approved for public release; distribution unlimited   |                       |   |                        |
| 17. DISTRIBUTION STATEMENT (of the abstract entered in Block 20, if different from report)   |                       |   |                        |
| 18. SUPPLEMENTARY NOTES  |                       |   |                        |
| 19. KEY WORDS (Continue on reverse side if necessary and identify by block number)<br><br>Applied mathematics                      Multiphase flows<br>Symbolic manipulation                    Nonlinear partial differential equations<br>Special functions                          Summer institutes   |                       |   |                        |
| 20. ABSTRACT (Continue on reverse side if necessary and identify by block number)<br><br>An attempt was made to respond to an ARPA request for ideas concerning ways of maintaining a fruitful STO/6.1 community relationship. In order to test possible formats for helping ARPA we have chosen a specific field--applied mathematics. In order to have a definite framework we have restricted attention to possible programs that would cost less than $\sim \$2 \times 10^6$ /year and require 3 to 10 years to get significant results. Actually most of the projects are in the few $\$10^5$ range and could be of approximately 5 years duration. |                       |   |                        |

## 19. KEY WORDS (Continued)

## 20 ABSTRACT (Continued)

We suggest a number of projects that would lead to useful and important results. The list is, however, by no means exhaustive.

The principal recommendations are:

- (1) Upgrading and making portable, systems for the symbolic manipulation of mathematical expressions (Macsyma, Reduce).
- (2) Handbooks on Special Functions
  - (a) Revision and additions to the work "Higher Transcendental Functions," by A. Erdelyi, W. Magnus, F. Oberhettinger, and F. Tricomi.
  - (b) Rewriting the National Bureau of Standards Handbook of Mathematical Functions.
- (3) Summer Institute on Applied Mathematics.
- (4) Work on computing algorithms.
- (5) Support of work on multiphase flows.
- (6) Support of work on nonlinear partial differential equations--with emphasis on soliton behavior.
- (7) Creation of a specialized Applied Mathematics Institute. This work would deal with a restricted set of problems of importance for national security. The model is CRD in Princeton.

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## I INTRODUCTION

This report is an attempt to respond to an ARPA request for ideas concerning ways of maintaining a fruitful STO/6.1 community relationship. We remark that this is a good time for ARPA to be reconsidering this question. The financial problems of the Universities today suggest the possibility of getting much more basic research per dollar there than could be obtained from FCRC or private contractors. (The Universities have the talent, and because of economic and political changes they would probably be receptive.) However, because of the special nature of some problems we will also consider means of fostering research outside the Universities.

In order to test possible formats for helping ARPA we have chosen a specific field--Applied Mathematics. This choice was made since it is close to our particular field of expertise. The attempt was made to see where and how money invested by ARPA would yield a significant return over a finite number of years.

In order to provide a definite framework, we have asked what ARPA could get for something like  $\$2 \times 10^6$  per year for a period of 5 to 10 years. This is somewhat arbitrary but does influence our considerations in the following ways: An order of magnitude less money would not really be a program. An order of magnitude more would lead to a consideration of many possible programs that we have deliberately ignored--for example, the design and construction of large special-purpose computers. The time frame was chosen because it is thought that anything requiring much less than 3 years is hardly basic research. Anything requiring much more than 10 years is probably not an ARPA problem. In this connection we come

against a fundamental question. If we are talking about important basic research in the nation's interest it might be expected that it would be supported by other agencies, such as NSF or NIH. However, given human and financial limitations we recognize that there are areas important for ARPA that are not sufficiently pursued. Conversely, there are areas of importance to ARPA that we feel are adequately supported and hence we have not considered them here. (One example is information theory.)

The input for this report comes from three sources:

- (1) Extensive discussions among ourselves.
- (2) An informal conference held at Rockefeller University on 9 April 1975. Present were 15 representative leaders in the field.
- (3) Detailed interviews with many active workers during the summer of 1975.

It has been found that there is considerable optimism in the field.

We quote one leader:

"The availability of really fast computers with large storage capacities, and the development of new computer methods hold out the promise of a golden age of mathematics applied to the hard sciences: there are unprecedented opportunities in all branches of mathematical physics, fluid dynamics, plasma physics, meteorology, oceanography, geophysics, population dynamics, mathematical physiology, and computational chemistry, just to mention a few."

We have asked, for what areas could support lead to significant results for ARPA? Three goals have been set: Educational, Methodological, and Specific Problem Solving. The ordering is arbitrary. As will be seen, they are usually interrelated.

Below, besides a general discussion in the various areas we give some details and indicate how specific programs could be implemented. The list of problems we consider is by no means exhaustive. They are, however, representative and important.

When we compare the various problems that we discuss, some generalizations appear. Thus there seems to be a critical size for groups working on them. There is a need for an Applied Mathematician, a Numerical Analyst, a Computer expert, and an expert in the particular field to be working together. Further, there is a need for contact with real problems. For example, considering the amount of computing being done, the efficiency of the algorithms used is all-important. It is felt that these algorithms are best developed in the context of a real problem.

In Section II we discuss methodology. Essentially, this is the development of tools that will increase the efficiency of people in solving particular problems. "Educational programs" as used in Section III means attempts to train people who will work on ARPA problems, to present specific problems to people with appropriate skills, and to keep ARPA management aware of significant new developments. Section IV is devoted to a number of important problems on which major progress could be expected in the 5-to-10-year time frame indicated above. The point of Section V is to indicate various organizational ways of implementing the programs described in this report.

## II METHODOLOGICAL PROGRAMS

We have considered a number of ways to materially improve the ability and efficiency of people solving the mathematical problems arising in engineering and science. Some of these we describe below.

### A. Symbolic Manipulation of Mathematical Expressions

The object here is to use a computer to solve applied mathematics problems analytically. To understand a solution, it is frequently much more advantageous to have an analytic expression than to have pages of numbers. Also, when the solution is near a singularity it can be very difficult to evaluate numerically. There are situations in which the sheer magnitude of the problem prevents humans from obtaining a solution-- even though it is known that there is a closed form for it. For example, there exist some quantum electrodynamics problems that require finding the trace of some 70,000 four-by-four matrices.<sup>1\*</sup> Even if a person could do this the confidence level of the result would be nil.

At present there are about a dozen programs that do some symbolic mathematical manipulation. The programs that seem to offer the most promise for the future are Macsyma (MIT),<sup>2</sup> Reduce (University of Utah),<sup>3</sup> and Scratchpad (IBM). These are rather similar. Macsyma is the most highly developed and so in describing the current status we will be principally referring to that. However, there is a sufficiently different approach to the future between Macsyma and Reduce that we will come back to this.

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\* References are listed at the end of the report.

Currently, programs can do rational function manipulation, differentiation, integration, matrix manipulation, power series manufacturing, and solving classes of differential and integral equations. The integration is for both definite and indefinite integrals--as long as the result can be expressed in terms of rational functions, exponentials, logarithms, trigonometric functions, or error functions. Problems in many areas have already been solved with these programs. Among these areas are celestial mechanics, plasma physics, fluid mechanics, structural mechanics, and numerical analysis.<sup>1,4,5</sup>

What needs to be done? Essentially one would want to increase the class of operations that can be handled, and increase the availability to users. Thus, one would like to be able to handle Bessel functions, elliptic functions, general hypergeometric functions, and more complicated integrals. While the number of desirable extensions is finite, there are many of these. However, support in amounts of \$30,000 to \$100,000 per year for a number of years for Macsyma and/or Reduce should yield significantly upgraded capability.

The problem of availability to users is multifold:

- (1) At present one needs to learn a fairly large language to use Macsyma efficiently. For example, there are a number of commands that will solve a particular problem. To know which algorithm is most effective for a given problem, at present one needs a detailed knowledge of the manual to decide. It has been estimated that \$100,000 per year for 2 years could materially simplify Macsyma for the user.
- (2) These are very large programs. The Macsyma approach is to simply add on capabilities at MIT. The hope is that eventually (1985?) cheap minicomputers will be available with large memories on which Macsyma can be run (not on a time-sharing basis). The other approach (that of Hearn) is to adapt Reduce to large existing computers around the country. This is an important question and one that we are not now in a position to answer.

Essentially the answer depends on how optimistic one is about the future costs of fast memory. If symbolic mathematical manipulation is to be pursued, a committee should be set up to try to settle the question.

We would like to emphasize that symbolic manipulation should be very much pushed. It could revolutionize the education and work habits of maybe  $(10 \text{ to } 100) \times 10^3$  engineers and scientists in the country. (It would tremendously increase their efficiency.)

#### B. Handbooks on Special Functions

The members of our group are in unanimous agreement that the existence of handbooks describing the properties of Special Functions has been invaluable for the application of mathematics to the sciences and engineering. The wide availability of computing facilities has increased, rather than diminished, the need for a comprehensive and reliable compendium of the analytic properties of special functions. Unfortunately the existing handbooks are in some areas seriously incomplete and out of date. As an example, one of us had to use spheroidal functions in his own research work recently. He found the necessary information scattered in various journal articles, mostly recent and not easy to find. Almost none of the information is in the books.

A short description of the existing books is perhaps in order. The first comprehensive books of facts on tables of the higher transcendental functions were published shortly after the turn of the century. In 1902 E. T. Whittaker published A Course of Modern Analyses. Subsequent editions with G. N. Watson culminated in the 4th edition in 1927. A short summary of useful facts, with no derivations or proofs, was given by W. Magnus and F. Oberhettinger in 1943. The current standard reference books were published in the early 1950s. These are the three volumes,

Higher Transcendental Functions, (or HTF) by A. Erdelyi, W. Magnus, F. Oberhettinger, and F. Tricomi.

There is a second type of book, which was first typified by Jahnke and Emde, Funktionentafeln mit Formeln und Kurven, 1909, republished with minor revisions up till 1945. The modern version of this is the Bureau of Standards Handbook of Mathematical Functions (or AMS 55), first printed in 1964, edited by Abramowitz and Stegun, with chapters written by various people. These books contain summaries of properties, tables, and graphs. The latter is a mixed book. Some of the chapters are well written, while others are inadequate.

We remark that the books we have mentioned are among the books most widely cited by users of mathematics. However, it has been estimated that the knowledge of special functions has increased by some 50% since HTF was published. It would seem that great need would be served by updating this.

Some idea of the magnitude of work required is obtained by noting that production of HTF ("the Bateman Manuscript Project") required twelve man-years of time from the senior mathematicians, and an additional seven man-years of time from the research assistants. New volumes should take about as long to complete.

One of the main difficulties in a project of this sort is finding people to do it. Fortunately we have seen a detailed proposal to do this from Professor Richard Askey of the University of Wisconsin.<sup>6</sup> This also enables us to be rather specific as to what might be done, over what period of time, and for how much money.

In essence, Askey proposes a five-year program costing of the order of \$300,000 per year. Approximately four books would result. Topics to be included are:

- The classical orthogonal polynomials.
- The classical hypergeometric function  ${}_2F_1(a,b; c; x)$  and the confluent hypergeometric function  ${}_1F_1(a; c; x)$ .
- Bessel functions.
- Other hypergeometric functions.
- Elliptic and theta functions.
- Solutions to named differential equations--e.g., Lamé functions, spheroidal wave functions, and Mathieu functions.
- Gamma functions.

In addition, shorter versions of the material in the books would be written and published in the National Bureau of Standards Journal of Research. These papers will include more on the computational aspects than would be given in the larger books. Eventually these would be compiled together to form a new version of AMS 55.

The plan of the project that Askey describes in his proposal seems sound. While one may not share some of his personal enthusiasms, they can be respected. Obviously it is right that each section of the work be written by an enthusiast for the particular functions he is covering. Some difficulties may arise if it turns out that each author wants to write three volumes on his own pet functions. The project will probably need an editorial advisory committee with authority to adjudicate disputes and set some ultimate limits to the output of paper. It is important that the advisory committee and the principal investigator agree to a well-defined statement of their respective spheres of authority before such a project is funded. Another activity of the Advisory Committee might well be to try to help limit the resulting cost of the books to the user. Only if they are relatively inexpensive, and thus readily available, will they serve a useful function. (It has been estimated that the replacement of AMS 55 could be marketed for between \$7 and \$10. This is probably acceptable.)

In summary, we strongly recommend that ARPA support such a project. It is again an area in which a significant improvement of the efficiency of mathematical work for DoD would be achieved.

C. An Amalgam

One very exciting idea would be to combine the suggestions of Sections II-A and II-B, above. Essentially one would have the improved HTF available on a computer. Dramatic improvement in doing analysis and numerical calculations together would result. This, however, is presumably a much larger project than suggested in either Section II-A or II-B separately. A small study as to feasibility and cost of such a project would be well worthwhile.

D. Computing Algorithms

Clearly, for computer efficiency the algorithms used are all-important. Considering the amount of money spent by DoD on computing, almost any significant improvement in algorithms would pay for their development many times over. In the last five years a new set of ideas has proved unexpectedly efficient. These include fast transforms, fast inversion methods, and alternating-direction and fractional-step methods for integrating partial differential equations. The Applied Mathematics community thinks that all sorts of surprises are in store--with big payoffs.

The question arises, how to develop these? This is difficult, since what one is saying is that one wants bright ideas. Some possibilities are:

- (1) As indicated earlier, it would seem that such algorithms might most readily develop in the context of a real specific problem. Then, in a large program (such as those sketched below) a certain amount of freedom, approximately 25%, might be left for people to search

for new algorithms rather than plunging ahead to obtain the most direct brute-force result.

- (2) Five-year career awards for bright, interested people. They would work in the environment of some large computing center--e.g., the Courant Institute.
- (3) A group of possibly approximately four Ph.D.s might be set up at some institution (e.g., the Mathematics Research Center at Madison) to
  - (a) Make comparisons of numerical algorithms with regard to efficiency, accuracy, and convenience
  - (b) Look for new algorithms for parallel computers
  - (c) See if complex languages have consequences for machine design.

E. Information Retrieval Systems

It has been suggested that ARPA fund a several-year project to reduce literature search in various commonly used areas to computer-accessible form. While it is fairly obvious that such availability would save time and money, the main point here is that this would be particularly useful if done for very limited areas--e.g., matrix inversion techniques. It seems that very large library systems are too awkward to deal with conveniently.

These would be very limited projects, requiring of the order of one Ph.D. plus a few clerical workers. Presumably this could be done easily at Universities.

### III EDUCATIONAL PROGRAMS

There are a number of goals of such programs:

- (1) To introduce problems of interest to ARPA to people who would not otherwise know of them and yet might have the expertise to contribute to the solution.
- (2) To get people in various fields together to maximize communication across disciplinary boundaries. (For example, the theory of signal detection has recently found application of the abstract mathematical theory of Martingales to be fruitful.)
- (3) To acquaint ARPA contractors and management with significant advances in theory and techniques. Several programs have been suggested.

#### A. Summer Institutes

Very considerable interest has been shown in the idea of Summer Institutes or Symposia. Experience has shown these to be of great value. In Appendix A we give the details about three such. These are roughly similar. They involve approximately 30 people and a period of 6 to 10 weeks, and cost on the order of \$100,000. The differences between these institutes are mainly in the emphasis on communication versus problem solving, and in the specificity of the problems.

It is thought that such Institutes could have a significant impact on ARPA problems at relatively small cost.

Two mechanisms for implementing such symposia have been suggested: One (modest) would involve setting up a small steering committee that would keep an eye on promising developments and that would organize the summer sessions. This group could also serve the function of recommending

further support of certain lines. The second (more ambitious) would have the Summer Symposium run by a possible new ARPA Institute (see Section V). From the present point of view the main advantage would be that momentum built up on a problem during the summer could be maintained. Also, people who did continue to develop the work started during the summer would have a place to inject their results into the system.

B. Review Articles

With increasing specialization and the highly abstract and condensed form in which pure mathematics is written in the United States, there is a striking need for review articles "translating" important new results into a pedagogical form. Such could appear in a review journal or commissioned monographs. The result again would be to make the latest technique available to the ARPA community.

#### IV SPECIFIC PROBLEMS

We consider here a number of problems of importance for which it is felt that significant progress could be made in a 5-year program.

##### A. Multiphase Flows

This subject pertains to the relative motion between materials that can occur whenever there are bubbles, droplets, or chunks embedded in a fluid. A typical example is the flow of a water-steam mixture. Practical applications are almost innumerable. For example, most processes in the chemical and casting industries involve these flows. Applications of immediate importance to the DoD include combustion processes in interior ballistics, combustion in engines of various kinds, fuel-air explosions, flows involving cavitation, smokescreens, the mixing phase of Rayleigh-Taylor instability in several circumstances, and seismic coupling in porous media. One specific example is the interior ballistics of liquid-propellant guns. The propellant flow is a mixture of liquid and gas, burned and unburned phases. There is an interface with a complicated geometry, and droplets of each phase are mixed into the other. At present, the functioning of liquid-propellant guns is very unreliable. If better understanding of the interior flow and combustion leads to a resolution of these problems, the benefits will be substantial.

Theoretical work in the field has concentrated on formulating the basic equations, finding macroscopic representations of the microscopic exchange processes, and finding techniques for solving practical problems.

The most activity in the field seems to be at Los Alamos<sup>7</sup> and Aero-Jet General. Work at these places has been mainly in the direction of

reactor safety. The question is, will the emergency cooling system work? Governing equations have been written down and a number of numerical codes have been written. These involve some semi-phenomenological terms that can be varied. The largest of these can handle two space dimensions (axial symmetry) and time. It seems feasible to extend these to three space dimensions. Relatively few computations have been done and even less comparison with experiment has been made. This is an important lack. As indicated, the interactions are modeled--not really derived from fundamental principles. It does not seem likely that a basic theoretical solution for realistically complicated situations will be available in the foreseeable future. The problem is much like that in practical turbulence calculations. We take reasonable model equations with adjustable parameters. When the equations are adjusted to a given class of experiments we expect them to then predict the behavior in flows that are not too dissimilar from those of the experiment. What is needed?

- (1) There is some question as to the validity of the basic equations. A controversy as to whether these are well posed mathematically is not adequately settled in the literature. This is a problem for an applied mathematician-hydrodynamicist.
- (2) The proper turbulence modeling needs to be included.
- (3) Much more work needs to be done on the microphysics. What are the effects of contaminants, bubble growth, bubble size distribution, and the breakup of interfaces?
- (4) Modeling of mixing flows need to be done. For example, one should be able to treat the effects of Taylor instability in laser pellet fusion.
- (5) Means to describe instabilities such as those in liquid propellants are needed.
- (6) Modeling to treat the problem of surface breakup is needed--in particular, means to describe smoothly the transition between the various regimes.
- (7) Experiments are essential. As emphasized above, the theories are phenomenological. Detailed comparison with predictions must be available.

A program to attack these problems would involve something like 4 to 5 experienced people and would cost of the order of  $\$(200 \text{ to } 250) \times 10^3$  per year. These people would be working on various aspects of the modeling problem. However, it would be desirable that they be in close enough contact so that there could be considerable interaction.

What could one expect to achieve by a 5-year program of this kind? A reasonable prediction is that one will have a significant capacity to treat a large class of these problems.

There are some caveats. Unfortunately, many problems involve ranges of parameters or complex combinations of processes that are not accessible to simple experiments. This can be a serious limitation on the utility of multiphase flow calculations. An example in which this may become a serious problem is the liquid-propellant gun, where turbulent flow, heat transport, and complex chemical processes are tangled together. As the recently developed codes and numerical techniques are applied to a variety of problems, results will be mixed. In some cases much better understanding and predictive capability will result. Even in the best cases, this will require work on code development and the detailed physics over a period of years. The various areas of applications of these techniques are different enough that a single standardized code is not likely to be useful. There will also be problems that will be intractable even after several years of effort because of the complexity and difficulty of the processes involved. It is not possible to predict in advance which problems will permit a successful computational attack, although it seems fairly clear that the simpler problems can be treated.

#### B. Nonlinear Partial Differential Equations

In Section IV-A we discussed nonlinear partial differential equations (n.l.p.d.e.). There, however, the emphasis was mainly semi-phenomenological

and numerical. In this subsection the emphasis is more fundamental. The point is the following: In the last ten years a large class of n.l.p.d.e. has been found that exhibits remarkable solutions known as solitons. (For a comprehensive review, see A. C. Scott et al.<sup>8</sup>) These are localized highly stable solutions. (Stable, in that after collisions between them they emerge essentially unmarred.) They are intrinsically nonlinear phenomena. In the linearized limit they disappear. (The origin is in a balance between nonlinear and dispersive terms.) They are general in that at least for some equations the solution of the general initial value problem is asymptotically only a sum of solitons. It is interesting that they were first discovered numerically, and only afterward were they described analytically.

Areas in which these solutions have been obtained include the theory of water waves, plasmas, laser fusion, laser propagation, transmission lines, and transport properties of solids.

At present quite a bit is known about a number of one-dimensional problems. These are primarily exceptional cases in which the systems are conservative and admit an infinite number of constants of motion. In some cases it is known that a small amount of dissipation does not affect the stability materially. However, the limits are not known. Also, the stability of systems admitting soliton solutions but not having an infinite set of constants of motion is not known. In two (and three) dimensions, soliton-like solutions are known to exist in conservative systems in which in addition to nonlinearity there exists dispersion or diffraction. For the nonlinear Schrödinger equation (this is appropriate for various laser problems) soliton-like solutions appears in two dimensions (at least). Such solutions again seem to play a significant role in the behavior of the general solution.

What is needed here are a few projects of the following kind: One to two senior investigators with access to a large computer (e.g., a 7600) at a cost of approximately  $$(50 \text{ to } 100) \times 10^3$ . Expected results for a 5-year program would be:

- (1) A complete understanding of the one-dimensional case. Thus we should know what classes of equations exhibit such behavior, when the solutions are stable, and what (realistic) modifications maintain or destroy these properties.
- (2) Much wider knowledge of the existence of these things in higher dimensions and some idea of the stability properties.

Why should this matter? We note that this may be a far-out approach with limited application. The soliton behavior may be atypical. However:

- (1) Solitons may be a fairly general property. It may be possible to interpret the solutions of many problems in terms of solitons and their interactions. They may play a similar role to that of shock waves.
- (2) In addition to the fact that solitons occur in the solutions to some idealized equations in the fields discussed above, it is likely that many kinds of devices can be based on the concept. The following are a few that have been suggested:
  - (a) The Japanese<sup>9,10</sup> have demonstrated theoretically and experimentally that encoded radiation and reception of messages is possible using solitons.
  - (b) It has been suggested that solitons could be useful in transmission along optical fibres. The point here is that we want short pulses for a high rate of information. The stability of the soliton prevents the usual disturbances due to dispersion. One application might be small transmission lines in buildings (of interest to the telephone company). Another might be to remove sensitivity to EMP in missile guidance systems.
  - (c) Proper use of the soliton concept could be quite important for optimum amplification in lasers (e.g., the CO<sub>2</sub> lasers).

- (d) Solitons might be of importance in detecting disturbances in the ocean.

Note: We have not investigated the cost effectiveness of these ideas. The point is that there may well be a wide variety of devices based on the concept.

Here we have concentrated on the soliton-type behavior of solutions of n.l.p.d.e. This is merely because this is something about which there is now a significant amount of knowledge and much present activity--though too little numerical work for general basic understanding. There are equations that show other interesting properties that might well be studied. For example, it is believed that there are situations where the stable configuration for nonlinear equations is an arrangement of vortices.<sup>11</sup> The nervous system and detonation processes also show pulse-like properties that are, however, not exactly of the soliton type.

### C. Turbulence

This again is a problem involving n.l.p.d.e. However, the occurrence is so general and so important that it deserves a few remarks:

- (1) It has been suggested that a study be made of the possibility of a special-purpose computer for turbulence calculations. The main questions are:
  - (a) What are the consequences of anticipated (circa 1985) technology?
  - (b) What are the consequences of approaching the problem in a somewhat different way? Namely, suppose one first asks for the best computing algorithms for the problem and then asks for the computer to implement these?

This would be a rather limited study (of the order of one year). Involved would be approximately four people: an applied mathematician, a hydrodynamicist, a numerical analyst, and a computer designer.

- (2) Recently there have been some attempts to attack the turbulence problem by calculations based on the interactions of vortices--treated as finite elements. At present these are quite exploratory.<sup>12</sup> Support would be for small (approximately \$50,000) University contracts.

D. Other Problems

There are a number of other areas in which significant work is going on in the Universities that should be supported because of potential impact on DoD problems and because there are developments that should be made use of by the ARPA community. Among these are:

- (1) Stochastic differential equations
- (2) Signal processing.

In addition, there are a number of problems we discuss in Appendix B in connection with a proposed ARPA Institute.

## V INSTITUTES

We have considered the desirability of an Applied Mathematics Institute. Three models come to mind: the Courant Institute at NYU, the Mathematics Research Center at Madison, and CRD at Princeton. Their budgets are of the order of  $\$5.5 \times 10^6$ ,  $\$1.3 \times 10^6$  and  $\$2.5 \times 10^6$ , respectively. As a model we rejected Courant and MRC. This is not because these are not effective organizations. Indeed, many of the recommendations we have made could well be implemented through them. Rather, we saw no reason why ARPA should be duplicating them. Instead they may well be strengthened--as would be the case if they were awarded some of the work we have mentioned.

CRD is a more interesting model. The point is to have an institution where applied mathematics, important to national security problems, which cannot be done or is not being done, can be done.

Some characteristics of CRD: There are of the order of 30 technical people. The work is in a very definite and limited area. They work on real problems and have very close rapport with the organization on whose problems they work. A summer workshop run by CRD gets outside people into their problems, gets some problems solved, and stimulates the staff. It seems to be a very effective organization.

A proposal for such an organization might go as follows: Start with 5-to-10 people and a budget of approximately  $\$500 \times 10^3$ /yr. Eventually this might grow to 20-to-30 people and  $\$2 \times 10^6$  per year. Some caveats are as follows:

- (1) There should be a well-defined mission.
- (2) There should be real problems and very close contact with the agencies whose problems they are.
- (3) The Institute should be flexible--i.e., while it would initially deal with a limited class of problems it should also be able to turn to another limited class after a period of time.
- (4) It should be located near some major University. This is to attract the type of people needed, to keep them stimulated, and to have appropriate expertise and library facilities available.

Such an Institute could very well be responsible for the Summer Symposia suggested earlier.

Some suggestions as to initial problems are given in Appendix B.

We remark that while this Institute could be very useful to the nation, it need not be purely ARPA supported. General DoD support or even support by a consortium of agencies might be possibilities.

## VI CONCLUSIONS

It should be emphasized that this study is in the nature of an experiment. We are trying to develop a format in which to make recommendations as to how basic research money may be usefully spent. Accordingly, we have made a number of suggestions. We regard these as typical and important. However, the list is far from exhaustive. Also we have described the projects in varying amounts of detail. Perhaps from this a consensus about the amount of detail that is useful will emerge.

Roughly, all the projects are for an approximately 5-year period. The costs vary between  $\$5 \times 10^4$  to  $\$2 \times 10^6$  per year. The principal recommendations are:

- (1) Upgrading and making portable systems for the symbolic manipulation of mathematical expressions (Macsyma, Reduce).
- (2) Handbooks on special functions
  - (a) Revision and additions to the Bateman Manuscript Project
  - (b) Rewriting of the National Bureau of Standards Handbook.
- (3) Possibly bringing together (1) and (2)--i.e., HTF on a computer.
- (4) Work on computing algorithms.
- (5) Summer Institutes on applied mathematics.
- (6) Support of work on multiphase flows.
- (7) Support of work on nonlinear partial differential equations--particularly with emphasis on soliton behavior.
- (8) Creation of a specialized Applied Mathematics Institute. This would deal with a restricted set of problems of importance for national security. The Model is CRD.

Appendix A

MODELS FOR SUMMER SYMPOSIA

## Appendix A

### MODELS FOR SUMMER SYMPOSIA

As models for Summer Symposia we give a brief description of three such.

#### 1. The Battelle Recontres

During the years 1967-74 the Battelle Memorial Institute financed a total of five of these meetings, with the purpose of encouraging mathematicians and physicists to understand each other's problems. The cost of each meeting was of the order of \$100,000, not including the use of the building (the Battelle Research Center in Seattle), which was provided free of charge. The ground-rules were as follows:

- (1) Overall supervision by a steering committee (half mathematicians and half physicists) who chose the director and topic for each meeting.
- (2) The director invited about 6 senior people as lecturers and chose about 24 more junior people as participants.
- (3) Participants were required to stay for the full duration of the meeting (usually 6 weeks).
- (4) About half the working time was occupied with lectures, the rest being free for private discussions and research.
- (5) Emphasis was put on finding topics and people to maximize communication across disciplinary boundaries.

The following subjects were covered in the various meetings: Group Theory, Hyperbolic Differential Equations, Differential Geometry and General Relativity, Ergodic Theory and Statistical Mechanics, Modern Mechanics. The directors were Cécile Dewitt, Marcel Froissart, Valentin Bargmann, Andrew Leonard, and Jurgen Moser.

In general the meetings were very fruitful. Many of the participants wrote letters stating that they had been decisively helped in their research by new ideas and new contacts encountered at the meetings. There were numerous examples of successful collaborations begun at the meetings and continued afterward. The Battelle management stopped supporting the meetings in 1974 because of internal financial difficulties. The need for such meetings, as a means of introducing young post-graduates to current problems in the field of applied mathematics, is at least as great today as it was when the meetings began in 1967.

If ARPA wishes to support basic research in applied mathematics, a series of annual summer meetings, with roughly the same style and format as the Battelle recontres, would be a cost-effective method of doing so.

## 2. The CRD Summer Project

This symposium brings about 20 people from various universities around the country to CRD in Princeton. A practical problem of interest to CRD is posed. The participants plus the CRD staff then work on this in various groups for approximately 3 months. The cost is ~\$120,000. CRD officials feel that they obtain more results per dollar than with anything else they do. A bonus is the stimulus given to the permanent people--it keeps them scientifically alive.

## 3. The NYU Summer Institute

This is funded by ONR through a private company--"Applied Mathematics Institute." It is run by J. Keller and H. Reese. The purpose is to get mathematicians involved in applied problems. The format is to introduce a problem and then attempt to solve it in various informal groupings. Frequent ad hoc lectures are given.

There are approximately 12 people full time for 7 weeks. In addition there are about 12 people who participate for 1 week or less. These latter are primarily lecturers or people bringing data. Travel and living expenses, but not salary, are paid. This year the program cost is close to \$60,000.

The topic this year is Ocean Acoustics. Last year it was Inverse Problems and the year before it was Nonlinear Problems.

Appendix B

REASONS FOR, AND POSSIBLE TASKS OF, THE PROPOSED INSTITUTE

## Appendix B

### REASONS FOR, AND POSSIBLE TASKS OF, THE PROPOSED INSTITUTE

In the main text we have considered the suggestion of setting up an Applied Mathematics Institute. The model chosen was that of CRD at Princeton rather than the Mathematical Research Center at Madison or the Courant Institute. As indicated, this was not in any sense derogatory to the latter two. Rather it was felt that there is a class of problems that are important and yet cannot be adequately solved in a conventional Institute environment. CRD is rather unique in that:

- (1) It works on problems that cannot be worked on in universities.
- (2) It works on real problems.
- (3) It has close contact with its customers and their data.

Why an Institute? First, there are classes of problems that could be addressed well today but are not. For example, in a discussion of the speech-understanding program it was pointed out that for some applications a word-spotting capability would be valuable. The principal investigator admitted this could well be within the capability of his program--but in his University situation he just could not work on this limited application. Another example is the HASP program. To date, the investigators have had no real data to work with. Instead they have been using made-up data. Believability in the utility of the work cannot be high unless this is remedied. The difficulty here is lack of close coupling to the potential customer.

In general, real problems have not always been appropriately addressed. The benefits to DoD are frequently offshoots of results obtained. Also,

the practical difficulties in applying idealized results may really be the crux of the problem. (As a side remark we note that attention to real and limited problems may also be of benefit to the field involved. For example, a number of the possible tasks we mention below have a considerable Artificial Intelligence aspect. This field might be much advanced by dealing with real situations.)

A second reason for an Institute is the frequent need for an interdisciplinary approach. As just stated, the problems listed below have a strong AI aspect. However, people in that field tend to start from the assumption that what is given is a perfectly typed or digitized input. They shy away from the question of how that is to be obtained. What is needed for practical systems is an integrated project that includes, in addition to AI (or Computer Science in general) theorists, engineers and signal processors.

What problems might such an Institute address? Probably there are very many. However, it is felt that the Institute should work in a restricted area. While the charter should be flexible enough to permit the Institute to evolve, at any given time the problem under investigation should be related.

The following is a list of problems that were discussed this summer:

- (1) Communication monitoring.
- (2) More generally: speech understanding in a real world.
- (3) HASP.
- (4) More generally: Integration of the outputs of the surveillance systems in existence.
- (5) The Morse Code problem.
- (6) Strategic Alert: By this we mean a system to integrate all available intelligence and then assist in evaluating the strategic situation.

These problems have enough in common to illustrate a typical coherent program. Also, they are important and we think it probable that a 5-to-10-year program would yield significant solutions. The mathematics involved would be that of statistics, information theory, electrical engineering, and computer science. (The last might well contain a significant AI aspect.)

What else might the Institute do? We suggest in the text that, if Summer Symposia are supported, the Institute could well run them. The benefits to be accrued are mentioned in Appendix A. Also, the Institute would be a natural place to develop ideas arising during the summers. The Institute would be the natural focus for workers in the areas involved.

It is thought that, properly implemented, an Institute as outlined might have a considerable impact on national security problems.

#### ACKNOWLEDGMENTS

In addition to various Jason members we would like to thank the following for contributing their advice to us: R. Askey, H. Cohen, R. Fateman, H. Grad, A. Hearn, C. W. Hirt, M. Kac, T. Kailath, M. Kruskal, P. Lax, C. C. Lin, R. May, J. Moses, L. Neuwirth, S. Orszag, J. B. Rosser, S. Rubinow, D. Slepian, F. Tappert, J. Watson, N. J. Zabusky.

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