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SOME PHILOSOPHICAL VIEWS ON ALGORITHMS
AND COMPUTING METHODS IN APPLIED MATHEMATICS

by

A. MIELE

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Some Philosophical Views on Algorithms
and Computing Methods in Applied Mathematics^{1,2}

by

Angelo Miele³

Abstract. This paper summarizes some of the work done by the Aero-Astronautics Group of Rice University in the area of numerical methods and computing methods. It describes some of the philosophical thoughts that have guided this work throughout the years. Recommendations are offered concerning allocation of funds and distribution of funds. Additional recommendations are offered in order to bridge the gap between the top management of government agencies and the academic community.

Key Words. Aerospace engineering, applied mathematics, numerical methods, algorithm research, algorithm development.

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I. Introduction

Colonel Geesey, General Welch, Professor Thrall, Dr. Andrews, Ladies and Gentlemen:

Over the past several days, I have heard the terms C^2 and C^3 mentioned so many times by so many speakers that I cannot avoid the feeling of being an expert on the subject. Yet, was I an expert on the subject barely three days ago? Certainly not.

When I first glanced at the program and saw the symbols C^2 and C^3 repeated over and over, I thought: C^2 must denote some function which is continuous together with its first and second derivatives; analogously, C^3 must denote some function which is continuous together with its first, second, and third derivatives. Then, I thought: What are the military up to? Why are they interested in functions which might exhibit a discontinuity in the fourth derivative?

After listening to the opening speech of General Welch, it became apparent to me that the terms C^2 and C^3 were being employed in a context quite different from that familiar to pure and applied mathematicians. After glancing again at the program, I saw that one of the papers being presented had the title: C^3 at Sea: A Commander's View. Then, I said to myself: perhaps C^3 denotes some new kind of periscope, or atomic submarine, or periscope mounted on some atomic submarine.

Obviously, not even my second interpretation was correct, and it took me a full day to establish a minimum of familiarity with the jargon of generals and admirals. Slowly, but surely, by the end of the first day, I finally understood

that C² means command and control and that C³ means command, control, and communication.

2. Existence of a Gap

Allow me to look backward in time and glance panoramically at the content of this three-day meeting. After listening at the succession of presentations given by generals, admirals, professors, senior scientists, and various technical personnel, what is my overall feeling? Where do we stand now?

Somehow, I have the impression that there are two main possibilities:

(a) perhaps, the wrong people were invited at this meeting; in that case, I am definitely one of the wrong people; or (b) perhaps, the right people were invited at this meeting; in that case, it seems apparent that a considerable gap exists between the top management of the military on one side and the academic people on the other side.

Under the assumption that interpretation (b) is correct, then the best thing which can be done at this moment is to make an effort in order to bridge the gap. With this in mind, I shall devote the rest of my lecture to presenting my philosophical views on research on algorithms and computing methods in applied mathematics. In particular, I shall focus my attention on the work of the Aero-Astronautics Group of Rice University. After this presentation is completed, then each of you can make up his mind as to whether (and to what degree) my work fits within the frame of a C^2 -situation or a C^3 -situation. While I am not so sure that my work is relevant to the C^2 -point of view and/or the C^3 -point of view, nevertheless, there is little doubt in my mind that my work is relevant to AE(meaning aerospace engineering), ND(meaning national defense), and USA(meaning the United States of America).

3. Engineering Design

First, let me give you my background. I graduated in Aerospace Engineering from the University of Rome in 1946, and my initial intention was that of becoming a designer of all sorts of aerospace vehicles. Since the Italian industry was in total shambles in 1946 (we had lost the war), I accepted a job at the Military Aircraft Factory of Cordoba, Argentina. The director of the factory was Brigadier General I. San Martin, who had studied aerospace engineering in Italy at the Polytechnic of Turin, and who had a very soft spot in his heart for Italian technical personnel. He was affectionately called "El Petiso", meaning the short one, for obvious reasons.

As per my wishes, I was given several design assignments: first, the aerodynamic design of a propeller; next, the computation of the flight performance of a training plane; then, the complete preliminary design of a training plane. As my work in design progressed, I started to become aware of the tremendous limitations facing an aircraft designer: there were deadlines to be met and designs to be completed while employing very incomplete information. Early in the game (I was then only 25 years old), I decided that I did not like this state of affairs. I decided that incomplete information was unappealing to me and that, by temperament, I required more complete information. I felt that, in order to become a really good aircraft designer, a knowledge of advanced research techniques was necessary. With this in mind, I undertook a fateful (only for Miele, not for the rest of the world) and irreversible step toward basic research in aerospace engineering.

4. Engineering Research

My first love was flight mechanics. In 1947-48, the problems of the mechanics of flight of turbojet-powered vehicles were all important, and I devoted considerable time to the study in depth of flight trajectories. Within the class of feasible flight trajectories, the study of the optimum flight trajectories is of particular importance, and this in turn leads to mathematical problems which belong to the realm of the calculus of variations and/or optimal control.

In 1952, the great and late Antonio Ferri offered me to join his research group at the Polytechnic Institute of Brooklyn. I accepted with enthusiasm and I worked with him for three years at the aerodynamic design of some of the hypersonic wind tunnels of the Polytechnic Institute of Brooklyn.

In 1955, I joined the staff of Purdue University, and returned to my first love (mechanics of flight), with a new twist: the study of the optimum trajectories of rocket-powered vehicles. I was fortunate enough to convince both AFOSR and NASA to back my work on optimum flight trajectories. In particular, I started a long and rewarding association with AFOSR.

In 1959, I joined the staff of the Boeing Scientific Research Laboratories and continued my work on optimum flight trajectories. This work culminated in the book Flight Mechanics (Addison-Wesley, 1962). In addition, I became interested in the theory of optimum aerodynamic shapes. Together with my research group at BSRL, I tackled a wide variety of optimization problems occurring in the aerodynamics of supersonic, hypersonic, and free-molecular flows. Mathematically

speaking, these problems also belong to the realm of the calculus of variations and/or optimal control.

In 1964, I decided that it was time to return to university teaching and research. The combination of Houston with Rice University proved to be particularly attractive, also in view of the fact that the NASA-Johnson Space Center is located in Houston. At Rice University, I continued aggressively my work on optimum aerodynamic shapes under the sponsorship of AFOSR and NASA-Langley Research Center. This work culminated in the edited book Theory of Optimum Aerodynamic Shapes (Academic Press, 1965).

5. Applied Mathematics Research

About 10 years ago, while working on the problem of optimizing wings, fuselages, and wing-fuselage combinations under different flow regimes, our research program reached a point where advances became increasingly difficult. My feeling was that we were in the presence of a rather steep wall and that a tremendous amount of analytical ingenuity was required in order to make relatively small advances. Perhaps, the best way to portray the situation is this: I felt like a man who is trying to walk on broken glasses without shoes.

The reason for the changed situation was the following. We had almost exhausted our bag of analytical tricks, and we were facing numerical difficulties of ever increasing complexity. When I sensed that this was the case, I said to myself: Angelo, you can no longer be a first-rate engineer without a full and total knowledge of numerical methods. With this in mind, I undertook another fateful (again, only for Miele, not for the rest of the world) and perhaps irreversible step toward basic research in numerical methods and computing techniques.

Since I felt that to work simultaneously on engineering research and mathematical research was rather inefficient (any good general would understand that), I dumped overnight all of my engineering problems and started to work at full blast on mathematical problems only. My intention was to gain rapid knowledge of numerical techniques, in order to be able to return to engineering at a later time and then tackle more complex problems.

In the meanwhile, Mr. R.L. Pritchard, formerly of the Boeing Company, had joined my group at Rice University. Together with him, I undertook a review

of previous work in the area of algorithms for optimal control theory. As soon as we started this review, we realized that we faced two severe limitations. First, the work of some of the leading authorities on the subject was not exactly a masterpiece of clarity. Second, there were some huge holes in the existing body of knowledge; and there were unanswered questions all over the place.

In the light of this situation, we decided early in the game that we should develop our own independent research program, leading to new algorithms for solving all sorts of problems of applied mathematics on a digital computer. Since I could bank on my previous reputation in engineering research, it was not difficult for me to persuade AFOSR, NASA, and later on NSF to back the work of the *Aero-Astronautics Group of Rice University* in the area of numerical methods.

As soon as our program on algorithms for optimal control theory had started, I realized that the solution of optimal control problems on a digital computer cannot be divorced from the solution of differential equations. That being the case, we started a second program dealing with the numerical solution of two-point and multi-point boundary-value problems on a digital computer.

For several years, the computing center of Rice University had been equipped with a Burroughs 5500 computer; more recently, it has been equipped with an IBM 370/155 computer. In spite of the considerable capabilities of these computers, we found that some optimal control problem could tax the memory of these computers almost to the limit. We also found that some optimal control problem could be quite expensive on a digital computer. Since our funds were relatively limited, it seemed difficult for us to be able to develop in this way the

type of systematic information which is needed in order to give algorithms for optimal control theory their most useful structure.

To offset the above difficulty, I reasoned as follows. For every problem of optimal control, there must be a counterpart in mathematical programming which is easier to solve: it does not test the memory capabilities of a given computer, and it does not require as much computer time. In this spirit, we started a third program leading to the development of computer algorithms for mathematical programming problems.

Just as the solution of optimal control problems is related to the solution of differential equations, the solution of mathematical programming problems is related to the solution of nondifferential equations (namely, algebraic and transcendental equations). As a consequence, it was natural for us to start a fourth program dealing with the numerical solution of nondifferential equations.

In summary, after a few short years, our research program on computing methods had grown to a considerable size. Indeed, it included calculus of variations, optimal control, differential equations, two-point and multi-point boundary-value problems, mathematical programming, and solution of nondifferential equations. Several people have contributed in a substantial way to this program. While the list is a long one, I would like to mention Professor H.Y. Huang (presently with EPRA, Exxon Production Research Company, Houston, Texas) and Dr. A.V. Levy (presently with CIMAS, Computing Center of the University of Mexico, Mexico City, Mexico).

6. Thoughts on Algorithm Development and Usage

In Section 5, I discussed the natural development of our research program on computing methods. In this section, I shall describe some of the philosophical thoughts that helped shaping our program.

(a) First of all, the nature of a digital computer is such that the use of vectors and matrices is beneficial to problem formulation. On a digital computer, a problem of flight mechanics is no different from a problem of chemical engineering, as long as both problems are described by the same kind of vector differential equation and vector boundary conditions, for example,

$$\dot{\mathbf{x}} - \mathbf{f}(\mathbf{x}, t) = 0, \quad 0 \leq t \leq 1, \quad (1)$$

$$\mathbf{g}(\mathbf{x}, t) = 0 \quad \text{at } t = 0, \quad (2)$$

$$\mathbf{h}(\mathbf{x}, t) = 0 \quad \text{at } t = 1, \quad (3)$$

where $\mathbf{f}, \mathbf{g}, \mathbf{h}$ are vector functions of appropriate dimensions.

This concept helps grouping some apparently different problems into broad classes of problems, such that a common algorithm can be developed for solving every problem belonging to the same class. The fact that a particular problem deals with flight mechanics and another particular problem deals with chemical engineering comes into play only a posteriori, after an algorithm has been developed, through different specialization of the functions $\mathbf{f}, \mathbf{g}, \mathbf{h}$ and their derivatives.

If the above point of view is taken, then one can develop algorithms useful for a wide variety of problems of the real world, even though the primary interest of a particular scientist might be just flight mechanics. Indeed, by giving generality

to problem formulation, one can succeed in developing algorithms not only useful in aerospace engineering, but also simultaneously useful in other areas of engineering, science, and economics.

(b) In line with (a), it is simply uneconomical and inefficient to try to develop a new mathematical algorithm every time one faces a new technical problem. Usually, it is more economical and efficient to try to employ transformation techniques such that a problem A belonging to a given class, for which algorithms are not available, is converted into a problem B belonging to another class, for which algorithms are already available.

Of course, transformation techniques have their own limitations. They can be used providing the structure of problem B is not too different from the structure of problem A. With these limitations in mind, judicious use of transformation techniques can be helpful in limiting to a considerable degree the proliferation of algorithms solving similar and/or related problems.

(c) The successful development of algorithms for digital computer usage is partly a science and partly an art. Computer experimentation plays a fundamental role, since this is the only way to uncover possible weaknesses and correct these weaknesses if they exist. Only in this way can one hope to develop algorithms that are robust, that is, capable of leading to the solution of many different problems under a wide variety of operating conditions.

(d) As a corollary to the above thought, it is rather unlikely that a successful algorithm might be developed by some mad scientist, who gets up on a given

morning on the right side of the bed. It is nice to dream of some genius who had a wild idea that just happened to work without a hitch on a digital computer. But frankly, this is not the standard situation; and this is because a digital computer has its own rules of operations and its own internal logic; and while this logic is limited, it is usually more consistent than our own human logic.

(e) A scientist has the duty to test his theories experimentally, in order to understand its limitations and try to offset these limitations, if at all possible. If this is not possible, then a scientist has the duty to state these limitations clearly, so that they can be corrected through subsequent work of the same group of people or some other group of people.

(f) Frank exchange of information, collaboration, and interaction between scientists belonging to the same group is fundamental in order to succeed in algorithm research and development. It is most important that both information relative to failures and information relative to successes be conveyed promptly to every element in a group. Indeed, negative information can be as important as positive information in shaping the long-range goals of a group and in determining the most efficient distribution of tasks within a given group.

(g) When applying optimization algorithms to the solution of engineering problems, it must be remembered that nothing catastrophic usually happens, from the point of view of the performance index, if an exact optimum is replaced by an approximate optimum. This is the same as saying that, in the real world, satisfaction of the feasibility equations is usually more important than satisfaction of the optimality conditions.

Paradoxically, it might seem that I am stating that optimization is less important than we like to think, and this thought might shock some professional optimizers. But, if one recovers from the shock, then one can take advantage of this thought when selecting the algorithm to be employed in the solution of a given engineering problem and when choosing the so-called stopping conditions for the feasibility equations and the optimality conditions.

By adhering to the point of view expressed under points (a) through (g), the Aero-Astronautics Group of Rice University has succeeded in developing a wide variety of robust algorithms useful for all sorts of problems of applied mathematics. Perhaps, the best measure of this success is the fact that our group has received over the years some 5000 requests for the various reports and papers that summarize the results of our research program. Does this mean that the proof of the pudding lies in the eating?

7. A View of the Future. Allocation of Funds

Even though the digital computer is barely 20-25 years old, considerable advances have been achieved thus far in the science and art of computing. It appears probable that these advances will continue without any substantial slackening over the next 25-30 years. It also appears probable that, by the beginning of the next century, a relatively steep wall might be reached. Beyond that time, progresses might become slower and more painful to achieve.

Since the next 25-30 years appear to be crucial to the development of new and more sophisticated computing techniques, the following questions arise: (i) Should allocations for research in computing methods be increased? (ii) What is the proper level of support?

In approaching the above questions, the proper reasoning is as follows. Basic research in numerical methods and computing techniques transcends in importance basic research in every other area of engineering, science, and economics, for a simple reason. Real-world problems of aerospace engineering, electrical engineering, mechanical engineering, and so on, ultimately require that some set of equations and/or inequalities be solved on a digital computer. Therefore, to acquire a capability in numerical methods and computing techniques implies to acquire an enhanced capability in engineering, science, and economics.

In the light of the above reasoning, it appears that the answer to question (i) is decisively affirmative. Concerning question (ii), one must remember that research in computing methods is relatively inexpensive and occupies at this

moment only a tiny fraction of the overall budget of major government agencies, such as AFOSR, ARO, ONR, and NSF. Thus, it is entirely appropriate for the top management of the military as well as for the top management of NSF to consider doubling or tripling the funds presently allocated for computing research.

8. Distribution of Funds

In this section, I shall touch a very sensitive topic, that of the distribution of research funds within the nation. The standard operating procedure of government agencies seems to be more or less as follows. Suppose that government agency XYZ has a yearly budget of 2000K. Furthermore, suppose that we divide this budget into 100 equal packages, each worth 20K. Then, it follows that government agency XYZ can support 100 principal investigators, spread more or less with uniform density all over the country.⁴

In proceeding along the above lines, government agency XYZ avoids political criticism. At the same time, it buys technical insurance: if agency XYZ supports 100 efforts, even admitting that only 20% of these efforts turn out to be good, then agency XYZ is actually supporting 20 good research programs.

The drawback of the above operating procedure is that it is in direct conflict with two basic concepts of the real world: (i) even though the USA is the richest nation in the world, its financial means are limited; and (ii) in any given area of research, the supply of true talent is also quite limited.

Because of the realities of (i) and (ii), one must wonder whether a more efficient use of national resources can be achieved through concentration of effort, that is, through the allocation of larger grants into more capable hands. In suggesting this course of action, I feel that some immediate clarification is necessary. I am

⁴In the C^1 -language (C^1 meaning contracts), it is known that 1K = \$1000.

not really thinking about semipolitical handouts of the Themis type. I am rather thinking about allocation of larger chunks of research funds into the hands of the best available technical talent.

Now, I am fully aware of the fact that allocation of research funds solely on the basis of technical considerations might cause a national outcry: I can envision scores of senators and congressmen firing telegrams all over the place, because their pet university did not get the proper share of the research pie.

In the light of the above possibility, it might be that the best course of action is a compromise between national needs and local needs. As the Romans used to say, in medio stat virtus. For example, consider once more the budget of government agency XYZ (2000K), and suppose that this budget is split into two equal parts: 1000K devoted to the support of small efforts (each worth 20K) and 1000K devoted to the support of large efforts (each worth 100K). If this course of action is taken, then government agency XYZ might be supporting 50 small research efforts plus 10 large research efforts. In this way, political criticism might still be avoided; technical insurance might still be bought. Yet, the realities of the financial situation of the country and of the nature of basic research might be more adequately considered than ever before.

9. Lagrange and Napoleon

During the first day of this meeting, General Welch mentioned a particular technical problem in which Lagrange multipliers played a role. I would like to inform General Welch that I have been dealing with Lagrange multipliers for the past 30 years. At Rice University, we have developed all sorts of algorithms, even algorithms capable of optimizing the distribution of Lagrange multipliers within a system governed by equations and/or inequalities. Also, we have optimized the distribution of Lagrange multipliers for systems described by differential equations and appropriate boundary conditions.

The name Lagrange is of such a common usage in today's technical world that a comment is in order. Contrary to superficial appearance, Lagrange was neither a French nor a Louisiana Cajun. He was an Italian, born in Turin in the year 1736. Indeed, his certificate of birth bears the name of Giuseppe Lodovico Lagrangia. He taught ballistics at the Military Academy of Artillery in Turin and founded the Academy of Sciences of Turin, a dusty academy that still exists today. By the way, I was just elected a Corresponding Member of that venerable academy.

At the age of 30, Lagrangia (already famous for his work on Analysis, Algebra, Calculus of Variations, and Analytical Mechanics) left Turin, first for Germany (where he spent 21 years) and then for France (where he spent the last 26 years of his life). In France, he changed the spelling of his name to Lagrange, and this explains why we talk today about Lagrange multipliers rather than Lagrangia multipliers.

But Lagrange was not the only Italian who became prominent in the French-speaking world. Since this is a meeting of generals, I would like to mention the name of the greatest general of all times, Napoleone Bonaparte, who was born in Aiaccio, Corsica, from an Italian family.

It is not ironic that the two greatest Frenchmen that ever lived, Lagrange and Napoleon, both happened to be Italians ?

10. Concluding Remarks

I have done my best to give everybody in this audience an idea about the nature of the work of the Aero-Astronautics Group of Rice University in engineering and applied mathematics. I have described some of the philosophical thoughts guiding our work in numerical analysis and computing methods.

I realize that our academic work has become so technical that, quite often, people in government agencies are at a loss in understanding the implications of this work. This leads to the gap that I have described at the beginning of my talk.

To bridge this gap, it would be a good idea that government personnel (in particular, AF personnel) be sent to work with senior scientific personnel, working in the universities, for a period of one-two years. In this sense, I support the recommendations provided by Professor R. E. Kalaba in the opening lecture of this session.

Concerning the relation of my work with C^2 -situations and C^3 -situations, I cannot tell for sure whether a connection exists. But I can think of two problems to which my work applies and which should be important to the top management of the military: weapons allocation problems and interception problems. At any rate, while I am not sure of the relevance of my work to C^2 and C^3 , I am quite sure of its relevance to AE, ND, and USA.

Now, suppose that there is somebody in this audience who is still not satisfied with my statements and says: "Dr. Miele, I like your speech, but I still do not understand the relation of your work with C^2 and C^3 ". Then, my answer would

be more or less as follows: "Mr. so and so, I believe that your question is very perceptive and stimulating. But I am unable to supply a clear-cut answer, for reasons of national security. Specifically, in order to expound about the relations of my work with C² and C³, I would have first to expound about the relations of my work with C⁴ and C⁵. And this I cannot do, because it is militarily classified information".

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