Optimal Control and Differential Equations

Edited by

A. B. SCHWARZKOPF
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IN MEMORIAM

It has been the writer's good fortune to know W. T. Reid since 1936, when he was one of the bright young members of the Department of Mathematics at the University of Chicago. We were colleagues in the System Evaluation Department of the Sandia Corporation during portions of 1952 and 1953 and in the Department of Mathematics at The University of Oklahoma from 1964 until 1976.

His enthusiasm and meticulous grasp both of theories and of essential details were shown in the classroom, but especially in seminars and conferences on differential equations and variational theory. He believed in the highest standards for mathematical education and yet was patient with those of limited ability who were struggling to follow. He shared generously the breadth and depth of his knowledge with anyone who sought his counsel. W. T. and Idalia have been the best of companions on social occasions.

The death of this good man, this cherished friend, on October 14, 1977, represents a profound personal loss to his many former students and associates and is a material loss to the mathematical community.

George M. Ewing

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Preface

The articles in this volume were all presented at the Conference on Optimal Control and Differential Equations held at The University of Oklahoma in Norman, March 24–27, 1977. The occasion for this conference was the retirement of Professors W. T. Reid and George M. Ewing from the faculty of The University of Oklahoma. Since their retirement also signaled the passing of a generation of mathematicians who made fundamental advances in the calculus of variations and related problems in differential equations, it seemed appropriate to mark this occasion with a conference that would attempt to assess the present state of mathematical knowledge in these areas and suggest directions for new research efforts.

We invited the authors appearing in this volume to present talks on their own fields of expertise describing the field, rather than presenting new research. The conference itself attracted nearly one hundred participants, and was made a success through the efforts of many people. We would like to acknowledge especially the financial support of the Army Research Office, Durham, Grant No. DAAG29-77-M-0059. We also appreciate the encouragement and financial support of Dean Paige Mulhollan of the College of Arts and Sciences; Dean Gordon Atkinson of the Graduate College; and Dr. Gene Levy, Chairman of the Department of Mathematics, all at The University of Oklahoma.

We finally acknowledge the special talents of Ms. Trish Abolins for her expert work as a copy editor and technical typist for the preparation of this volume.

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Optimal Control

THE CALCULUS OF VARIATIONS FROM THE BEGINNING THROUGH OPTIMAL CONTROL THEORY

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Before I begin this talk, I would like to sketch briefly what I plan to do. I hope to speak of some of the important stages of the development of the calculus of variations, with a disproportionately large part of the hour allotted to recent developments. But I have no intention of listing important discoveries with their dates. Rather, I shall try to say something of the underlying patterns of thought at each stage, and to comment on the change in that pattern produced by each of the new ideas. It may seem that I am deriding our predecessors for not having seen at once all that we have learned. I have no such intention. We must all do our thinking on the foundation of what we already know. It is hard to assimilate a genuinely new idea, and even harder to realize that ideas we have earlier acquired have become obsolete.

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Preparing this talk has forced me to formulate with at least some pretension to clarity what is meant by the calculus of variations. There is no universal agreement on the definition of the subject, and I have gradually come to the conclusion that part of the reason is that there are at least two related but different sets of ideas that are often brought together under the same name. The first set might be called the theory of extrema. A functional is defined on some class of functions; the problem is to find a function in the given class that minimizes or maximizes the functional on that class. If this theory of extrema is included in the calculus of variations, Caratheodory may be justified in asserting that the first problem in the calculus of variations was that of finding a curve of given length that joins the ends of a line segment, and together with that segment encloses the greatest possible area. This was solved, according to Caratheodory, by Pappus, in about 290 A.D.

The second set of ideas is concerned with functionals on linear topological spaces, ususally function spaces, and constitutes a part of a differential calculus on such spaces. The central problem in this part of the theory is that of finding stationary points of functionals; that is, points at which the directional derivatives in all directions exist and are all 0 . Since such points are characterized by means of investigating the effect on the functional produced by small variations of the function which is the independent variable, this study of stationary points can reasonably be called the calculus of variations.

The two sets of ideas both have important applications, but to different problems. At one extreme we have those problems such as the isoperimetric problem of Pappus just mentioned, and more recently problems in which a function is to be found that produces a best possible result in some sense, such as propelling an airplane between given points with least expenditure of fuel. At the other extreme we have situations in which the presence or absence of a maximum or minimum is irrelevant; only the consequences of stationarity matter. These consequences often include the satisfaction of a set of differential equations. According to what is misnamed "the principle of least action," the motion of a set of particles follows a time-development for which a certain integral, called the "action," is stationary. The function for which the action is stationary is the one for which the classical equations of motion are satisfied, and the satisfaction of those equations is all that we want.

In between these two extremes we have the problems of relative extrema. Let us say that a function y is in the weak ε-neighborhood of another function y_0 if there is a homeomorphism between their graphs such that at corresponding points, the values of y and y_0 differ by less than ε, and so do the values of their derivatives. The function y is in the strong ε-neighborhood of y_0 if this holds with the reference to the derivatives deleted. A functional has a weak (strong) relative minimum at y_0 if for some positive ε, the functional has at y_0 its least value on the set of all those y in the domain of the functional that are in the weak (strong) ε-neighborhood of y_0. These concepts have some applications, related to stable and unstable equilibrium; but I have a strong suspicion that relative maxima and minima were usually studied, not because they were really wanted, but because available theory did not permit the study of absolute maxima and minima.

For lack of time I shall say little about the second set of ideas, based on stationarity. This means that I shall disregard some important pure mathematics and some important applications. I have mentioned that the principle of least action is of this type. So too is Hamilton's study of optics and its extension into calculus of variations by Jacobi. So is all the mathematics of quantum theory that is based on a Hamiltonian. So, too, is Marston Morse's theory of the calculus of variations in the large. I shall choose for my principal subject the development of the first set of ideas, that I have called the theory of extrema.

In the eighteenth century the distinction between the two sets of ideas was hardly noticed. If it could be shown that any curve that minimized some functional had to satisfy a certain condition, and a curve could be found that did satisfy that condition, it was accepted without comment that that curve did furnish the minimum. Nor has such a feeling quite disappeared. On page 16 of the book by Gelfand and Fomin (English translation) we read: "In fact, the existence of an extremum is often clear from the physical or geometric meaning of the problem, e.g., in the brachistochrone problem, the problem concerning the shortest distance between two points, etc. If in such a case there exists only one extremal satisfying the boundary conditions of the problem, this extremal must perforce be the curve for which the extremum is achieved." I disagree with this on three counts. First, if the calculus of variations is mathematics, our conclusions must be deducible logically from the hypotheses, with no use of anything that is "clear from the physical meaning" — even if anything is ever that clear in physics. Second, if the mathematical expression is meant to be a model of a physical situation, we are not entitled to unshakeable confidence that the model we have chosen is perfect in all details; rather, we should keep in mind that a mathematical model of a physical system is necessarily a simplification and idealization. Third, the principle as stated is untrustworthy. For example, if A and B are two points in the upper half-plane there always exists a curve joining them such that the surface of revolution obtained by rotating it about the x-axis has least area. If A and B are properly located, there is just one extremal that joins them, and it does not furnish the least area. (See G. A. Bliss, "Calculus of Variations," p. 116.)

In the early eighteenth century the necessary conditions for a minimum in various specific problems were found by ingenious devices, usually involving replacing a short arc of the curve by another short arc with the same ends. In 1760, Lagrange unified these special solutions by means of the idea of a variation. Suppose that a function $x \to y(x)$ $(x_0 \leqq x \leqq x_1)$ minimizes a functional $J(x(\cdot))$ in a certain class K of functions. Suppose further that we can find a family of functions y_α $(-b < \alpha < b)$ such that for each α in $(-b,b)$ the function $x \to y_\alpha(x)$ $(x_{0,\alpha} < x < x_{1,\alpha})$ is in the given class K. Then the derivative at $\alpha = 0$ of the function $J(y_\alpha(\cdot))$, if it exists, must be 0. The function

$$x \to \eta(x) = \partial y_\alpha(x)/\partial\alpha \qquad (\alpha = 0)$$

is often called a variation of y; Lagrange used the term "variation" and the symbol δy for the product of this by $d\alpha$ The variation of the functional, which is the derivative of $J(y_\alpha(\cdot))$ at $\alpha = 0$, is the directional derivative of J in the direction η. In many interesting cases its vanishing is equivalent to the satisfaction of a certain differential equation; this is the Euler-Lagrange equation.

For the purposes of mechanics the goal had now been reached. The Euler-Lagrange equation permitted the introduction of general coordinate systems, and the concept of stationary curve unified the whole theory of classical mechanics, as Lagrange showed in his masterful work. But it was a mental confusion, consistent with the somewhat uncritical ideas of the period, to think that any stationary curve would certainly furnish a maximum or a minimum, as wished. In his "Principia," Isaac Newton had discussed the problem of finding a surface of revolution with assigned base and altitude that minimized a functional that Newton thought represented the drag when the body is moved through a fluid. Legendre published his necessary condition for a minimum in 1786, a century later; but in 1788, he published another paper, entitled "Mémoire sur la manière de distinguer les maxima des minima dans le calcul de variations," in which he pointed out that a curve could satisfy the Euler-Lagrange equation for the integral expressing the Newtonian resistance and still not give the surface of least resistance. The most interesting feature of his proof is that he showed that the Weierstrass condition for a minimum was not satisfied — and Weierstrass was born until twenty-seven years later. This work must not have had the immediate effect that it deserved. Mathematicians continued to act as though the only feature of importance was the satisfaction of the condition for stationarity. More than two decades later Robert Woodhouse, F. R. S., a Fellow of Caius College, Cambridge, published a book entitled "Treatise on Isoperimetrical Problems and the Calculus of Variations," (1810), in which Legendre is not mentioned. In this book, Woodhouse poses the problem of maximizing the integral

$$\int [d^2y/dx^2]^2 dx ,$$

the class of curves not being clearly specified. By use of variations he came to the conclusion that the maximum is provided by the line segment joining the end-points. Had he used Legendre's results he would have recognized the falsity of his conclusion. But even without having read Legendre, he should have noticed that unless the end-points coincide, no maximum can exist, and the line segment gives to the integral the value 0 , an obvious minimum.

The guiding principle during the eighteenth century and more than half the nineteenth seemed to be that if a minimizing function is sought for some functional, then by inventing more and more necessary conditions for a minimum we can feel steadily more confident that a function that passes all the tests is in fact the minimizing function sought. The first necessary condition was stationarity, established when the curve being tested can be varied in arbitrary directions. The next in order of time was the Legendre condition, still in the domain of Lagrange-type variations, and in fact needing only variations that leave the function unchanged outside a small interval. Next came the condition of Jacobi. Like that of Legendre, it expressed the fact that for a minimum all directional second derivatives (second variations) must be *nonnegative;* but unlike Legendre's it required the variation of the function along long intervals. Next came the necessary condition of Weierstrass. Unlike the others, it cannot be established by means of Lagrange-type variations or directional derivatives. The function being tested is compared with other functions near it in position but widely different in derivative. That is to say, the Weierstrass condition is necessary for a strong relative minimum, not for a weak one.

But Weierstrass made a more significant contribution than the discovery of a new necessary condition. For unconditioned problems, in which the minimum of an integral

$$\int_{x_0}^{x_1} f(x,y(x),y'(x))dx$$

is sought in the class of all sufficiently well-behaved functions with assigned end-values, he was able to prove that when a function $y(\cdot)$ satisfies conditions that are slight strengthenings of the four known necessary conditions, it will provide a