# THE MATHEMATICAL SCIENCES AND WORLD WAR II

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I shall present an account of some of the activities in mathematics that were carried on during World War II and comment on their impact on the development of the mathematical sciences in the United States after the war. Most of this memoir will be concerned with aspects of mathematical activity with which I had personal contact because of my role as executive assistant to Warren Weaver, who was Chief of the Applied Mathematics Panel of the Office of Scientific Research and Development during the war, and with war-related developments that came within my purview because of my responsibilities as head of the mathematical research program of the newly established Office of Naval Research (ONR) after the war.[1]

## The Mathematical Environment in the United States Before World War II

I want first to try to set the wartime work in context by speaking briefly about the mathematical environment in the United States in the 1930's and early forties. Applied Mathematics was not strongly represented at American universities, although Richard Courant, who had come to this country in 1934, had drawn together an able group at New York University, and William Prager, with effective support from R. G. D. Richardson, then Dean of the Graduate School at Brown, in 1941 established at Brown a Program of Advanced Instruction and Research in Applied Mechanics.[2] As Professor Prager said in 1972:

> In the early thirties, American applied mathematics could, without much exaggeration, be described as that part of mathematics whose active development was in the hands of physicists and engineers rather than professional mathematicians. This is not to imply that there were no professional mathematicians genuinely interested in the applications, but that their number was extremely small. Moreover, with a few notable exceptions, they were not held in high professional esteem by their colleagues in pure mathematics, because there was a widespread belief that you turned to applied mathematics if you found the going too hard in pure mathematics. As a distinguished evaluation committee...put it [in 1941]: "In our enthusiasm for pure mathematics, we have foolishly assumed that applied mathematics is something less attractive and less worthy."[3]

The situation in mathematical statistics was somewhat similar. By 1940 only a handful of universities in the United States were offering serious work in this field. Harold Hotelling was at Columbia and Jerzy Neyman was at Berkeley. S. S. Wilks, who had earned his Ph.D. at Iowa under H. L. Rietz, had been appointed at Princeton in 1933 to develop work in mathematical statistics. However, he "did not give a formal course in statistics at Princeton until 1936, owing to a prior commitment that the university had made with an instructor in the department of

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In 1962, Mina Rees received the first of the MAA's Awards for Distinguished Service to Mathematics; a summary of her career and her many honors up to that date appears on pages 185–187 of volume 69 of this MONTHLY. At that time she had recently become Dean of Graduate Studies at the City University of New York; she retired in 1972 as President Emeritus of the Graduate School and University Center. We welcome this opportunity to publish her reminiscences of the war years.—*Editors* economics and social institutions who had been sent off at university expense to develop a course on 'modern statistical theory' two years before; and owing to the need for resolution by the university's administration of an equitable division of responsibility for the teaching of statistics between that department (which . . . had been solely responsible for all teaching of statistics) and the department of mathematics. Wilks . . . in the spring of 1937 . . . gave an undergraduate course [in statistics], quite possibly the first carefully formulated college undergraduate course in mathematical statistics based on one term of calculus."[4]

On the other hand, U.S. research in what we now call "core mathematics" had been assuming increased importance on the international scene in the twenties and thirties. Moreover, it had a substantial flowering just before America felt herself inevitably drifting toward active participation in the war. For, with the coming of Hitler in 1933, many of the world's leading mathematicians had sought asylum in the United States and had greatly enriched the quality and quantity of mathematical activity in this country. In 1940, *Mathematical Reviews* was established by the American Mathematical Society, with two of the notable refugees, Otto Neugebauer and William Feller, assuming editorial responsibility, a step that fundamentally changed the reliance of American (and world) mathematicians on *Zentralblatt für Mathematik*, which had been for a decade the world's reviewing journal for mathematics.

With the passage of time, it became increasingly clear that war was inevitable. In the developing mobilization of mathematicians in support of the war effort, some enlisted or were drafted, some remained at their colleges or universities and participated in the training programs in mathematics that the armed services were setting up, and some left their universities to assume specific war-related activities.

Where did the mathematicians go who left their universities to assume noncombat war-related tasks, and what was the nature of their work?

There were many working for the armed services, some in uniform, like Herman Goldstine at Aberdeen and J. H. Curtiss in the Navy's Bureau of Ships, and others as civilians, like E. J. McShane at Aberdeen and F. J. Weyl in the Navy's Bureau of Ordnance. A number of mathematicians were attached to various Air Commands as members of Operations Research teams, like G. Baley Price in the Eighth Air Force; and others were associated with British and Canadian research efforts. There was the Navy's Operations Research Group, directed by the MIT physicist Philip M. Morse. Another group of mathematicians was working on war tasks in industry. For mathematicians, Bell Telephone Laboratories was, perhaps, the most familiar of the industrial laboratories, but a number of industrial groups (e.g., RCA, Westinghouse, Bell Aircraft) with war contracts employed mathematicians professionally. There was a group of mathematicians in cryptanalysis and another group in the Manhattan Project, which had been set up to develop the atomic bomb.

In addition, a large number of mathematicians were employed in the various parts of the Office of Scientific Research and Development (OSRD), a civilian establishment in the Executive Office of the President.

The OSRD had several parts: one devoted to medical research; one devoted to fuse research, a project of highest priority and secrecy; and the third and largest, the National Defense Research Committee (NDRC), which comprised groups of scientists and engineers concerned with submarine warfare, radar, electronic countermeasures, explosives, rocketry, etc. One of the divisions was the Radiation Laboratory at MIT. NDRC had been set up in 1940, even before the United States entered the war, to provide scientific assistance to the military forces. There was initially no mathematics division. By 1942 the demands for analytical studies had increased rapidly. As Warren Weaver observed in his autobiography:

> As the war went on, the emphasis [by NDRC] on the design and production of hardware necessarily tapered off somewhat, for the practical reason that by then a brand-new device simply could not be conceived of, designed, built in pilot model, tested, improved, standardized, and put into service in time to affect the conduct of the war.[5]

**The Establishment of the Applied Mathematics Panel**

By the Fall of 1942, Vannevar Bush, who headed OSRD, decided to reorganize NDRC to enable it to perform its remaining tasks more competently and to incorporate into the reorganization a new unit, the Applied Mathematics Panel (AMP).[6] The task assigned to the Panel, as it was called, was to help with the increasingly complex mathematical problems that were assuming importance and with those other problems that were relatively simple mathematically but needed mathematicians to formulate them adequately. Warren Weaver agreed to serve as Chief.

Weaver, who had been Professor and Chairman of the Mathematics Department of the University of Wisconsin, was, in 1940, Director of the Division of Natural Sciences of the Rockefeller Foundation. In the original NDRC, he was head of a Fire Control Section whose most important assignment was to develop an anti-aircraft director that would serve as an essential component in the system that was needed to protect Britain from German bombing; and he was, personally, deeply involved in this development. However, in February 1942, when the AA director developed under his guidance was accepted by the Army (as the M-9 Director),[7] Weaver became available for his new assignment.[8a]

Many of the mathematicians who left their universities to work on war-related problems were employed, during the war, under contract with the new Applied Mathematics Panel. But many others were attached to projects that were being carried forward under other parts of NDRC, such as those I have already mentioned. A. H. Taub, for example, was attached to the explosives division. Much interesting and important applied mathematics was going on there and in many other divisions of NDRC. But AMP was set up to provide additional mathematical assistance, aiding the military services and other divisions of OSRD when they were asked to do so, provided they considered that they had a reasonable chance to do something useful. By the end of the war, AMP had undertaken almost 200 studies, nearly one-half of which represented direct requests from the armed services.

The general policy of the Panel was based on recommendations made by a group of mathematicians known as the Committee Advisory to the Scientific Officer. The Panel consisted of Richard Courant, G. C. Evans, T. C. Fry (Deputy Chief), L. M. Graves, Marston Morse, Oswald Veblen, S. S. Wilks, and, of course, Warren Weaver as Chairman. I was a civil servant and technical aide to the Chief. Among other technical aides were I. S. Sokolnikoff and S. S. Wilks, who were my colleagues on the Board of Editors of the *Summary Technical Report of the Applied Mathematics Panel*. The Panel (with its own office in New York) set up contracts with eleven universities, including Princeton, Columbia, New York University, the University of California (Berkeley), Brown, Harvard, and Northwestern, and had responsibility for the work of the Mathematical Tables Project (established originally as a scientific program by the National Bureau of Standards and administered during its first five years by the Works Project Administration).[8b] Many of the country's ablest mathematicians were employed on these university contracts, and many moved from their homes in order to participate. Two economists, W. Allen Wallis, who was to become Chancellor of the University of Rochester, and Milton Friedman, who was to win a Nobel prize in economics, operated as statisticians. John von Neumann, who had come to Princeton in 1930 and moved to the Institute for Advanced Study in 1933, was also one of those involved with the Panel. But his role, not only during the war but after its conclusion, was unique; for he was a consultant or other participant in so many government or learned activities that his influence was very broadly felt. It was during the war that the seminal book *Theory of Games and Economic Behavior* reached the printer, evolving from von Neumann's early work with some of the basic ideas and from his collaboration, beginning in 1940, with the economist Oskar Morgenstern. Moreover, as a consultant to the Aberdeen Proving Grounds, which sponsored the work at the University of Pennsylvania, where the ENIAC, the first electronic digital computer, was being developed, von Neumann had a profound influence on the design of electronic computers even in their initial stages. And his perceptions of the most urgent directions in computer development were greatly affected by the needs of the Manhattan Project. Until the time of his death in 1957, von Neumann continued to have great influence on the development of computers and of game theory. (Since I had no direct contact during the war either with the Manhattan Project or with cryptanalysis, I shall not discuss mathematical contributions to these fields, although I am sure they are of interest. The work of the Manhattan Project is, perhaps, better known than that of the cryptologists and the cryptanalysts who played a critical role in the Allied victory).

## Wartime Computing and the Post-War Computer Program

Mathematicians had been alerted as early as 1940 to the fact that we were on the threshold of a new computer age when George Stibitz, surely one of the most powerful of the early digital computer designers, demonstrated, at the summer meetings of the mathematical organizations at Dartmouth in 1940, a machine he had designed at Bell Telephone Laboratories. As the *Bulletin of the American Mathematical Society* reported (46 (1940) 841): "The Bell Telephone Laboratories exhibited a machine for computing with complex numbers. The recording instrument at Hanover was connected by telegraph with the computing mechanism in New York. This machine was available to members from 11 A.M. to 2 P.M. each day of the meeting." Dr. Stibitz's paper was entitled "Calculating with Telephone Equipment." In fact, as the pressure for machine computation developed during the war, telephone relays proved to be the most reliable components available in the earliest days of automatically sequenced calculators. The focus at that time was on getting machines into operation that would immediately solve important problems and provide a significant advance over the desk calculators that were being very skillfully used wherever scientific workers were trying to get answers to pressing problems.

Aberdeen was heavily engaged in ballistic computations and, as I mentioned above, was supporting machine development at the University of Pennsylvania. The Navy's Bureau of Ordnance, also in acute need of computation, had its major machine development at Harvard, where (with IBM support) Howard Aiken had a machine in operation before the end of the war. The earliest operating large-scale computers (which had telephone relays as their principal components) did not have the speed of the automatically sequenced electronic computers developed somewhat later, but they made important contributions to the military needs during wartime and to the swelling interest of mathematicians and engineers in the potential of automatically sequenced machines. Before the end of the war, there was an awakening realization among mathematicians that a new focus in numerical analysis would be needed as the machines became more important in scientific work. It would be false to give the impression that there was a widespread concern among the country's leading mathematicians about what would be needed in numerical analysis or, indeed, about what would happen in computer development. But some of the men and women who had had wartime experience did develop an interest in this emerging field. As the speed and capacity of machines increased after the war's end, the scope of mathematical problems that would require attention if the machines were to be properly used expanded significantly and, partly under the stimulation of the Office of Naval Research, these problems aroused the interest of increasing numbers of mathematicians.

Although automatically sequenced electronic computers were not available before the end of the war, the needs of the war played a decisive role in their initial development and the military services continued their interest and provided much of the financing for the post-war developments. In 1946 the ENIAC, the first electronic computer, became operational at the Moore School; in 1947 it was moved to Aberdeen. By that time, the activities leading to the establishment of the National Applied Mathematics Laboratories of the National Bureau of Standards were already under way. These Laboratories were jointly supported by those agencies of the federal government that had a stake in developing or using large-scale automatic computing facilities. ONR was one of the supporting agencies. The Laboratories would, when they were established, include a Computing Laboratory, a Machine Development Laboratory, a Statistical Engineering Laboratory, all in Washington; and, a little later, an Institute for Numerical Analysis, located on the UCLA campus. An Applied Mathematics Executive (later Advisory) Council, consisting of some of the country's most active scientists in the field, as well as representatives from the various government agencies, was formed to serve as a forum before which practically all major undertakings in the computer field were thrashed out with decisive effects on their scope and orientation. It was here that a reasonable national level of research in this new field was set, taking account of the current state of electronics and relevant theories and the scope of required and probable applications. The needs of the Census Bureau were pressing, and military programs in the computer field played a large role. The work of the code-makers and code-breakers was, to a certain extent, incorporated informally, as were developments at Los Alamos. The existence of all these pressures and the support of government agencies, as well as the impressive performance of the National Bureau of Standards, were largely responsible for the establishment of U.S. leadership in computer technology. These developments took place during 1946–1953. At that time, commercial companies began to make major commitments to the production of computers, making them generally available. Many of the people who supported this effort had been trained in the code-making and code-breaking establishment.

## An Overview of the Work of the Applied Mathematics Panel

**Fluid Mechanics, Classical Dynamics, the Mechanics of Deformable Media, and Air Warfare.** Since the Applied Mathematics Panel represented the largest group of mathematicians organized under government auspices to provide mathematical assistance wherever it was needed during the war, it may be of interest to give a brief overview of the nature of the studies carried on by the Panel from its founding in late 1942 until its dissolution at the end of 1945.[9]

Most AMP studies were concerned with the improvement of the theoretical accuracy of equipment by suitable changes in design or by the best use of existing equipment, particularly in such fields as air warfare. It often happened that a considerable development of basic theory was needed. The following illustrations are taken from the work at New York University, Brown, and Columbia.

At New York University, the work in gas dynamics was principally concerned with the theory of explosions in the air and under water and with aspects of jet and rocket theory. New results were obtained in the study of shock fronts associated with violent disturbances of the sort that result from explosions. A request by the Bureau of Aeronautics for assistance in the design of nozzles for jet motors gave rise to an extended study of gas flow in nozzles and supersonic gas jets. In this field, as in every part of the work of the Applied Mathematics Panel, one result of the work was to provide men (alas, there were not many women) who were broadly and deeply informed in a number of important and difficult fields and who were therefore often called upon as consultants. I have a vivid remembrance of a visit in the company of Richard Courant and Kurt Friedrichs to the rocket work going on at the California Institute of Technology. The Caltech people were having trouble with the launching of their rockets, and they were eager for advice. When I talked about that visit fairly recently with Professor Friedrichs, he was characteristically modest; but when we left Pasadena back in 1944, the Caltech people had new experiments planned, at least partially inspired by suggestions they had received. And the outcome, whether or not significantly affected by Friedrichs's suggestions, was successful.

Because so many questions were raised by wartime agencies about the mathematical aspects of the dynamics of compressible fluids, a Shock Wave Manual was prepared at NYU and published in its first version in 1944 by the Applied Mathematics Panel. It was one of the major documents of continuing mathematical interest to grow out of the Panel's work. Its successor, the book *Supersonic Flow and Shock Waves*,[10] was published in 1948. Its preface stated:

> The present book originates from a report issued in 1944 under the auspices of the Office of Scientific Research and Development. Much material has been added and the original text has been almost entirely rewritten. The book treats basic aspects of the dynamics of compressible fluids in mathematical form; it attempts to present a systematic theory of non-linear wave propagation, particularly in relation to gas dynamics. Written in the form of an advanced text book, it accounts for classical as well as some recent developments, and, as the authors hope, it reflects some progress in the scientific penetration of the subject matter. On the other hand, no attempt has been made to cover the whole field of non-linear wave propagation or to provide summaries of results which could be used as recipes for attacking specific engineering problems . . .
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> Dynamics of compressible fluids, like other subjects in which the non-linear character of the basic equations plays a decisive role, is far from the perfection envisaged by Laplace as the goal of a mathematical theory. Classical mechanics and mathematical physics predict phenomena on the basis of general differential equations and specific boundary and initial conditions. In contrast, the subject of this book largely defies such claims. Important branches of gas dynamics still center around special types of problems, and general features of connected theory are not always clearly discernible. Nevertheless, the authors have attempted to develop and to emphasize as much as possible such general viewpoints, and they hope that this effort will stimulate further advances in this direction.

After the war, the NYU group continued its interest in a number of the problems worked on during the war with support from all the military services. J. J. Stoker's studies of water waves, in particular, were continued. And, with the growth of computers, the group greatly expanded its work in fields related to computer applications.

At Brown, the work focused on problems in classical dynamics and the mechanics of deformable media. The mathematical output of the Brown group was substantial; but I think it is worth quoting a paragraph from a letter from William Prager, the head of the Brown group, to Churchill Eisenhart, written in June 1978. He says:

> While the Applied Mathematics Group at Brown University worked on numerous problems suggested by the military services, I believe that its essential service to American Mathematics was to help in making Applied Mathematics respectable . . . The fact that the Program of Advanced Instruction and Research in Applied Mechanics, the forerunner of Brown's Division of Applied Mathematics, relied heavily on the financial support available under a war preparedness program illustrates the influence of the war on the development of the mathematical sciences in the U.S.

It is certainly true of the post-war programs at Brown and at NYU that they drew great strength from the importance of their work to the war effort and from the interest of the military services in their continuing vitality after the war.

At Harvard, the work in underwater ballistics produced a polished account of the water entry problem and, like all the other projects, it provided a group of expert advisers, in this case for the Navy. Moreover, it gave applied mathematics in the United States an important, newly active participant, Garrett Birkhoff.

The three projects I have thus far mentioned were all concerned with what can be described as classical applied mathematics. The largest of the so-called "Applied Mathematics Groups," the one at Columbia, had a different kind of assignment. For several years, its work was devoted primarily to studies in aerial warfare, the most extensive analyses being devoted to air-to-air gunnery. At the time of its establishment in 1943, this group was headed by E. J. Moulton; during its last year, from the beginning of September 1944 to the end of August 1945, Saunders Mac Lane was its "Technical Representative."

The final summary of the work done by the Applied Mathematics Group at Columbia under the AMP contract, as well as related work done elsewhere in the United States and abroad, was reported in the *Summary Technical Report of the Applied Mathematics Panel*[11] under the following headings: (1) Aeroballistics—the motion of a projectile from an airborne gun; (2) Theory of deflection shooting; (3) Pursuit curve theory—important because the standard fighter employed guns so fixed in the aircraft as to fire in the direction of flight, and important also in the study of guided missiles that continually change direction under radio, acoustical, or optical guidance unwillingly supplied by the target; (4) The design and characteristics of own-speed sights—devices designed for use in the special case of pursuit curve attack on a defending bomber; (5) Lead computing sights—which assume that the target's track relative to the gun mount is essentially straight over the time of flight of the bullet; (6) The basic theory of a central fire control system; (7) The analytical aspects of experimental programs for testing airborne fire control equipment; (8) New developments, such as stabilization and radar.

That part of the program of the Applied Mathematics Panel that was concerned with the use of rockets in air warfare was primarily the responsibility of Hassler Whitney, who served as a member of the Applied Mathematics Group at Columbia. He not only integrated the work carried on at Columbia and Northwestern in the general field of fire control for airborne rockets but maintained effective liaison with the work of the Fire Control Division of NDRC in this field and with the activities of many Army and Navy establishments, particularly the Naval Ordnance Test Station at Inyokern, the Dover Army Air Base, the Wright Field Armament Laboratory, the Naval Bureau of Ordnance, and the British Air Commission.

All these studies were concerned with the best use of equipment or with changes in equipment that could be effected in time to be of use in World War II. Two studies in air warfare carried out under AMP auspices came closer to having general tactical scope than did most of the other work done by the Panel. In 1944, the Panel responded to a request from the Army Air Force (AAF) asking for collaboration "in determining the most effective tactical application of the B-29 airplane" by setting up three contracts: one at the University of New Mexico, to carry on large-scale experiments; a second at Mt. Wilson Observatory, to carry on small-scale optical studies; and a third at Princeton, to provide mathematical support for the whole undertaking.[12] At Mt. Wilson the staff was concerned principally with the defensive strength of single B-29's against fighter attack, and the effectiveness of fighters against B-29's. One indirect result of the optical studies was a set of moving pictures showing the fire-power variation of formations as a fighter circles about them. Warren Weaver reports that, concerning such pictures, the President of the Army Air Forces Board remarked that he "believed these motion pictures gave the best idea to air men as to the relative effect of fire power about a formation yet presented." Certain of these pictures were flown to the Marianas and viewed by General LeMay and by many gunnery officers at the front.[13] The extent to which the claim can be made here for the power of mathematics may be limited, but the study was an effective one.

**Probability and Statistics.** Another part of the Panel's work in the analytical studies of aerial warfare was concerned with flak analysis and fragmentation-and-damage studies. These were based on probability studies of damage to an aircraft or group of aircraft from one or more shots from anti-aircraft guns, with some attention to related problems arising in air-to-air bombing or in air-to-air or ground-to-air rocket fire. Probability considerations arose in a wide array of Panel studies, as did statistical problems. Indeed, the need for the use of statistics and probability theory was so great that there were four contracts concerned with such problems. To quote S. S. Wilks:

> The methodology of research varied from formal mathematical analysis, at one extreme, to synthetic processes and statistical experiments or models at the other. Formal analysis is the more precise and hence satisfying process, but the difficulties of formulating the problem in analytical terms and then (worse) of finding numerical solutions increase rapidly with the complexity of the bombing situation. For example, it is very easy to deduce almost all the probability consequences regarding the problem of aiming a single bomb at a rectangular target, but very few deductions can be made directly from the equations which describe the dropping of a train of as few as three bombs on a rectangular target. Since the problem of dropping a train of three bombs is itself extremely simple, compared to many common bombing operations, it is apparent that formal mathematical processes cannot alone be depended upon to carry the burden, but they are powerful when used in conjunction with synthetic methods and statistical models.[14]

By the end of the war the major effort of three of the four statistical research groups was being spent on nineteen studies dealing with probability and statistical aspects of bombing problems.

The other major fields in which statistical work was being carried on were the development of statistical methods in inspection, research, and development work; the development of new fire effect tables (work that was continued after the war under a contract between Princeton and the Navy); and miscellaneous studies relating to such things as spread angles for torpedo salvos, land mine clearance, and search problems.

*Statistical Methods in Inspection, Research, and Development: The Genesis of Sequential Analysis.* The first of these major fields, the development of statistical methods in inspection, research, and development, was assigned to the largest of the statistical research groups, the one at Columbia (SRG-C). W. Allen Wallis, the Director of Research of this group, said in a recent speech[15] that this was surely the most extraordinary group of statisticians ever organized, taking into account both number and quality, and that it was a model that has not been equaled of an effective statistical consulting group. I can certainly attest that it was a tremendously productive group and an exciting one to be associated with. The great bulk of its work was in consulting or in the investigation of problems of a predominantly statistical or probabilistic nature. It developed a variety of useful materials, both theoretical and practical, that have become established parts of statistics. The most striking of these is sequential analysis, called by Wallis "one of the most powerful and seminal statistical ideas of the past third of a century." He reports that the 1975 and 1976 volumes of *Current Index to Statistics* each lists between 50 and 55 articles that include the term "sequential analysis" in their titles, and he asserts that sequential analysis continues to be one of the dominant themes in statistical research.

The importance of sequential analysis during the war is attested by Warren Weaver. He writes in his summary of AMP's work:

> During the war, it was recognized by the Services that the statistical techniques which were developed by the Panel for Army and Navy use, on the basis of the new theory of sequential analysis, if made generally available to industry, would improve the quality of products produced for the Services. In March 1945, the Quartermaster General wrote to the War Department liaison officer for NDRC a letter containing the following statement: "By making this information available to Quartermaster contractors on an unclassified basis, the material can be widely used by these contractors in their own process control and the more process quality control contractors use, the higher quality the Quartermaster Corps can be assured of obtaining from its contractors. For, by and large, the basic cause of poor quality is the inability of the manufacturer to realize when his process is falling down until he has made a considerable quantity of defective items . . . With thousands of contractors producing approximately billions of dollars worth of equipment each year, even a 1% reduction in defective merchandise would result in a great saving to the Government. Based on our experience with sequential sampling in the past year, it is the considered opinion of this office that savings of this magnitude can be made through wide dissemination of sequential sampling procedures." On the basis of this and similar requests, the Panel's work on sequential analysis was declassified, and the reports . . . were published. The Quartermaster Corps reported in October 1945 that at least 6,000 separate installations of sequential sampling plans had been made and that in the few months prior to the end of the war new installations were being made at the rate of 500 per month. The maximum number of plans in operation simultaneously was nearly 4,000.[16]

The story of the genesis of sequential analysis is given below chiefly because the tale is an interesting one but also because of the importance of the results at the time of their discovery and their continuing importance. The following account is excerpted from a letter sent to Warren Weaver by Allen Wallis in March 1950 in response to a question asked by Weaver in January of that year:

> Late in 1942 or early in 1943 you assigned us the task of evaluating an approximation developed by (Navy) Captain Garret L. Schuyler that was supposed to simplify a complicated British formula for calculating the probability of a hit by anti-aircraft fire on a directly approaching dive bomber. Schuyler's approximation was no good. Ed Paulson worked on the problem for us and was able to give rather simple formulas bounding the correct probability . . .
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> [Paulson and I worked up] material on comparing two proportions which is now presented in Chapter 7 of *Techniques of Statistical Analysis*. When I presented this result to Schuyler, he was impressed by the largeness of the samples required for the degree of precision and certainty that seemed to him desirable in ordnance testing. Some of these samples ran to many thousands of rounds. He said that when such a test program is set up at Dahlgren [U.S. Naval Proving Ground] it may prove wasteful. If a wise and seasoned ordnance expert like Schuyler were on the premises, he would see after the first few thousand or even few hundred rounds that the experiment need not be completed . . . he thought it would be nice if there were some mechanical rule which could be specified in advance stating the conditions under which the experiment could be terminated earlier than planned . . .
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> . . . Several days after I returned to New York I got to thinking about Schuyler's comment . . .
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> This was early in 1943, after Milton Friedman had joined SRG but before he had been able to move his family to New York. He was commuting from Washington to New York for two or three days each week. He and I regularly had lunch together, and one day I brought up Schuyler's suggestion. We discussed it at some length, and came to realize that some economy in sampling can be achieved merely by applying an ordinary single-sampling test sequentially. That is, it may become impossible for the full sample to lead to rejection, or for it to lead to acceptance, in which case there is no sense in completing the full sample. The fact that a test designed for its optimum properties with a sample of predetermined size could be still better if that sample size were made variable naturally suggested that it might pay to design a test in order to capitalize on this sequential feature; that is, it might pay to use a test which would not be as efficient as the classical tests if a sample of exactly $N$ were to be taken, but which would more than offset this disadvantage by providing a good chance of terminating early when used sequentially. Milton explored this idea on the train back to Washington one day, and cooked up a rather pretty but simple example involving Student's $t$-test.
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> When Milton returned to New York we spent a great deal of time at lunches over this matter . . . We finally decided to bring in someone more expert in mathematical statistics than we . . . We decided to turn the whole thing over to Wolfowitz.
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> The next day we talked with Jack but were totally unable to arouse his interest . . .
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> We got Wald over the next morning and explained the idea to him . . . We presented the problem to Wald in general terms for its basic theoretical interest . . .
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> At this first meeting Wald was not enthusiastic and was completely non-committal . . .
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> The next day Wald phoned that he had thought some about our idea and was prepared to admit that there was sense in it. That is, he admitted that our idea was logical and was worth investigating. He added, however, that he thought nothing would come of it; his hunch was that tests of a sequential nature might exist but would be found less powerful than existing tests. On the second day, however, he phoned that he had found that such tests do exist and are more powerful, and furthermore he could tell us how to make them. He came over to the office and outlined his sequential probability ratio to us. This is the ratio of the probability under the null-hypothesis, with which I had been puttering around, to the probability under the alternative hypothesis—or rather, the reciprocal of this ratio. He found the critical levels by an inverse probability argument, showing that the same critical levels result no matter what assumption is made about the *a priori* distribution . . .
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> While it later developed that there had been previous work related to sequential analysis, you can see from the foregoing account that Wald's development did not actually grow out of preceding work . . .
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> . . . While Wald was still preparing his monograph on the theory,[17] we started to work on a book on applications. We were understaffed at that time, and other work had higher priority. Finally, we arranged with Harold Freeman of MIT to take on the job as a special assignment. He wrote the first version of *Sequential Analysis of Statistical Data: Applications*. While he was working on this, he was called in by the

Boston office of the Quartermaster Corps for advice on acceptance inspection, and it seemed to him that sequential analysis was eminently suitable for their problem. He therefore gave a series of lectures to the staff, including the top officer, a Colonel Rogow, who had come to the Quartermaster Corps from Sears Roebuck and who after the war became president of Eversharp . . . Rogow encountered considerable opposition in introducing sequential analysis, particularly from the Army Ordnance Department . . . but he achieved an amazingly quick revolution in the QMIS. Actually, sequential analysis deserves only a small part of the credit for the total improvement achieved. Much of the improvement was due simply to better methods of inspecting given items, better methods of reporting, etc. Nevertheless, sequential analysis became the opposite of a scapegoat: something to which all the credit could be attached, so that it would not be necessary to say that they were simply doing what could have been done twenty years sooner.

The Navy interest in sequential analysis came first from John Curtiss. I gave him Wald's basic formulas at lunch one day . . . He was quick to perceive the usefulness of sequential analysis in sampling inspection work. Curtiss was the first to suggest to me that the decision criteria be transformed from levels of the likelihood ratio to levels for the actual count of defectives, to be shown as a function of sample size. This was an adaptation of the standard tables of acceptance and rejection numbers used by Army Ordnance and taken by them from the Bell Laboratories. At SRG we later thought of the graphical presentation of these acceptance and rejection numbers.

### The Effect of Wartime Pursuits on Mathematicians and Statisticians

The foregoing account will, I think, justify Wallis's claim for the importance of sequential analysis and his pride in the fact that it originated in the Statistical Research Group at Columbia. He makes another claim for that Group—that it contributed definitively to the subsequent careers of a substantial number of men who were to become leaders in statistics in the next three decades. One may say more generally, I think, that for a number of mathematicians, whether their work was in AMP or elsewhere, what they did during the war had a substantial impact on their subsequent careers. Herman Goldstine became a computer authority, Barkley Rosser became a versatile applied mathematician, John Curtiss committed himself for a considerable period to the building and administration of the Applied Mathematics Laboratories of the National Bureau of Standards. And there are many others whose careers were essentially changed.

As to other claims made by Wallis for the Statistical Research Group at Columbia, these, too, apply more generally. I have already emphasized the consulting role played by many Panel mathematicians; and the quality of the members of all the groups was truly noteworthy. In particular, the Applied Mathematics Group at Columbia, like the Statistical Research Group there, was distinguished by the quality and number of its members. However, its work was very diverse and constrained by the needs of wartime problems. Thus, in spite of its wartime importance, the work of AMG-C did not serve as a basis for a mathematical field of growing importance as did the work of the Applied Mathematics Group at New York University and that at Brown. But, during and after the war, the work at AMG-C was much appreciated. The Naval Ordnance Development Award was conferred on the Group for distinguished service to the research and development of Naval Ordnance; and the military services used the Group as consultants on a wide variety of problems.

### Military Evaluations of Contributions of Mathematicians

In a conversation with Warren Weaver in June 1978, shortly before his death, I asked him how he assessed the view of the military of the value of AMP's work. He said that, initially, their attitude toward the Panel was a pretty restrained one. There were few people in the Army who had had enough training to have any concept of what could be done, a principal exception being Major (now General) Simon of Aberdeen. Many of the Army aviators had had more scientific training than the men in the other branches of the Army, and many of the Navy people were eager for help; so the Navy and what later became the Air Force were among the "first believers."

Problems were usually forwarded to the Applied Mathematics Panel after a responsible person in the services had written to Warren Weaver saying that they had a problem and, though they were not at all sure that the Panel could help, they would like to get together to discuss it. Then a group from the Panel would go down to Washington for a meeting that usually brought in some "high brass." Fortunately, some of the early problems were easy to solve. One particular one was concerned with the determination of the kind of barrage of torpedoes to lay down against a big Japanese vessel to maximize the probability of hitting the ship. The Navy had no idea how fast the vessels concerned could accelerate in a straight line, how rapidly they could turn, etc., but they did have good photographs of large numbers of Japanese vessels. The people at NYU quickly provided the information that, in 1887, Lord Kelvin had established that the waves following a ship moving in a straight line are confined to a sector of semi-angle 19°28′ regardless of the ship's size and speed, provided the speed is constant. The ship's speed is indicated by the spacing of cusps along the bow waves.[18]

Since the photographs of the Japanese ships were almost always taken in turns it was desirable to extend Lord Kelvin's analysis to turning ships. We found that this could be done rather simply and that we could get the data we needed from a picture of the wavelets. In a test

In the ensuing years, universities in the United States developed a variety of ways in which to handle the interest of students and potential employers in the availability of instruction in operations research. In some universities, departments of operations research were established in the liberal arts college. In others, the subject was taught in the business school and, usually, in the engineering school. The patterns have great variety.

One of the most prominent fields of operations research, linear programming, was started in 1946 and was a natural continuation of Air Force planning activities that had developed during the war. Extraordinary coordination had been required during the war to ensure that the economy had the capability to relinquish men, materiel, and productive capacity from the civilian to the military sector on a schedule that permitted necessary training of men, deployment in combat theaters, supply and maintenance, and a wide spectrum of other requirements. Time was a critical factor.

George Dantzig, when he returned to the Office of the Air Controller after completing his Ph.D. in 1946, was requested to mechanize this planning, since it seemed likely that electronic computers with very large capacity and great speed would soon become available. He realized that the complex wartime procedures were unsuitable for high-speed computation. He found that the equations to be satisfied in order to achieve the required degree of combat readiness at a stated time were so complicated that he could not see how to impose the additional requirement of minimum cost. Finally, he saw that the goal of the complex procedures used during the war could be achieved by using inequalities instead of equations. By the end of 1947, he had described the problem mathematically, formulated a method of solution, and recognized that there was a wide range of applications. Mathematically, the problem is to find a solution of a system of linear equations and linear inequalities that minimizes a linear form.

Dantzig arranged to have the Mathematical Tables Project of the National Bureau of Standards test the method he proposed (the simplex method) on the diet problem formulated by George Stigler in 1945,[24a] carrying out the computations by hand. The solution required nearly 17,000 multiplications and divisions, which were carried out by five statistical clerks using desk computers in 21 working days. This was the first life-size computation to be performed by the simplex method, and it established that the method would be practicable for virtually all problems once appropriate electronic computing machines became available.[24b]

Although Fourier,[25] in the 1820's, and Kantorovich,[26] in 1938 and subsequently, had also realized the importance of the subject and devised methods in many ways similar to those of Dantzig for solving these problems, Fourier died in 1830 without developing his ideas, and Kantorovich published his results in a monograph that was unknown outside of Russia until it came to the attention of T. C. Koopmans in the middle 1950's and was translated into English through his efforts. Thus the contemporary development of linear programming stems directly from the Air Force beginning. This development was of first importance both to economic theory and to phases of practice in business and industry that were central to operations.

In addition to Dantzig's Air Force colleagues, the Washington mathematical community furnished active support. The National Bureau of Standards provided research and computing assistance, and the Office of Naval Research gave support for related university research. In this respect, special mention should be made of the Princeton project under A. W. Tucker, which catalyzed the interest of academic mathematicians. Tucker and his former students, David Gale and Harold Kuhn, were active in developing and systematizing the underlying theory of linear inequalities. Their main efforts were in game theory, whose equivalence with linear programming had been conjectured by von Neumann as early as October 1947, when he met George Dantzig for the first time and learned from him of his efforts in linear programming.[27]

The role of catalyst for economists was played by T. C. Koopmans, who had, in fact, anticipated some aspects of linear programming concepts in research in transportation theory he had undertaken during the war.[28] He recognized the importance of Dantzig's work and identified the implications of linear programming for the whole theory of resource allocation.
of the mathematical results in an experimental run of a new destroyer, the agreement of theory and observation was extremely good—within a few percent for both speed and turning radius.[19] The Navy found this result impressive. The method developed by the Applied Mathematics Panel was adopted by the Navy's Photographic Interpretation Center, which incorporated much of the research in an official handbook. This and similar experiences won over the armed services to the notion that mathematics could be of great help to them.

There were, of course, many problems to which we could make no useful contribution. But there were also some important successes, as illustrated in the following account given in Warren Weaver's Summary.[20]

> In January 1944, Brigadier General Robert W. Harper, AC/AS (Training), wrote in a letter to Dr. Vannevar Bush, Director of OSRD, that "the problems connected with flexible gunnery are probably the most critical being faced by the Air Forces to-day. It would be difficult to state the importance of this work or the urgency of the need; the defense of our bomber formations against fighter interception is a matter which demands increasing coordinated expert attention."...
>
> The immediate proposal contained in General Harper's letter was that the Applied Mathematics Panel should recruit and train competent mathematicians who had the "versatility, practicality, and personal adaptability requisite for successful service in the field"; it was planned that these men, after two months' training in this country, would be assigned to the Operations Research Sections in the various theaters to devote their attention to aerial flexible gunnery problems. The Panel was in a position to carry out this program because it had already been drawn into studies of rules for flexible gunnery training and because it had access to many of the ablest young mathematicians in the country. The assignment was completed promptly [and was much appreciated by the Air Forces].

In June 1944, General Harper, in a letter to Dr. Bush, paid tribute to OSRD for the outstanding work done in training the ten mathematicians for Operational Research Groups and stated that the demands for more such men had come in at such a rate that it was deemed necessary to train eight additional mathematicians.[21] The recruitment of these men proved more difficult than in the earlier training assignment because so many "competent and willing mathematicians had already entered upon war work." (See Note 21.) However, the task was successfully completed. One of those recruited in this second group, Dr. John W. Odle, reports:

> [The] training was extremely valuable to me and was directly applicable to my subsequent assignment in the flexible gunnery subsection of the Operations Research Group at the Eighth Air Force in England. Without the general orientation and the specialized instruction that I received ... I would have been woefully lost in a field of endeavor that was completely new and unfamiliar to me ... The training certainly opened up immense new vistas to me. In fact, that introduction to OR, and my later wartime experiences as a practitioner, completely changed the course of my career.[22]

### Some Effects of Wartime Work on Mathematics

This and other wartime programs that put American mathematicians in touch with operations research activities being carried on in the field, as well as those being pursued in the United States, had an effect after the war's end. Two post-war efforts to increase interest in nonmilitary uses of operations research should be mentioned. The first is a speech by Philip M. Morse, head of the Operations Research Group of the U.S. Navy during the war, who was the Josiah Willard Gibbs lecturer at the meeting of the American Mathematical Society in December 1947. He spoke on the subject "Mathematical Problems in Operations Research," basing his paper on several mathematical problems that arose in operations research during World War II.[23] The paper emphasized the potentials for use of operations research in peacetime applications, in particular, in business and industry. The second post-war effort to increase interest in the peacetime uses of operations research that I shall mention was an undertaking of the National Research Council. In April 1951 the Council published a brochure prepared by its Committee on Operations Research, entitled "Operations Research with Special Reference to Nonmilitary Applications," which sought to introduce the methods of operations research into business and industry in the United States.

Koopmans and Kantorovich shared a Nobel prize in economics for work involving linear programming. Other Nobel Laureates in economics associated with the subject include Kenneth Arrow, Ragnar Frisch, Wassily Leontieff, Paul Samuelson, and Herbert Simon.

The Navy's interest in linear programming was based on a recognition of its potential contributions to the Navy's logistics operations. ONR's Logistics Program was set up in 1947, and a separate Logistics Branch of the Mathematical Sciences Division was established in 1949.

### Summary and Conclusion

In 1968, the National Academy of Sciences published a report[29] that comments on the development of new fields that "combine the use of numerical data . . . with mathematical models to provide guidance for managerial action and judgment." It says, in part:

> During World War II, the use of simple mathematical models and mathematical thinking to study the conduct of military operations became a recognized art, as first scientists and later mathematicians, lawyers, and people with other backgrounds demonstrated its effectiveness. After the war, attempts to apply the same attitudes and approaches to business and industrial operations and management were pressed forward rather successfully. Combined with techniques and thinking drawn from, or suggested by, classical economics, this line of development has now led to an active field [variously called management science, operations research, cost-benefit analysis, optimization theory, mathematical programming, etc.] . . .
>
> Whatever the title, the flavor of what is done is the same, combining the use of numerical data about operating experience so characteristic of early military applications with mathematical models to provide guidance for managerial action and judgment. This field was created by scientists accustomed to the use of mathematics; both its spirit and its techniques have always been thoroughly mathematical in character. This mathematical approach is steadily penetrating the practice of management and operation.
>
> A number of the leading schools of business administration have concluded that mathematics is important both as a tool and as a language for management, and that training for the professional class of managers should include a substantial dose of this field of many names. Therefore, calculus, linear algebra, and computer programming either must be prerequisite for entrance or must be taken early in the graduate training program . . .
>
> This field is pervasively mathematized and computerized, but it is far from being strictly a mathematical science. The pattern of its problems is frequently described as formulating the problem, constructing a mathematical model, deriving a solution from the model, testing the model and the solution, establishing control over the solution, and implementing the solution. Only one of the six steps is completely mathematical; the others involve the actual problem in an essential way. In these other steps, of course, there are many applications, some of them crucial, of statistics and computer science. The mathematical step, especially when dealing with management rather than operational problems, often draws on concepts and results from the field of optimized allocation, control, and decision.

> A good practitioner combines the characteristics of most professional consulting and of most effective application of mathematics: abundant common sense, willingness to produce half-answers in a half-hour, recognition of his key roles as problem formulator and contributor to long-run profits (rather than as problem solver or researcher). Yet for all this, and in an alien environment, he must retain his skill as a mathematician.

Under the stimulus of government support, the development of these new fields at a time of expanding availability and greater sophistication in computers has brought about a great increase in the mathematization of many aspects of business and industry.

With the increasing mathematization of society, the Association for Computing Machinery came into being in 1947; the Industrial Mathematics Society, in 1949; the Operations Research Society of America and the Society for Industrial and Applied Mathematics, in 1952; and the Institute of Management Sciences, in 1953. Courses, or components of courses, dealing with mathematics for the behavioral sciences were offered by the mathematics departments of a number of liberal arts colleges with the encouragement of the Mathematical Association of America's Committee on the Undergraduate Program in Mathematics, while, in some universities, separate courses in mathematics were taught in the economics department, the school of industrial management, the engineering school, and so on. In many universities, separate departments have been established with names like Computer Science, Operations Research, Systems Science and Mathematics, and Applied Mathematics. Thus, as the uses of mathematics have expanded in new directions, many institutions have adopted new organizational arrangements to accommodate the new content, much of which reflects developments in the mathematical sciences that grew out of military requirements in World War II.

## Notes

1. See: Mina S. Rees, Mathematics and the government: The post-war years as augury of the future, in The Bicentennial Tribute to American Mathematics, 1776–1976, Mathematical Association of America, 1977, pp. 101–116.

2. After a stay in Turkey, Prager had been appointed Professor at Brown in 1941.

3. William Prager, Quart. Appl. Math., 30 (1) (1972) 1.

4. Churchill Eisenhart, Dictionary of Scientific Biography, vol. 14, Scribner, New York, 1976, pp. 383, 384.

5. Warren Weaver, Scene of Change, Scribner, New York, 1970, p. 87.

6. In the spring of 1942, a presentation was made to James B. Conant, Chairman of NDRC, and Vannevar Bush, Director of OSRD, by Marshall Stone and Marston Morse, as representatives of the American Mathematical Society. The discussion was based on a carefully prepared memorandum that described wartime activities considered appropriate for members of the American Mathematical Society. The establishment of the Applied Mathematics Panel may have been influenced by this presentation, but the American Mathematical Society was not consulted about the nature of the work to be undertaken by AMP, nor about its staffing pattern, and there were initial complaints about what was perceived as too little use of distinguished "pure" mathematicians in the work of the Panel.

7. The M-9 director was spectacularly successful during the buzz bomb attacks on Britain in 1944, working in combination with automatic radar tracking developed by the Radiation Laboratory and the proximity fuse developed by the fuse section of OSRD. General Sir Frederick A. Pile, who was in charge of the British Anti-Aircraft Command at that time, wrote to General George Marshall in August 1944: "The equipment you have sent us is absolutely first class. . . As the troops get more expert with [it] I have no doubt very few bombs will reach London." His prediction proved to be correct.

8a. This account is adapted from Warren Weaver's autobiography (see Note 5), pp. 78–87.

8b. The location of contracts established by the Applied Mathematics Panel, with the names of the "Technical Representatives," follows:

> *Applied Mathematics Groups*: NYU, R. Courant; Columbia, E. J. Moulton, S. Mac Lane, A. Sard; Brown, R. G. D. Richardson; Institute for Advanced Study, J. von Neumann; Princeton, M. M. Flood; Northwestern, E. J. Moulton, W. Leighton; Carnegie Institution of Washington, Pasadena, W. S. Adams; Harvard, Garrett Birkhoff; University of New Mexico, E. J. Workman.

*Statistical Research Groups*: Columbia, H. Hotelling; University of California (Berkeley), J. Neyman; Columbia, J. Schilt; Princeton, S. S. Wilks.

*Computation*: The Franklin Institute, H. B. Allen; The National Bureau of Standards, Arnold Lowan.

9. This account draws freely on Warren Weaver's Summary that appears in each of the three volumes of the Summary Technical Report of the Applied Mathematics Panel, NDRC, Washington, D.C., 1946. This was published with a confidential classification, but the whole of the report has now been declassified.

10. Richard Courant and K. O. Friedrichs, Supersonic Flow and Shock Waves, Interscience, New York, 1948.

11. Summary Technical Report of the Applied Mathematics Panel (see Note 9), vol. 2, pp. 9–124.

12. Ibid., vol. 2, pp. 197–220.

13. Ibid., vol. 2, p. 3.

14. Ibid., vol. 3, Probability and Statistical Studies in Warfare Analysis, p. ix.

15. This was an invited address delivered on August 14, 1978, at a meeting of the American Statistical Association. It was entitled "The Statistical Research Group, 1942–1945." It is to be published in revised form by the Journal of the American Statistical Association, 8 June 1980.

16. Warren Weaver (see Note 9), p. 5.

17. Abraham Wald, Sequential Analysis, Wiley, New York, 1947. The basic work on this volume was done at SRG-C and was published as a restricted report in 1943 by the Applied Mathematics Panel.

18. Lord Kelvin (Sir W. Thomson), On the waves produced by a single impulse in water of any depth, or in a dispersive medium, Proc. of the Royal Society of London, Ser. A, 42 (1887) 80–85.

19. J. J. Stoker, Water Waves: The Mathematical Theory with Applications, Interscience, New York, 1957, pp. 229, 230.

20. Warren Weaver (see Note 9), pp. 3, 4.

21. Saunders Mac Lane, Summary Report on AMP Study 103, AAF Training Program, Columbia University Division of War Research—Applied Mathematics Group, 24 August 1945.

22. John W. Odle, Letter to Dr. Churchill Eisenhart, 21 May 1979.

23. Bull. Amer. Math. Soc., 54 (1948) 602–621.

24a. George J. Stigler, The cost of subsistence, Journal of Farm Economics, 27 (2) (May 1945) 303–314.

24b. New results, which would provide a possibly significant improvement on this method of solution, were reported in January 1979 by a Russian mathematician. These results were unknown in America until early summer 1979. See L. G. Hačijan, A polynomial algorithm in linear programming, Soviet Math. Dokl., 20 (1979) 191–194.

25. Jean Baptiste Joseph Fourier, Solution d'une question particulière du calcul des inégalités, in Oeuvres de Fourier, Tome Second, Gauthier-Villars, Paris, 1826, pp. 315–328 (including notes by G. Darboux). Darboux comments in the Preface to the second volume of the Oeuvres that Fourier's enthusiasm for the problem seemed to be somewhat exaggerated.

26. L. V. Kantorovich, Mathematical Methods of Organizing and Planning Production, Leningrad S.U. Press, Leningrad, 1939.

27. George B. Dantzig, Linear programming and its progeny, Naval Research Reviews, Office of Naval Research, Washington D.C., June 1966, p. 6.

28. T. C. Koopmans, Exchange ratios between cargoes on various routes (Memorandum for the Combined Shipping Adjustment Board, Washington D.C., 1942, pp. 1–12), in Scientific Papers of Tjalling C. Koopmans, Springer, New York, 1970, pp. 77–86.

29. The Mathematical Sciences: A Report, National Academy of Sciences, publication #1681, 1968, pp. 113, 114.