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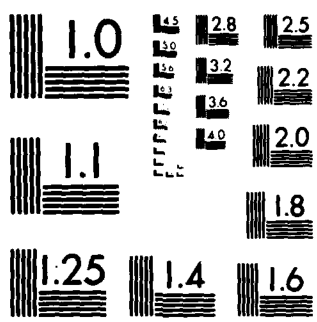

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REMINISCENCES ABOUT THE ORIGINS OF LINEAR PROGRAMMING

by

George B. Dantzig

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REMINISCENCES ABOUT THE ORIGINS OF LINEAR PROGRAMMING

George B. Dantzig[1]

ABSTRACT: Some recollections about the early days of linear programming, the contributions of von Neumann, Leontief, Koopmans and others, and about some of the extensions that have taken place from 1950-1970 and some from 1970-1980.

Linear Programming is viewed as a revolutionary development giving us the ability for the first time to state general objectives and to find, by means of the simplex method, optimal policy decisions to practical decision problems of great complexity.

In the years following its conception in 1947, in connection with the planning activities of the military, **linear programming** has come into wide use.

In academic circles mathematicians, economists, and those who go by the name of Operations Researchers or Management Scientists, have written hundreds of books on the subject and, of course, an unaccountable number of articles.

Interestingly enough, in spite of its wide applicability to everyday problems, linear programming was unknown prior to 1947. It is true that two or three individuals may have been aware of its potential -- for example Fourier in 1823 and de la Vallee Poussin in 1911. But these were isolated cases. Their works were soon forgotten. Kantorovich in 1939 made an extensive proposal that was neglected by U.S.S.R. It was only after the major developments in mathematical programming had taken place in the West (1959), that Kantorovich's paper became known. To give you some idea of how meager the research effort was: Motzkin in his Ph.D. thesis lists only 42 papers before 1936 on linear inequality systems authored by such names as Stokes, Dines, McCoy, .

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My own contributions to the field grew out of my World War II experience. I had become an expert on programming planning methods using desk calculators. In 1946 I was the Mathematical Advisor to the U.S. Air Force Comptroller. I had just formally completed my Ph.D. and was looking for an academic position. In order to entice me into not taking another job, colleagues challenged me to see what could be done to mechanize the planning process. I was asked to find a way to more rapidly compute a time-staged deployment, training and logistical supply program. In those days mechanization meant using analog devices or punch card equipment.

Consistent with my training as a mathematician, I set out to formulate a model. I was fascinated by the work of Wassily Leontief who proposed in 1932 a simple matrix structure which he called the Interindustry Input-Output Model of the American Economy. It was simple in concept and could be implemented in sufficient detail to be useful for practical planning. I soon saw that it had to be generalized. Leontief's was a steady-state model and what was needed was a highly dynamic model, one that could change over time. In Leontief's model there was a one-to-one correspondence between the production processes and the items produced by these processes. What was needed was a model with many alternative activities. The application was to be large scale, hundreds of items and activities. Finally it had to be computable. Once the model was formulated, there had to be a practical way to compute what quantities of these activities to engage in that was consistent with their respective input-output characteristics and with given resources. The model I formulated would be described today as a time-staged, dynamic linear program with a staircase matrix structure. Initially there was no objective function; explicit goals did not exist because practical planners simply had no way to implement such a concept.

A simple example illustrates the fundamental difficulty of formulating a planning problem using an activity analysis approach. Consider the problem of assigning 70 men to 70 jobs. An "activity" consists in assigning the i-th man to the j-th job. The restrictions are: (1) each man must be assigned, there are 70 such, and (2) each job must be filled, also 70. The level of an activity is either 1, meaning it will be used or 0, meaning it will not. Thus are 2 x 70 or 140 restrictions, 70 x 70 or 4900 activities with 4900 corresponding zero-one decision variables. Unfortunately there are 70! different possible solutions or ways to make the assignments. The problem is to compare one with another and select one which is "best" by some criterion.

Now 70! is a big number, greater than 10^{100}. Suppose we had an IBM 370-168 available at the time of the big bang 15 billion years ago. Would it have been able to look at all the 70! combinations by the year 1981? No! Suppose instead it could examine 1 billion assignments per second? The answer is still no. Even if the Earth were filled with such computers all working in parallel, the answer would still be no. If, however, there were 10^{50} earths or 10^{44} suns all filled with nano-second speed computers all programmed in parallel from the time of the big bang until sun grows cold, then perhaps the answer is yes.

This simple example illustrates why up to 1947, and for the most part to this day, a great gulf exists between man's aspirations and his actions. Man may wish to state his wants in terms of an objective to be extremized but there are so many different ways to go about it, each with its advantages and disadvantages, that it was impossible to compare them all and choose which among them is the best. Invariably man has turned to a leader whose "experience" and "mature judgment" would guide the way. Those in charge like to do this by simply issuing a series of ground rules or edicts to be executed by those developing the program. This was the situation in late 1946. I had formulated a model that satisfactorly represented the technological relations usually encountered in pratice. In place of an explicit goal or objective function were a large number of ad hoc ground rules issued by those in authority to aid the selection. Without the latter, there would be, in most cases, an astronomical number of feasible solutions to choose from.

All that I have related up to now in the early development took place before the advent of the computer, more precisely, before in late 1946 we were aware that it was going to exist.

To digress for a moment, I would like to say a few words about the electronic computer. To me, and I suppose to all of us, one of the most startling developments of all time is the penetration of the computer into almost every phase of human activity. Before a computer can be intelligently used, however,a model must be formulated and good algorithms developed. To build a model requires the axiomatization of a subject matter field. In time this gives rise to a whole new mathematical discipline which is studied for its own sake. Thus, with each new penetration of the computer, a new science is born.

Von Neumann notes this tendency to axiomatize in his paper on The General and Logical Theory of Automata. In it he states that automata have been playing a continuously increasing role in the natural sciences. Automata have begun to invade certain parts of mathematics too, particularly but not exclusively mathematical physics or applied mathematics.

Their role in mathematics presents an interesting counterpart to certain functional aspects of organization in nature. The natural systems (e.g., central nervous system) are of enormous complexity and it is clearly necessary first to subdivide what they represent into several parts which to a certain extent are independent, elementary units. The problem then consists of understanding how these elements are organized as a whole. It is the latter problem which is likely to attract those who have the background and tastes of the mathematician or a logician. With this attitude, he will be inclined to forget the origins and then, after the process of axiomatization is complete, concentrate on the mathematical aspects. (End of the paraphrase of von Neumann.)

By mid-1947 I decided that the <u>objective</u> had to be made explicit. I formulated the planning problem as a set of axioms. The axioms concerned the relations between two kinds of sets: the first were the set of items being produced or consumed and the second the set of activities or production processes in which items could be inputed or outputed in fix proportions as long as the proportions were non-negative multiples of each other. The resulting mathematical system to be solved was **the minimization of a linear form subject to linear equations and inequalities.** The use of the linear form as the objective function to be extremized was the <u>novel</u> feature.

Now came the non-trivial question: Can one solve such systems? At first I assumed the Economists had worked on this problem. So I visited T.C. Koopmans in June 1947 at the Cowles Foundation at the University of Chicago to learn what I could from mathematical economists. Koopmans became quite excited. During World War II he worked for the Allied Shipping Board on a transportation model and so he had the theoretical as well as the practical planning background necessary to appreciate what I was presenting. He saw immediately the implications for general economic planning. From that time on, Koopmans took the lead in bringing the potentialities of linear programming models to the attention of young economists like K. Arrow, P. Samuelson, H. Simon, R. Dorfman, L. Hurwicz to name but a few. Their research led to several Nobel Prizes in Economics.

Seeing that economists did not have a method of solution, I next decided to try my own luck at finding an algorithm. I owe a great debt to Jerzy Neyman, the mathematical statistician, who guided my graduate work at Berkeley. My thesis was on two famous unsolved problems in mathematical statistics which I mistakeningly thought was a homework assignment and solved. One of them, later published joint with Wald, was on the Neyman-Pearson Lemma. In today's terminology, my thesis was on the existence of Lagrange multipliers (or dual variables) for a general linear program over a continuum of variables each bounded between zero and one and satisfying linear constraints expressed in the form of Lebesgue integrals. There was also a linear objective to be extremized. The particular geometry used in my thesis was in the dimension of the columns instead of the rows. This column geometry gave me the insight that made me believe the **Simplex Method** would be a very efficient solution technique for solving linear programs. This I proposed in the summer of 1947 and by good luck it worked!

It was nearly a year later however in 1948, before we realized just how powerful the Simplex Method really was. In the meantime, I decided to consult with the "great" Johnny von Neumann to see what he could suggest in the way of solution techniques. He was considered by many as the leading mathematican in the world. On October 3, 1947, I visited him for the first time at the Institute for Advanced Study at Princeton. I remember trying to describe to von Neumann, as I would to an ordinary mortal, the Air Force problem. I began with the formulation of the linear programming model in terms of activities and items, etc. Von Neumann did something which I believe was uncharacteristic of him. "Get to the point," he said impatiently. Having at times a somewhat low kindling-point, I said to myself "O.K., if he wants a quicky, then that's what he'll get." In under one minute I slapped the geometric and the algebraic version of the problem on the blackboard. Von Neumann stood up and said "Oh that!" Then for the next hour and a half, he proceeded to give me a lecture on the mathematical theory of linear programs.

At one point seeing me sitting there with my eyes popping and my mouth open (after all I had searched the literature and found nothing), von Neumann said: "I don't want you to think I am pulling all this out of my sleeve on the spur of the moment like a magician. I have just recently completed a book with Oscar Morgenstern on the Theory of Games. What I am doing is conjecturing that the two problems are equivalent. The theory that I am outlining for your problem is an analogue to the one we have developed for games. Thus I learned about Farkas' Lemma, and about duality for the first time. Von Neumann promised to give my problem some thought and to contact me in a few weeks. He did write to me proposing an iterative scheme which Alan Hoffman and his group at the Bureau of Standards around 1952 compared with the Simplex Method and also with proposals of Motzkin. The Simplex Method came out a clear winner.

As a result of another visit to Princeton in June 1948, I met Al Tucker. Soon Tucker and his students H. Kuhn and D. Gale began their historic work on game theory, nonlinear programming and duality theory.

The Princeton group became the focal point among mathematicians doing research in these fields. Twelve years later I remember a conversation with Professor Tucker, who had been reading the manuscript of my book Linear Programming and Extensions. Our conversation went like this: "Why", he asked, "do you ascribe duality to von Neumann and not to my group?" "Because he was the first to show it to me." He said, "that is strange for we have found nothing in writing about what von Neumann has done. What we have is his paper On a Maximizing Problem. "True," I said, "but let me send you a paper I wrote as a result of my first meeting with von Neumann." I sent him my report A Theorem on Linear Inequalities, dated 5 January 1948, which contained (as far as I know) the first rigorous proof of duality. Later Tucker asked me, "Why didn't you publish it?", to which I replied: "Because it was not my result -- it was von Neumann's. All I did was write up, for internal circulation, my own proof of what von Neumann outlined. It was my way of educating the people in my office in the Pentagon." Today everyone cites von Neumann as the originator of the duality theorem and credits Tucker, Kuhn and Gale as the publishers of the first rigorous proof.

Not too long after my first meeting with Tucker there was a meeting of the Econometric Society in Wisconsin attended by well-known statisticians, mathematicians and economists like Hotelling, Koopmans, von Neumann, and many others all well known today who were then just starting their careers. I was a young unknown. I remember being quite frightened with the idea of presenting for the first time to such a distinguished audience the concept of linear programming.

After my talk, the chairman called for discussion. For a moment there was silence; then a hand raised. It was Hotelling's. I must hasten to explain that Hotelling was huge. He used to love to swim in the ocean and when he did, it is said that the level of the ocean rose perceptively. This huge whale of a man stood up in the back of the room. His expressive face took on one of those all-knowing smiles we all know so well. He said devastatingly: "But we all know the world is non-linear". Then he majestically sat down. And there I was, a virtual unknown, frantically trying to compose the proper reply to the great Hotelling.

Suddenly another hand in the audience was raised. It was von Neumann. "Mr. Chairman, Mr. Chairman," he said, "If the Speaker does not mind, I would like to reply for him." Naturally I agreed. Von Neumann said: "The speaker titled his talk "Linear Programming' and he carefully stated his axioms. If you have an application that satisfies the axioms, use it. If it does not, then don't," and he sat down. In the final analysis, of course, Hotelling was right. The world is highly non-linear. Fortunately systems of linear inequalities (as opposed to equalities) permits us to approximate most of the kinds of non-linear relations encountered in practical planning.

In 1949, exactly two years from the time the linear progamming was first started, the first conference on mathematical programming (sometimes referred to as the first Symposium on Mathematical Programming) was held at the University of Chicago. Koopmans, the organizer, later titled the proceedings of the conference "Activity Analysis of Production and Allocation." Economists like Koopmans, Arrow, Samuelson, Hurwicz, Dorfman, Georgescu-Roegen, and Simon, mathematicians like Tucker, Kuhn, and Gale and Air Force types like Marshall Wood, Murray Geisler, and myself all made contributions.

The advent or rather the promise of the electronic computer, the exposure of theoretical mathematicans and economists to real problems during the war, the interest in mechanizing the planning process, and the availability of money for such applied research all converged during the period 1947-1949. The time was ripe. The research accomplished in these two short years, in my opinion, is one of the remarkable events of history. The Proceedings of the Conference remains to this very day an important basic reference, a classic!

While editing the proceedings, Koopmans asked me to do something to get rid of a condition I assumed to prove the Simplex Method. He wanted me to try to prove that the algorithm would converge without a non-degeneracy assumption, an assumption which I felt initially was reasonable. After all what was the probability of four planes in three space meeting in a point (for example)! But then something unexpected happened. It turned out that although the probability of a L.P. being degenerate was zero, every practical problem tested by my branch in the Air Force turned out to be so. Degeneracy couldn't happen but it did. It was the rule not the exception!

I proposed a method of perturbation of the RHS as a way of avoiding degeneracy when using the simplex method. The proofs I outlined and gave as homework exercises to classes that I was teaching at the time. Edmondston and others turned in proofs (March 1951). In the summer of 1951, Philip Wolfe, then a student at Berkeley, spent the Summer with me at the Pentagon and proposed a lexicographic interpretation of the perturbation idea which Wolfe, Orden and I published as a joint paper. A. Charnes independently developed a different perturbation scheme. Years later, Wolfe proposed a third way (based on my inductive proof of the simplex method) that is, in my opinion, the best one because it resolves degeneracy using only one extra column of information. Whether or not such a scheme is needed in practice has never been settled. It has been observed recently (1981) that even when there is no degeneracy, there is a high probability of near degeneracy. This suggests that pivot selection criteria should be designed to seek feasible solutions in directions away from degenerate and near degenerate basic feasible solutions. Doing so should reduce the total number of iterations.

The simplex method is also a powerful theoretical tool for proving theorems. To prove theorems it is essential that the algorithm include a way of avoiding degeneracy.

In the 1950's and 1960's many new subfields began to emerge. I have only time to say a few words about each.

Non-linear programming began around 1951 with the famous Kuhn-Tucker Conditions which are related to the Fritz-John Conditions (1948). Later Terry Rockafeller, P. Wolfe, R. Cottle, and others developed the theory of non-linear programming and extended the notions of duality.

Commerical Applications were begun in 1951 by Charnes and Cooper. Soon thereafter, practical applications began to dominate.

Network Flow Theory began to evolve around 1954. Ford, Fulkerson, and Hoffman showed the connections to graph theory. Recent research on combinatorial optimization is an outgrowth of their research.

Large-Scale Methods (my field) began in 1955 with my paper "Upper Bounds, Block Triangular Systems, and Secondary Constraints". In 1959-60 Wolfe and I published our papers on the Decomposition Principle.

Stochastic Programming began in 1955 with my paper "Linear Programming under Uncertainty", an approach which has been greatly extended by R. Wets in the 1960's. Important contributions to this field have been made by A. Charnes. Stochastic Programming is, in my opinion, one of the most promising fields for future research, one closely tied to large-scale methods.

Integer Programming began in 1958 by R. Gomory. Unlike the earlier work on the Travelling Salesman Problem by Fulkerson, Johnson and myself, Gomory showed how to systematically generate the cutting planes. Branch and bound techniques, which we also used in our paper, have been studied by E. Balas and many others. Branch and bound has turned out to be the most successful approach in practice for solving Integer Programs.

Complementary Pivot Theory was started around 1962-63 by Richard Cottle and myself and greatly extended by Cottle. It was an outgrowth of Wolfe's method for solving quadratic programs. In 1964 Lemke and Howson applied the algorithm to bimatrix games. In 1965 Lemke extended the approach to other non-convex programs. In the 1970's, Scarf, Kuhn, and Eaves extended it to solving fixed point problems. Lemke's results represent a historic breakthrough into the non-convex domain.

Polynomial Time Algorithms. In 1978 L.G. Khachian showed that an ellipsoidal type algorithm could solve all linear programs in polynomial time. It is an important theoretical result that unhappily cannot be used to solve problems encountered in practice because it is hopelessly slow. This leaves open the question why the simplex method solves the wide class of practical linear programs encountered in approximately linear time. Current research is concerned with proving theorems about the expected number of iterations.

In the late 1960's and 1970's the various subfields of the mathematical programming that I just outlined have each, in turn, grown exponentially. It is impossible for me in this short presentation to sketch these developments.

Before closing let me tell some stories about how various linear programming terms arose. The military refer to their various plans or proposed schedules of training, logical supply and deployment of combat units as a program. When I had first analyzed the Air Force planning problem and saw that it could be formulated as a system of linear inequalities, I called my first paper: Programming in a Linear Structure. In the summer of 1948, Koopmans and I visited the RAND Corporation. One day we took a walk near the Santa Monica beach. Koopmans said: "Why not shorten Programming in a Linear Structure to Linear Programming?" I replied: "That's it! From now on that will be its name." Later that day I gave a talk at RAND, the title **Linear Programming** ". The term **Mathematical Programming** is due to Robert Dorfman who felt as early as 1949 that the term linear programming was too restrictive. The term **Simplex Method** arose out of a discussion with T. Motzkin who felt that the approach that I was using in the geometry of the columns was best described as a movement from one simplex to a neighboring one. Mathematical Programming is also responsible for many terms which are now standard in mathematical literature. Terms like **Arg Min, Arg Max, Lexico-Max, Lexico-Min.** The term **Dual** is not new. But surprisingly the term **primal,** introduced around 1954, is. It came about this way: W. Orchard-Hays, who is responsible for the first commerical grade L.P. software, said to me at RAND one day around 1954: "We need a word that stands for 'the original problem of which this is the dual'." I, in turn, asked my father, Tobias Dantzig, mathematician and author, well-known for his books popularizing the history of mathematics. He knew his Greek and Latin. Whenever I tried to bring up the subject of linear programming, Toby (as he was affectionately known), would become bored. But on this occasion he did give the matter some thought and suggested **Primal** as the natural antonym since both primal and dual derive from the Latin. It was Toby's one and only contribution to linear programming; his sole contribution unless, of course, you want to count the training he gave me in classical mathematics or his part in my conception.

If I were asked to summarize my early and perhaps my most important contributions to linear programming, I would say they are three:

(1) **Recognizing** (as a result of five war-time years as a practical program planner) **that most practical planning relations could be reformulated as a system of linear inequalities.**

(2) **Expressing criteria for selection of good or best plans in terms of explicit goals** (e.g., linear objective forms) and not in terms of ground rules which are at best only a means for carrying out the objective not the objective itself.

(3) **Inventing the simplex method** which transformed a rather interesting approach to economic theory into a basic tool for practical planning of large complex systems.

The tremendous power of the simplex method is difficult to realize. To solve by brute force the Assignment Problem which I mentioned earlier would require a solar system full of nano-second electronic computers running from the time of the big bang until the time the Universe grows cold to scan all the permutations in order to be certain to find the one which is best. Yet it takes only a second to find the Optimum using an IBM 370-168 and standard simplex method software.

In retrospect it is interesting to note that the original problem that started my research is still outstanding -- namely the problem of planning or scheduling dynamically over time. Many proposals have been made on ways to solve large-scale systems of this type such as the Nested Decomposition Principle. Today this is an active, exciting and difficult field having important long term planning applications that could contribute to the well-being and stability of the world.

Prior to linear programming it was not meaningful to explicitly state general goals and so objectives were confused with the ground rules for solution. Ask a military commander what the goal is and he will say "The goal is to win the war." Upon being pressed to be more explicit, a Navy man will say "The way to win the war is to build battleships," or, if he is an Air Force general, he will say "The way to win is to build a great fleet of bombers." Thus the means becomes the objectives and these in turn spawn new ground rules as to how to go about building bombers or space shuttles that again become confused with the goals, etc., down the line.

The ability to state general objectives and then find optimal policy solutions to practical decision problems of great complexity is a revolutionary development. In certain areas such as planning in the petroleum and chemical industries, linear programming has come into widespread use for cost minimization. In other areas such as modeling the dynamics of growing populations of the world against a diminishing resource base, its potential for raising the standard of living has scarcely been realized.

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Some recollections about the early days of linear programming, the contributions of von Neumann, Leontief, Koopmans and others and about some of the extensions that have taken place from 1950-1970 and some from 1970-1980 are discussed. Linear programming is viewed as a revolutionary development giving us the ability for the first time to state general objectives and to find, by means of the simplex method, optimal policy decisions to practical decision problems of great complexity.

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