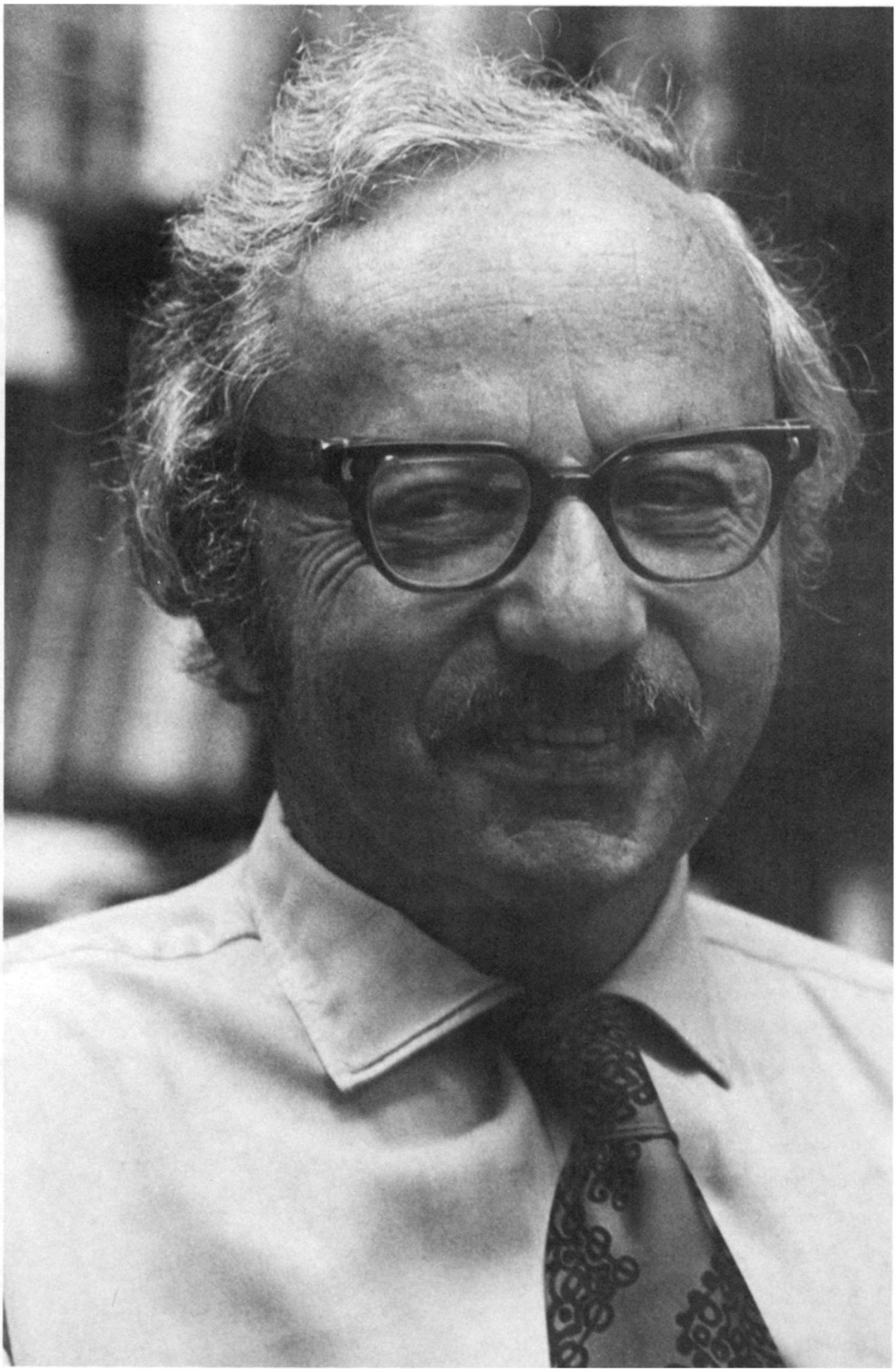


George Bernard Dantzig.

An Interview with George B. Dantzig: The Father of Linear Programming

by Donald J. Albers with Constance Reid

George Bernard Dantzig is rich in many ways, but as a boy growing up in the twenties he wore secondhand shoes. Today he and his wife Anne live in a lovely home on the edge of the Stanford University campus. His study walls are covered with awards, including several honorary doctoral degrees, the John von Neumann Theory Prize, and the National Medal of Science.

His accomplishments are especially noteworthy in view of the trouble that he had with mathematics in junior high school. "I was doing very poorly in my first course in algebra. To be precise, I was flunking."

In 1947 Dantzig invented linear programming and the simplex method. Dantzig's programming led to an explosion of economic, environmental and statistical applications. The iron and steel industry has used his method to evaluate iron ores, explore the addition of coke ovens, and select products. The Federal Energy Administration has used his method to explore energy policy alternatives. Linear programming has also been used or suggested for use to control water and air pollution, refinery scheduling, assigning personnel to jobs, and racially balancing schools.

Dantzig's work is often thought of as *applied mathematics*. He responds by saying, "I have never been able to tell the difference between the so-called pure and the nonpure and don't believe that there is any."

Dantzig was interviewed in his Stanford office in November of 1984.

What's this "Professor Dantzig" Stuff?

MP: Professor Dantzig, thanks for taking time to talk about yourself today.

Dantzig: What's this "Professor Dantzig" stuff?

MP: Okay, what should I call you?

Dantzig: Your name is Don, right Do you remember my first name?

MP: It's George.

Dantzig: Do you remember my middle name?

MP: It's Bernard.

Dantzig: What does George Bernard suggest?

MP: The well-known writer named George Bernard Shaw?

Dantzig: That's right. My father, Tobias Dantzig, was both a writer and a mathematician. He hoped that I would be a writer, and so he named me after George Bernard Shaw. He named my younger brother Henri Poincaré Dantzig after the great mathematician Henri Poincaré. My brother did, in fact, become a mathematician. He worked as an applied mathematician for the Bendix Corporation until he died in 1972 at age 54.

MP: I know your father's books, especially his wonderful *Number, The Language of Science*, but nothing much about him personally—except that he was born in Russia and studied in Paris with Poincaré.

Dantzig: Paris is a place where professors don't pay much attention to students so I can't say precisely what my father's relationship to Poincaré was. I do know that he attended his lectures, studied his works, and admired him greatly. One of his books is entitled *Henri Poincaré*.

MP: How did your father get to the United States?

Dantzig: He came to this country twice, once before he married my mother. On that first trip he visited his aunt in South Carolina whose family owned a general apparel business. He worked for them for a while as a peddler. He must have decided it had no future, for he returned to Paris. My mother, Anja Ourisson, had grown up in Poland and was studying mathematics at the Sorbonne at the time she met my father. After their marriage they emigrated to Oregon, where my father worked as a lumberjack. In those days, gangs of young men were hired to fell trees and nearly worked to death. Later he had a job as a road worker. On my birth certificate he is listed as a painter, probably a house painter. As a result of all the hard manual work, he developed huge arm muscles.

MP: How did he manage to get back into mathematics?

Dantzig: My father believed that he could never get a job in a university because of his heavy Russian accent; but one day at the public library he ran into Frank Griffin, the head of the mathematics department at Reed College, who told him he was crazy to be working as a lumberjack and road builder. Griffin assured him that with his academic credentials he could get a job in any university. That was a turning point in my father's career. He applied to Indiana University and was hired. He didn't have a formal Ph.D. at the time, but he soon acquired one while a professor there.

MP: Did he quit worrying about his accent after that?

Dantzig: I don't think he ever worried much about his accent. His spoken English was otherwise fluent, and he was known for his marvelous English writing style.

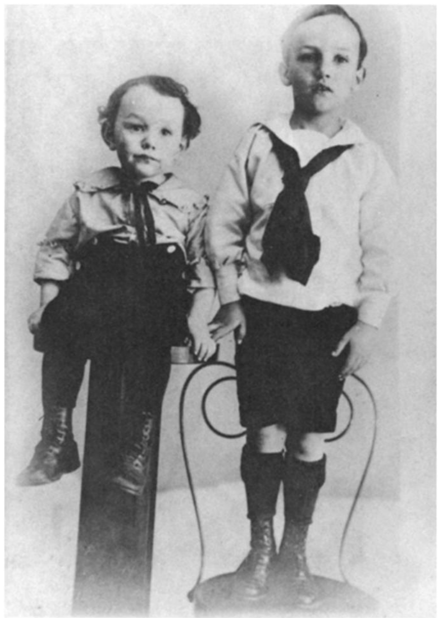
MP: He obviously was very important in your life. Can you tell me a little bit more about him?

Dantzig: He was a dynamic person with a very strong personality. He knew the classics and could quote them in Greek, Latin, and a dozen other languages. He was an excellent teacher and quite a raconteur. In the 1920's many of his friends were very interested in the philosophy of science. He used to hold salons in our home. From age eight until I was sixteen, I used to sit in the corner and listen to the smart-alec intellectuals of the 1920's expounding. I learned a lot but said little. After a while I began to suspect that they really didn't know what they were talking about. Perhaps this explains why to this day I can never get very excited about philosophical ideas.

Tobias Dantzig, father of George, as a young man. Born in Russia, he studied at the Sorbonne and later became a mathematics professor in the U.S.

Anja Ourisson, mother of George Dantzig. She studied mathematics at the Sorbonne and languages at Johns Hopkins University.

Baby George—age, one year.



The Dantzig brothers, Henry and George, in 1920.

Flunking Junior High School Algebra

MP: Can you remember when your own interest in mathematics was aroused?

Dantzig: Yes. I was in the ninth grade of Powell Junior High School in Washington, D.C. My father was teaching nearby at the University of Maryland. I was doing very poorly in my first course in algebra. To be precise, I was flunking. I remember walking home one day, furious with myself. How is it, I asked myself, that I, a son of a mathematician, do poorly while all the other kids in the class do so much better? I was very angry with myself. After that I sailed through algebra.

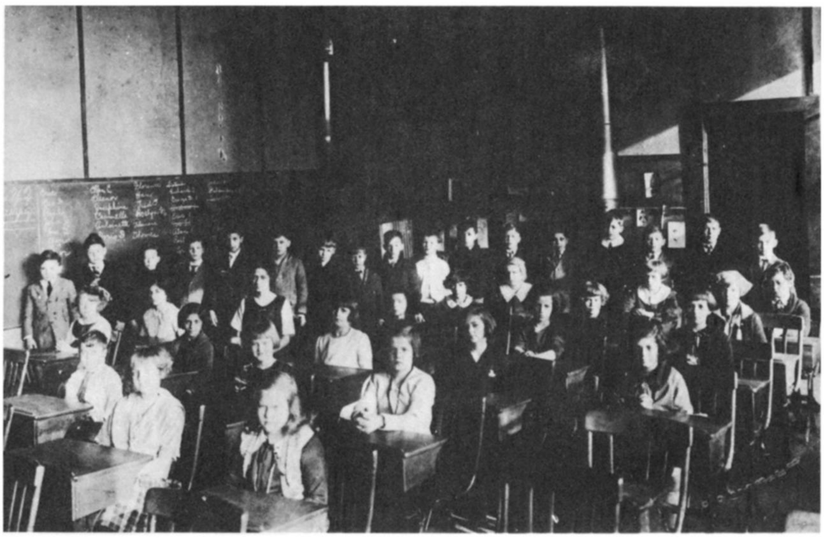
MP: It sounds as if you had a lot of self-confidence by grade nine.

Dantzig: Confidence came slowly. My interest in school work up to the seventh grade had been zero. I then began to blossom in science courses. Later on, after recovering from my poor start in algebra, I began to get top marks in mathematics. I was good in math and science in high school. I was on the chess team. I can't remember being interested in any other subjects.

MP: Do you remember any influential teachers?

Dantzig: Yes, especially Mr. Gilbert, my mathematics teacher at Central High School. I took geometry from him. Geometry really turned me on. Another important influence was Abe Seidenberg, who entered high school one year after I did. We did our math together and became good friends. He is now a professor at Berkeley.

MP: I studied from his book, *Lectures on Projective Geometry*.



The Big Prize—a photo of his classmates in the second grade. Dantzig (center of rear row) was awarded the photo for winning the long division contest. It was offered for sale but his family was too poor to buy it. The only way he could get it was to win the contest.

Ten Thousand Geometry Problems

Dantzig: Projective geometry was like mother's milk to me, to quote Eliza in Shaw's *Pygmalion*. I was brought up on it. My father taught me by giving me problems to solve. He gave me thousands of geometry problems while I was still in high school.

MP: Thousands?

Dantzig: I would say over ten thousand. After he gave me one and I came back with a solution, he would say, "Well, I'll give you another one." It seemed as if he had an infinite storehouse of them. At first he would check my solutions, but after a while he would accept them as correct and just give me another, and another, and another problem. The mental exercise required to solve them was the great gift from my father. Solving thousands of problems during my high school days—at the time when my brain was growing—did more than anything else to develop my analytical power.

MP: Just working those problems?

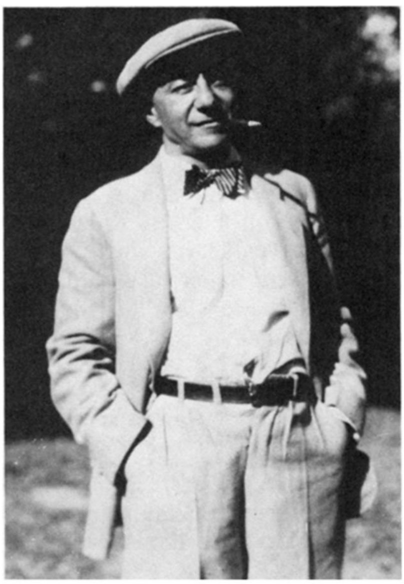
Dantzig: Yes, it was brain exercise. Problems on any other subject probably would have done as well.

MP: Did you ever feel that your father was pushing you into mathematics?

Dantzig: Never! It was I who asked for the problems. I believe he gave them to me just to get rid of me. It was almost as if he were saying, "Here's another problem. Now go away and don't bother me." He was always busy with whatever he was busy with—taking care of his students, writing, doing research, and so on. Eventually, of course, he did run out of problems and had to go to the Library of Congress to dig up additional ones.

MP: So you literally exhausted his supply of problems?

Dantzig: Eventually, yes. It seems that he didn't have an infinite supply after all.



Tobias Dantzig gave his son George a "great gift"—thousands of geometry problems to solve.

Secondhand Shoes

MP: You mentioned to me earlier that there wasn't much money when you were growing up.

Dantzig: We were always very poor. When my father taught at Johns Hopkins in 1919–20, we wore secondhand shoes. In the 1920's my mother obtained a master's degree in French from Johns Hopkins in order to qualify for a job at the Library of Congress. She was a linguist and a specialist in Slavic languages. Even with the income from two salaries, we were still poor. I don't remember ever having pocket money. There were hardly any jobs for kids. I did have a paper route once. My father never earned very much. When he retired as head of the math department at Maryland shortly after World War II, his pension was only $2,250 a year, which was half his yearly salary. This happened just before the postwar inflation. He moved to Los Angeles where he tried to pick up extra money teaching, consulting, and that sort of thing. Although his health was failing, he managed somehow. He never asked us, his children, for money. I don't know how he ever managed. Although he had been in Los Angeles only a short time when he died in 1957, hundreds of people came to his funeral. Nobody who ever met him forgot him. There was something magical about the way people were attracted to him.

"Applied Mathematics—I Didn't Seek it Out"

MP: By the time you finished high school was it clear to you that you would major in mathematics in college?

Dantzig: Yes.

MP: You said your father was teaching at the University of Maryland. So Maryland was a convenient place to go. Was that it?

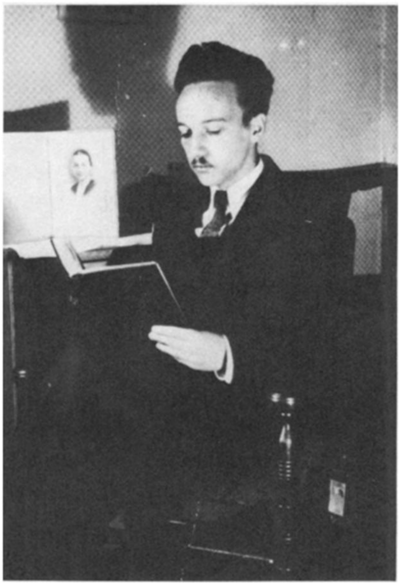
Dantzig: Yes, of course. It wasn't a time for high aspirations. I certainly had no dreams of going off to a fancy school which would require my family to support me away from home.

MP: Did your mathematics study have a strongly applied flavor at that time?

Dantzig: No, absolutely not. I don't recall a single application in any of the mathematics courses I took at Maryland. What math there was in physics and chemistry was pretty primitive. I did, however, encounter an interesting application of mathematics in a freshman chemistry course given by a Professor White. I wrote a little applied mathematics paper on how to efficiently extract iodine from a water solution using carbon tetrachloride as an extractor. He looked at it and said that it was a very interesting idea to subdivide the carbon-tet, but that he was sure someone must have already published the idea. Two years later, when I was a junior, he came around, very shamefaced, and showed me a paper just published on the same idea. That was the only thing I ever did at Maryland in the way of an application. I wasn't opposed to doing applied mathematics—it just never sought me out, and I didn't seek it out.

MP: After getting your bachelor's at Maryland in 1936, you went to Michigan for graduate study. Did you take any statistics at Michigan?

Dantzig: Mainly I studied under G. Y. Rainich, T. H. Hildebrandt, and R. L. Wilder. I did take a statistics course with H. C. Carver. In the summer of 1936 I married Anne Shmuner, and she came to Ann Arbor with me. We earned money



Dantzig as a graduate student in Michigan.

Newlyweds Anne and George Dantzig enjoying a picnic in Michigan in 1937.

working for Carver. He did some consulting on the side for a flour company. The company wanted to buy large quantities of wheat in the commodity market during the year at prices that would average out to the average annual price. Carver worked out a system of hedging that was supposed to be sensational. He swore me to secrecy; just the same, he was careful to camouflage the work sheets so that I would not discover his secret regression formula.

Statistics as taught by Carver seemed to me just a bag of tricks—it didn't have any rationale that I could discern. Everything else I took at Michigan was terribly abstract—so abstract that I had but one desire: to quit my graduate studies and get a job, which I did.

MP: So that's when you went to Washington, D.C.?

Dantzig: Yes. By luck—that was 1937, still the Depression—I got a job as a statistical clerk with the Bureau of Labor Statistics. The job I took, although at a lower civil service grade, was the one that had recently been vacated by Milton Friedman. I was assigned to a project called "Urban Study of Consumer Purchases" and asked to review a paper on double sampling by the famous statistician Jerzy Neyman, who was then at University College in London. It was my first encounter with statistical theory based on a logical rationale. I was very excited by the paper. Later I discovered it was the least representative of Neyman's contributions. I am sure that if I had seen anything more representative, I would have become even more excited.

MP: Did you like your work at the Bureau of Labor Statistics?

Dantzig: Yes, I learned a lot about practical applications. Our group was very good. My co-worker was Duane Evans. He and I became good friends. Later Evans' work in World War II on Wassily Leontief's input-output model of the U.S. economy changed the course of my career.

MP: So then how did you get from the Bureau to Berkeley? Like your father, you really moved around!

Dantzig: In retrospect I don't think I moved around too much. While at the Bureau, I wrote to Neyman, who was by then at Berkeley, and told him that I would like to finish my Ph.D. under him. At Berkeley in 1939, statistics was still part of the mathematics department, so the focus was on pure mathematics and not on statistics. The total number of courses in theoretical statistics that I was ever exposed to was two given by Neyman.

MP: What was Neyman like as a person?

Dantzig: Neyman had a dominating personality that he was able to assert long after he had been officially retired. In his seventies and eighties, he continued to run the statistical laboratory at Berkeley. No one dared to contradict him. He was top dog in every sense. I don't want to give the impression that he was a tyrant. He wasn't. He was very likeable—everyone respected him as the leading mathematical statistician in the world, quite correctly, I think.

Jerzy Neyman was "top dog in every sense" according to Dantzig.

How to Get a Ph.D.—Do Your Homework!

MP: How did it happen that you did your Ph.D. on a statistical topic when you took so few courses in statistics?

Dantzig: It happened because during my first year at Berkeley I arrived late one day at one of Neyman's classes. On the blackboard there were two problems that I assumed had been assigned for homework. I copied them down. A few days later I apologized to Neyman for taking so long to do the homework—the problems seemed to be a little harder to do than usual. I asked him if he still wanted it. He told me to throw it on his desk. I did so reluctantly because his desk was covered with such a heap of papers that I feared my homework would be lost there forever. About six weeks later, one Sunday morning about eight o'clock, Anne and I were awakened by someone banging on our front door. It was Neyman. He rushed in with papers in hand, all excited: "I've just written an introduction to one of your papers. Read it so I can send it out right away for publication." For a minute I had no idea what he was talking about. To make a long story short, the problems on the blackboard that I had solved thinking they were homework were in fact two famous unsolved problems in statistics. That was the first inkling I had that there was anything special about them.

MP: But you had apologized to Neyman for taking so long to do them.

Dantzig: Well, there was no particular deadline, and you know how graduate students take their time. A year later, when I began to worry about a thesis topic, Neyman just shrugged and told me to wrap the two problems in a binder and he would accept them as my thesis.

The second of the two problems, however, was not published until after World War II. It happened this way. Around 1950 I received a letter from Abraham Wald enclosing the final galley proofs of a paper of his about to go to press in the *Annals of Mathematical Statistics*. Someone had just pointed out to him that the main result in his paper was the same as the second "homework" problem solved in my thesis. I wrote back suggesting that we publish jointly. He simply inserted my name as coauthor into the galley proof.

Homework and Religion

MP: Is it true, as I have heard, that the story of your "homework problems" has been used by ministers in sermons?

Dantzig: Apparently so. The other day, as I was taking an early morning walk, I was hailed by Don Knuth as he rode by on his bicycle. He is a colleague at Stanford. He stopped and said, "Hey, George—I was visiting in Indiana recently and heard a sermon about you in church. Do you know that you are an influence on Christians of middle America?" I looked at him, amazed. "After the sermon," he went on, "the minister came over and asked me if I knew a George Dantzig at Stanford, because that was the name of the person his sermon was about."

MP: How did that happen?

Dantzig: The origin of that minister's sermon can be traced to another Lutheran minister, the Reverend Schuler of the Crystal Cathedral in Los Angeles. Several years ago he and I happened to have adjacent seats on an airplane. He told me his ideas about thinking positively, and I told him my story about the homework problems and my thesis. A few months later I received a letter from him asking permission to include my story in a book he was writing on the power of positive thinking. Schuler's published version was a bit garbled and exaggerated but essentially correct. The moral of his sermon was this: If I had known that the problems were not homework but were in fact two famous unsolved problems in statistics, I probably would not have thought positively, would have become discouraged, and would never have solved them.

World War II and Air Force Planning

MP: Did you finish your Ph.D. at Berkeley?

Dantzig: Not quite. I had completed my course work, and my thesis was settled in June 1941. But I had not defended my thesis or my minor thesis on dimension theory. This was six months before Pearl Harbor. Many of us wanted to contribute to World War II, which we believed the U.S. was about to enter. I went back to Washington during summer vacation and had an interview with Charles Bates "Tex" Thornton, who had been selected by Secretary Lovett to set up Air Force Statistical Control. The Air Force at that time did not have a good system for reporting the status of their aircraft. They didn't even know their total number of planes, which at the time was less than 100. The interview took place on the corner of 20th Street and Constitution Avenue. He wanted me to join him right away. My wife, Anne, who was with me at the interview, is very good at spotting talent. She told me Thornton was a man who was going places and I should take the job. She was right. As you know, after the war Tex founded Litton Industries.

MP: So it was your work with the Air Force that got you into this now famous subject, linear programming?

Dantzig: Not exactly right away. I stopped my graduate studies and joined the Air Force as a civilian. I was put in charge of the Combat Analysis Branch of Statistical Control. I set up a reporting system for combat units on the number of sorties flown, aircraft lost and damaged, bombs dropped, and targets attacked. I became quite expert at programming planning methods using the only "computing machines" we had then—people using hand-operated desk calculators.

My colleagues in the Pentagon included Brandon Barringer (a well-known Philadelphia banker), Robert McNamara (of World Bank fame), Edward Learned (of the Harvard Business School), and Warren Hirsch (the probabilist at New York University), who was my deputy.

The Challenge That Led to Linear Programming

In spring 1946 I returned to Berkeley and finished my Ph.D. I turned down an offer from Berkeley because it paid too little and returned to the Pentagon, where I became the mathematical adviser to the U.S. Air Force Comptroller. But I was really looking for an academic position. To entice me into remaining with the Air Force, two of my colleagues, Dal Hitchcock and Marshall Wood, challenged me to see what could be done to mechanize the planning process; that is, to find a more rapid way to compute a time-staged deployment, training, and logistical supply program. Mechanization in those days 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 being done at the Bureau of Labor Statistics by Duane Evans, Jerome Cornfield, and Marvin Hoffenberg on the input-output model of

Wassily Leontief. I had learned about it during the war in telephone conversations at night with Duane Evans—we were much too busy during the day to talk.

In my *Linear Programming and Extensions* you will notice that I pay great tribute to Leontief. It was Leontief who around 1932 first *formulated the Inter-industry Model of the American Economy*, *organized the collection of data* during the Great Depression, and finally *tried to convince policy makers to use the output* from the analysis. All of these things are necessary steps for successful applications, and Leontief took them all. That is why in my book he is a hero.

Leontief's model had a matrix structure which was simple enough in concept with sufficient detail that it could 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 his model there was a one-to-one correspondence between the production processes and the items produced by the processes. What was needed was a model with many alternative activities. Moreover, the application had to be large scale—with hundreds, perhaps thousands of activities and items. Finally, it had to be computable. In other words, once the model was formulated, there had to be a practical way to compute what quantities of these activities to engage in so as to be compatible with their input-output characteristics and 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, in other words no explicit goal. Such goals did not exist in any practical sense because planners simply had no way to implement them.

An Earth Filled with Computers

A simple example illustrates the fundamental difficulty of formulating a planning program using such an activity-analysis approach. Consider the problem of assigning 70 men to 70 jobs. An "activity" consists of assigning the ith man to the jth job. The restrictions are (a) that there are 70 men, each of whom must be assigned, and (b) that all of the jobs, also 70, must be filled. The level of an activity is either 1, meaning it will be used, or 0, meaning it will not. Thus there are 2×70, or 140, restrictions and 70×70, or 4900, activities with 4900 corresponding zero–one decision variables. Unfortunately there are also 70 factorial permutations, or ways to make the assignments. The problem is to compare these 70 factorial ways and to select the one which is optimal, or "best" by some criterion.

Now in this example 70 factorial is a very big number. To get some idea of how big, suppose we had had an IBM main-frame computer available at the time of the Big Bang fifteen million years ago. Would it—between then and now—have been able to examine all the possible solutions? No! But suppose that an even more powerful computer had been available, one that could have examined one billion assignments per second. The answer would still be no. Even if the Earth were filled with nanosecond-speed computers, all working in parallel, the answer would still be no. If, however, there were ten Earths, all filled with nanosecond-speed computers, all programmed in parallel from the time of the Big Bang until the sun grows cold, then perhaps the answer would be yes. The remarkable thing is that the simplex method with the aid of a modern computer can solve this problem in a split second.

This example illustrates why, up to 1947 and for the most part up 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 ways to go about doing the job, each with its advantages and disadvantages, that it has been impossible to compare them and to choose among them that one which is best. So, invariably, man has always had to turn to a leader whose "experience" and "mature judgment" would guide the way. The leader's guidance usually consisted in the issuance of a series of edicts or ground rules to those developing the programs. Although such methods are still widely used, the world today is far too complex for such simplistic methods to work, and they don't.

In late 1946, before we knew that high-speed electronic computers were soon going to exist, I had formulated a mathematical model that satisfactorily represented the technological relations usually encountered in practice. However, in place of any explicitly stated goal, or function to be extremized, there were a large number of *ad hoc* ground rules issued by those in authority to aid in the selection of the solution. Without these it would have been impossible to choose from the astronomical number of feasible solutions.

MP: That certainly has to be classed as a very messy real-world problem. Most mathematicians prefer problems which are cleanly formulated.

Dantzig: It is almost impossible for someone coming from a purely mathematical background with little exposure to applications to understand how to go about formulating a real-world problem in mathematical terms. There is a certain softness —a lack of precision— in the definition of many "dirty" real-world problems, which permits them to have many equivalent mathematical formulations. When I say "equivalent," I don't mean equivalent in the mathematical sense of one-to-one correspondence, but equivalent for the purpose of the application. From the point of view of the person looking for an answer, one definition of the problem may be just as satisfactory as another. But one definition may turn out to be completely amenable to mathematical analysis and solution while another may be mathematically hopeless. Only through detailed knowledge of the problem can one decide whether the more tractable formulation is just as acceptable. Linear programming models have been successful because with them many large problems can be formulated so that they are acceptable to planners and solvable on a computer.

The Young Father of Linear Programming

MP: You are often called the Father of Linear Programming. Is that a title you're comfortable with?

Dantzig: I have to tell you a story. Twenty-five years ago I visited Japan for the first time. When I got off the plane, the Japanese who met me were very surprised at how young I was. Since I had been billed as the Father of Linear Programming, they apparently expected to see an old man with white hair and a cane being helped down the ramp. I was 45 at the time.

MP: The preface to your book *Linear Programming and Extensions* opens with a provocative statement: "The final test of a theory is its capacity to solve the problems which originated it."

Dantzig: Did I say that? It's a great quote. Show me where.

MP: Your second paragraph isn't bad either: "This book is concerned with the theory and solution of linear inequality systems. On the surface, this field should be just as interesting to mathematicians as its special case, linear equation systems. Curiously enough, until 1947 linear inequality theory generated only a handful of

The Father of Linear Programming lecturing in Japan.

isolated papers, while linear equations and the related subjects of linear algebra and approximation theory had developed a vast literature. Perhaps this disproportionate interest in linear equation theory was motivated more than mathematicians care to admit by its use as an important tool in theories concerned with the understanding of the physical universe."

Dantzig: I think of modern mathematicians as a distinct race characterized by their non-interest in applications. From a historical point of view mathematicians before, let us say, 1820 were very closely tied to physics—or, in the case of probability theory, to gambling. For the past 150 years, however, mathematicians have created their own abstractions and followed the mathematical fads that happen to be in fashion. The fact that there is a whole world of exciting new mathematics out there in such fields as Operations Research, Computer Science, Optimization Theory has not excited their interest. I for one have no interest in trying to re-educate them—it would be a hopeless task. The most we can hope for is that they can be educated to the point that they don't prejudice gifted students too much against that wonderful world of mathematics that goes by different names.

MP: How do they bias students against applications?

Dantzig: By showing their contempt for anything which is not pure mathematics as they define it. Students are being brainwashed into thinking that pure mathematics is in some way purer than other forms of mathematics. I have never been able to tell the difference between the so-called pure and the nonpure and don't believe that there is any. Just because my mathematics has its origin in a real problem doesn't make it less interesting to me—just the other way around, I find it makes the puzzle I am working on all the more exciting. I get satisfaction out of knowing that I'm working on a relevant problem. I find that just as much mathematical ingenuity has to go into solving problems from a newly developing area as from some old so-called pure math area.

Computers are now being applied to almost every aspect of human activity. Every field of science, of medicine, engineering, business—you name it—is being computerized in some way. However, before you can put a problem into a computer and efficiently find a solution, you must abstract it. To abstract it, you have to build a model. Before you start to do anything with a model, you have to mathematize it. It is this process of abstracting applications from every aspect of life that has given rise to a vast new world of mathematics that is being developed outside mathematics departments. This mathematics is just as interesting and just as exciting and just as challenging as any mathematics that is taught in the regular courses. Most mathematicians remain completely unaware of it.

MP: One scenario I've heard a few times of late goes like this. Given the innate nature of mathematicians and given that there are all kinds of mathematics outside mathematics departments—important stuff, where the applicational value is often very apparent to students—we may see mathematics departments eventually take on a role similar to that of philosophy departments.

Dantzig: Possibly. However there will always be a few core courses that need to be taught: algebra, matrix theory, calculus, analysis, and some topology, so there will always be something for mathematics departments to do even if they continue to be indifferent to the developing new areas of mathematics.

MP: Do you feel that mathematicians are biased against the people who do operations research?

Dantzig: I myself have never experienced any bias. I believe I am well accepted as a mathematician. In addition, I've got the right "union card," a Ph.D. in mathematics from Berkeley.

Discovering the Linear Programming Model

MP: Can you remember your feelings when you first proposed the linear programming model?

Dantzig: To tell the truth, it was a very gradual awakening. When the planning problem was first formulated for the Air Force, the very notion of an objective function, the idea of a sharply defined goal, was nonexistent. Of course we paid lip service to the concept of a goal. In the military setting I often heard it said, "Our goal is to win the war." In a business setting one would hear, "Our goal is to make a profit." But you could never find any direct relationship between the stated goal and the actions to achieve the goal. If you looked closely at the next step, you would find that some leader in his conceit had promulgated a bunch of ground rules to guide the way to the goal. This is a far cry from honestly looking at all alternative combinations of actions across the board and picking the best combination. Those in charge often do a hand-wave and say, "I've considered all the alternatives," but this is so much garbage. They couldn't possibly look at all possible combinations. Before 1947 the possibility that there could be a tool like linear programming that would enable one to examine millions of combinations was inconceivable. There was no algorithm or computational tool for doing so.

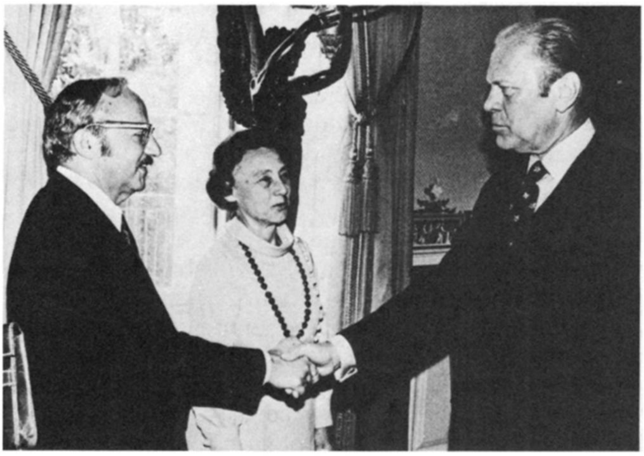
I didn't discover the linear programming model all in a flash. It evolved. About a whole year was spent deciding whether my model could be used to formulate practical scheduling problems. Planning and scheduling, as you know, were carried out on a vast scale during the war. Running the Air Force was the equivalent of running the economy of a whole nation. Hundreds of thousands of people were involved in the process. The logistics were on a scale that is impossible to convey to an outsider. My colleague Marshall Wood and I reviewed thousands of situations drawn from our wartime experience.

The ground rules used in planning were expressed in a completely different format from the way we now formulate a linear program. What we did was review these rules one by one and demonstrate that almost all could be reformulated acceptably in linear programming format. Not all. In some cases discreteness and nonconvexity also had to be taken into account.

When I first formulated my linear programming model, I did so without an objective function or goal. I struggled for a while with adding ground rules for selecting from the feasible solutions one that was in some sense "optimal." But I soon abandoned this approach and replaced it with an objective function to be maximized. The model I formulated was not specialized to the military. It could be applied to all kinds of planning problems—all one had to do was change the names of the columns and the rows, and it was applicable to an economic planning problem or to an industrial planning problem.

The general model was one which I assumed economists had looked at and for which they had developed solution techniques. Albert Kahn of the National Bureau of Standards suggested I visit T. J. Koopmans at the Cowles Foundation in Chicago. I did so in June 1947. Koopmans at first seemed indifferent to my presentation, but then he became very excited—it was as if, in a lightning flash, he suddenly saw its significance to economic theory. One reason why linear programming caught on so quickly outside the military can be traced back to the realization by Koopmans, in 1947, that a good part of economics could also be translated into the linear programming format. Incidentally, Koopmans became the leader of a brilliant group of economists who developed the theory of allocation of resources and its relation to linear programming. This culminated in 1975 when he received the Nobel Prize.

Koopmans, Dantzig, and Kantorovich. Koopmans and Kantorovich shared the Nobel Prize for economics in 1975. Dantzig's development of linear programming was fundamental to their work. Koopmans expressed regret that Dantzig was not named to share the honor.



Dantzig receiving the National Medal of Science from President Ford in 1975.

MP: Do you think that economists other than Koopmans would have spotted the significance of linear programing?

Dantzig: Hard to say. Economists had been developing economic models for over two hundred years without realizing its importance in spite of the fact that their field began with a linear model proposed by the Technocrats back at the time of the French Revolution. It was a rather poorly formulated input-output model of the Leontief type with various economic sectors consisting of the peasant, the artisan, and the nobility. But instead of developing the approach, economists over the next hundred years created more and more sophisticated nonlinear models. Walras's model, for example, was a very general nonlinear programming model. From the historical point of view, linear programming is an anachronism. It should have been the model that played a central role in economic thought from the beginning rather than emerging at a late date as a throwback. The anachronism came about because until very recently mathematical models were not being used by economists to obtain quantitative answers. They were used instead as a convenient substitute for long-winded logical verbal argument. Leontief was the first economist to break away from this classical use by constructing and solving a large scale quantitative model based on real data.

MP: How did the discovery of the simplex algorithm come about?

Dantzig: I am coming to that. I learned from Koopmans in early 1947 that the economists didn't have an algorithm, and that was bad news. The generals in the Air Force were paying us to solve real planning problems. By hook or crook, we were expected to find a practical way to solve them.

I set out in the summer of 1947 to invent one. I began by observing that the feasible region is a convex body—a polyhedral set. Therefore, we could improve by moving along edges from one extreme point to the next. But this procedure seemed hopelessly inefficient. In three dimensions, the region could be visualized as a diamond with faces, edges, and corner points. In the cases of many edges, the procedure might wander along improving edges for a long time before reaching the optimal corner point of the diamond.

MP: So that was your geometrical representation?

Dantzig: Yes. There was nothing novel in the procedure. Any mathematician would consider it as a possibility but would immediately discard it. So obviously I initially rejected the idea. I next looked at the problem using the geometry of the columns instead of the rows. Curiously, in the column geometry the algorithm I just described looked efficient. I found it extremely difficult to create a problem in m equations and n non-negative variables which I couldn't solve in m pivot steps; that is, in m moves along edges.

At first I thought that the method might be efficient but not necessarily practical. For a big problem there could be many combinations (corner points)—perhaps as many as the stars in the heavens. It might require a million steps to solve it. That might be considered efficient, since this number is small relative to the number of combinations involved, but hardly practical. So I continued to look for a better alternative algorithm.

That summer Koopmans sent one of his students, Leonid Hurwicz, to see me. Leo and I kicked around an idea we called "climbing up the beanpole," which was a precursor of the simplex method. It assumed the variables summed to unity. Later I generalized the procedure by getting rid of the convexity constraint. My branch at the Pentagon experimented with it. We looked around for some small examples to solve. One of them was a nutrition problem of George Stigler's. This problem became famous because it was the first practical problem to be solved by the simplex method.

Von Neumann to Dantzig: "Get to the Point."

That fall, while my group at the Bureau of Standards was experimenting with the simplex algorithm, I decided to consult with the "great" Johnny von Neumann and see what he could suggest in the way of solution techniques. He was considered by many to be the leading mathematician in the world. On October 3, 1947, I visited him for the first time at the Institute for Advanced Study. I began by explaining the formulation of the linear programming model in terms of activities and items and so forth. I described it to him as I would describe it to an ordinary mortal. He responded in a way which I believe was uncharacteristic of him. "Get to the point," he snapped. I said to myself, "Okay, if this man wants a quickie, then that's what he'll get." In less than a minute I slapped the geometric and the algebraic versions of my problem on the blackboard. He stood up and said, "Oh, that."

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 absolutely nothing), he said, "I don't want you to think that 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."

That was the way I learned for the first time about Farkas's lemma and the duality theorem.

On another visit to Princeton in June 1948 I met Albert Tucker. Soon Tucker and his students, Harold Kuhn and David Gale, began their historic work on game theory, nonlinear programming, and duality theory. Twelve years later Al Tucker, who had been reading the manuscript of my book *Linear Programming and Extensions*, asked me, "Why do you ascribe duality to von Neumann and not to my group?" I replied, "Because he was the first to show it to me." "That is strange," he said, "for we have found nothing in writing about what von Neumann has done." "True," I said, "but let me send you the paper I wrote as a result of my first meeting with von Neumann." I sent him the report I wrote for my Air Force branch, "A Theorem on Linear Inequalities," dated 5 January 1948, which contains (as far as I know) the first formal proof of duality. Later Tucker asked me, "Why didn't you publish it?" I replied, "Because it was not my result—it was von Neumann's. All I did was to write up, for internal circulation, my own proof of what von Neumann had outlined to me. 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.

MP: Von Neumann apparently made a strong impression on anyone he came in contact with.

Dantzig: Yes, people would come to him because of his great insight. In the initial stages of the development of a new field like linear programming, atomic physics, computers, or whatever, his advice proved invaluable. After these fields were developed in greater depth, however, it became increasingly more difficult for him to make the same spectacular contributions. I guess everyone has a finite capacity, and Johnny was no exception.

"The Simplex Algorithm Worked—I Could Stop Looking"

MP: So while you were off seeing von Neumann and trying to come up with a better algorithm, your group at the Air Force was experimenting with the simplex algorithm that you had given them? You weren't very optimistic about its usefulness?

Dantzig: That's right. As I said, I thought the method might be efficient but not practical so I continued to look for a better algorithm. About a year later, in June 1948, my group asked me why I continued to look elsewhere when the simplex algorithm was working out so well on the test problems.

MP: So it was completely unexpected?

Dantzig: Yes. Most of the time it solved problems with m equations in $2m$ or $3m$ steps— that was truly amazing. I certainly did not anticipate that it would turn out to be so terrific. I had had no experience at the time with problems in higher dimensions, and I didn't trust my geometrical intuition. For example, my intuition told me that the procedure would require too many steps wandering from one adjacent vertex to the next. In practice it takes few steps. In brief, one's intuition in higher dimensional space is not worth a damn! Only now, almost forty years from the time when the simplex method was first proposed, are people beginning to get some insight into why it works as well as it does.

MP: A fellow at Bell Labs, Narendar Karmakar, has been reported to have done something new with linear programming.

Dantzig: It is an important improvement on the theoretical result of Kachian that a linear program can be solved in polynomial time. Kachian's theorem states that the computational time is guaranteed to be less than a polynomial expression in the dimensions of the problem and the number of digits of input data. The bound is extremely high, hence not a practical result. We will just have to wait and see if interior algorithms, such as Karmakar's, will prove competitive in practice to the simplex method for general linear programs. I would not be surprised if it turns out to be an efficient way to solve problems with special structure such as multistage problems.

"Apparently Any Form of Government Can be Made to Work..."

MP: I notice that John D. Williams is one of the people whom you include in the dedication of your book on linear programming. How did he figure in your career?

Dantzig: In 1952 I left the Air Force to work for RAND. John was my boss. After I had worked for several months without receiving any direction, I went to see him. I said, "John, what is it that I'm supposed to do?" He didn't say a word, not one—he just sat looking at me from across his desk. Five minutes passed, and I began to get uneasy. Still not a word. Ten minutes passed. Finally he said, "George, you know better than to ask that question." I understood what he meant and got out of his office fast. John's policy was to let his researchers do their thing. For example, he tolerated me for nine years while I wrote my book. Of course, I also wrote a lot of papers during the same period.

Williams's organization chart for the Mathematics Division at RAND was horizontal. Nobody reported to anyone. The research output was remarkable and gave the RAND Corporation a worldwide reputation. My impression, however, is that its top administration never knew from whence the reputation came. Ray Fulkerson, Lloyd Shapley, Richard Bellman, Ted Harris, Selmer Johnson, Olaf Helmer, George Dantzig, to name but a few, were all producing papers like mad and doing so without any direction whatsoever. During that time, network flow theory was developed by Fulkerson, game theory by Shapley, dynamic programming by Bellman, and linear programming by Dantzig. It was a complete contrast to the group I had worked with in the Air Force. In the Pentagon everything was organized vertically. Orders came down from the top in military fashion. Even so, we were highly motivated and remarkably effective. I find it amusing that there could be these two very different ways to organize research—one anarchistic, the other dictatorial, and yet both highly efficient. Apparently any form of government can be made to work if the people are motivated enough.

MP: What caused you to leave RAND and return to the academic world?

Dantzig: My leaving had to do with the way we teamed up to do our research. In the beginning I was part of a team with Ray Fulkerson and Selmer Johnson. For a time we did great things together. Then after a while, although we remained good friends, each of us got busy doing his own thing. RAND at that time had about thirty in the Mathematics Division, which is a big group for doing just mathematical research. There were no new people being hired to work with us as disciples.

MP: So you need young minds up against the experience and expertise of the senior people?

Dantzig: Yes, there must be change, dynamics. My stimulus comes from students and working closely with researchers elsewhere.

The Simplex Algorithm

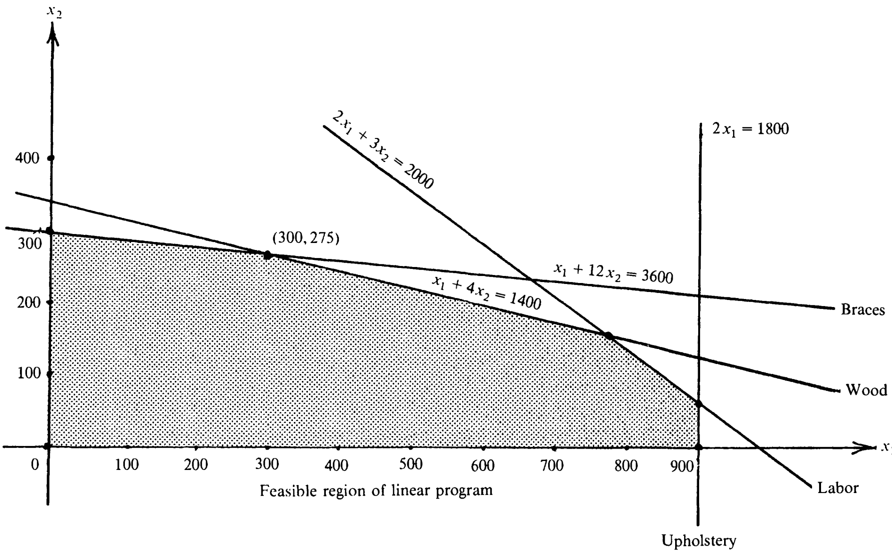
The Simplex Algorithm is the standard method of solving linear programs. A linear program seeks to maximize (or minimize) some linear functions, subject to a system of linear constraints.

Example of a Linear Program. A factory can manufacture chairs and tables. Let x_1 be the number of chairs produced and x_2 the number of tables. Chairs sell for \$40 apiece and tables for \$200 apiece. We seek to maximize our sales income, subject to limited supplies of raw materials. The inequalities below describe the production requirements and constraints on these materials:

Maximize $z = 40x_1 + 200x_2$ subject to $x_1 \geq 0,\ x_2 \geq 0$, and

$$\begin{array}{lrl} \text{Wood:} & x_1 + 4x_2 & \leq 1400 \\ \text{Labor:} & 2x_1 + 3x_2 & \leq 2000 \\ \text{Braces:} & x_1 + 12x_2 & \leq 3600 \\ \text{Upholstery:} & 2x_1 & \leq 1800 \end{array} \qquad (1)$$

The model (1) is called a linear program. The Figure below shows the *feasible region* (shaded area) of points (x, x_2) that satisfy the inequalities in (1). It is straightforward to show that a linear objective function assumes its maximum value at a corner of the feasible region (assuming the feasible region is bounded).



Feasible region of linear program

The Simplex Algorithm developed by George Dantzig starts at the origin (0,0), which is a corner of the feasible region, and moves along boundary edges from corner to corner increasing the objective function until a maximizing corner is reached.

The algorithm first introduces nonnegative *slack variables* x_3, x_4, x_5, x_6 to convert the constraint inequalities to equations:

Maximize $z = 40x_1 + 200x_2$ subject to $x_1 \geq 0,\ x_2 \geq 0,\ x_3 \geq 0,\ x_4 \geq 0,\ x_5 \geq 0,\ x_6 \geq 0$, and

$$\begin{array}{llllll} \text{Wood:} & x_1 + 4x_2 + x_3 & & & & = 1400 \\ \text{Labor:} & 2x_1 + 3x_2 & + x_4 & & & = 2000 \\ \text{Braces:} & x_1 + 12x_2 + & & + x_5 & & = 3600 \\ \text{Upholstery:} & 2x_1 & & & + x_6 & = 1800 \end{array} \qquad (2)$$

A vector $\mathbf{x} = (x_1, x_2, x_3, x_4, x_5, x_6)$ satisfying (2) is called *feasible*. It can be shown that a corner of the feasible region of (1) corresponds to a feasible vector $\mathbf{x}$ in which two of the six variables are zero. For example, the corner where the wood and braces constraints intersect corresponds to $x_3 = x_5 = 0$. We can recast (2) into matrix or tabular form:

Maximize $z = 40x_1 + 200x_2$ subject to $x_i \geq 0$ $(1 \leq i \leq 6)$ and

x_1	x_2	x_3	x_4	x_5	x_6	z	
1	4	1	0	0	0	0	1400
2	3	0	1	0	0	0	2000
1	12	0	0	1	0	0	3600
2	0	0	0	0	1	0	1800
−40	−200	0	0	0	0	1	0

(2′)

In linear algebra terminology, the columns for the slack variables are a basis for the column space; variables associated with the basis columns are called *basic*. Setting non-basic variables x_1 and x_2 equal to 0 in (2) gives us a feasible corner (the origin in (1)) because setting $x_1 = x_2 = 0$ in (2) makes the basic variables equal to the right-side values which are all positive.

The Simplex Algorithm repeatedly performs a change of basis to move to new a feasible corner (at which the non-basic variables are set equal to zero). The variable that enters the basis (and becomes positive) is the variable with the largest positive coefficient in the objective function. In (2), x_2 would enter the basis. Keeping $x_1 = 0$, we increase x_2 (which increases the objective function) until a basic variable is forced to zero. Since $x_5 = 0$ when $x_2 = 300$ in the braces equation, the variable x_5 leaves the basis. We can rewrite (2) in terms of the new basis by performing a Gauss-Jordan pivot on the coefficient 12 of x_2 in (2′). The new system is:

Maximize $z = (\frac{70}{3})x_1 - (\frac{50}{3})x_5 + 60000$ subject to $x_i \geq 0$ $(1 \leq i \leq 6)$ and

x_1	x_2	x_3	x_4	x_5	x_6	z	
$\frac{2}{3}$	0	1	0	$-\frac{1}{3}$	0	0	200
$\frac{7}{4}$	0	0	1	$-\frac{1}{4}$	0	0	1100
$\frac{1}{12}$	1	0	0	$-\frac{1}{12}$	0	0	300
2	0	0	0	0	1	0	1800
$-\frac{70}{3}$	0	0	0	$\frac{50}{3}$	0	1	60,000

(3)

Setting $x_1 = x_5 = 0$ yields a new feasible corner of (1), the corner where the braces constraint and the line $x_1 = 0$ intersect. From (3), we see that the objective function $z = (\frac{70}{3})x_1 - (\frac{50}{3})x_5 + 60{,}000$ is 60,000 at this corner.

The Simplex Algorithm continues with x_1 entering the basis (the coefficient of x_1 in the objective function is positive). Since the smallest ratio of rightmost column entry divided by x_1 entry occurs for row 1, we perform a Gauss-Jordan pivot on $\frac{2}{3}$. Hence, x_3 leaves the basis. This yields:

Maximize $z = -35x_3 - 5x_5 + 67{,}000$ subject to $x_i \geq 0$ $(1 \leq i \leq 6)$ and

x_1	x_2	x_3	x_4	x_5	x_6	z	
1	0	$\frac{3}{2}$	0	$-\frac{1}{2}$	0	0	300
0	0	$-\frac{21}{8}$	1	$\frac{5}{8}$	0	0	575
0	1	$-\frac{1}{8}$	0	$\frac{1}{8}$	0	0	275
0	0	3	0	1	1	0	1200
0	0	35	0	5	0	1	67,000

(4)

Since all coefficients in the current objective function $z = -35x_3 - 5x_5 + 67{,}000$ are negative, the corner point where $x_3 = x_5 = 0$ (the intersection of the wood and braces constraints in (1)) is the optimal corner. Therefore, the maximal value of the objective function is 67,000.

Alan Tucker, SUNY at Stony Brook

Politicians and Linear Programming

MP: Let's turn to another topic. In an article about you in the Stanford Campus Report, you say that policy makers often ignore powerful analytical tools. Are you optimistic about modeling playing a larger role in the future?

Dantzig: I keep hoping it will. I have been involved since 1975 in the development of a macroeconomic energy model called PILOT, which stands for Planning Investment Level Over Time. It contains a lot of detail bearing on energy, such as energy conservation, energy supply, industrial use, energy-saving devices in households, and so on. Recently we expanded the detail in the economy part of the model in order to estimate the impact of innovation, modernization, and foreign competition. It is a long-term model useful for analyzing trends forty years into the future, a tool that provides a tremendous insight into complex dynamic issues which face the nation.

In spite of the fact that the PILOT model is the real McCoy—a powerful tool for making policy decisions—decision makers do not line up to use PILOT or, for that matter, any other model. Decision making in a complex society is a haphazard, unstructured, and undisciplined process that doesn't lend itself to effective use of models. Policy makers, instead, look for quick answers to very complex questions—as a result the decisions they make are bad. Even when a decision maker is in a position to use the numbers produced by planning models as guidelines for action, he is reluctant to do so because the policies produced by models are never the whole answer. A model may help one to decide the best place to put a new airport, but then something unexpected always happens—like farmers or some other group, who have not been considered or even thought of when the model was formulated, coming forth with objections.

Politicians know the unexpected always happens so they tend to ignore the models and engage in an *ad hoc*, haphazard decision process instead.

MP: Why then have linear programming models been so successful for refinery scheduling?

Dantzig: A refinery is typically headed up by one person so he can use a model to determine what crude oil to buy and what to produce with it.

MP: But you are not optimistic about more complex applications?

Dantzig: No. Not at all. Many enterprises such as a nation are so complex that no one is really in charge. It is here that our models have the greatest potential for coming to grips with complexity and making a real contribution to the national well being. But this potential is frustrated by the lack of structure and discipline in the decision-making process itself.

The approach I favor for addressing this fundamental bottleneck to the effective application of models for national planning is to develop a disciplined structure for dialogue between all the special interest groups involved—one that will make use of a coordinating group whose job is to facilitate the bargaining process by supplying data from models on the feasibility and optimality of proposed compromises and trade-offs.

MP: Thank you very much, George. That's a good note to end on!