Nº50

# OPTIMA

JUNE 1996

MATHEMATICAL PROGRAMMING SOCIETY NEWSLETTER

*On October 27, 1995, I had the opportunity to interview Ralph Gomory in New York. Gomory has done fundamental work in integer programming. Among his contributions are the first algorithm for integer programming* [1], [2], *now known as Gomory's cutting planes, work with P.C. Gilmore on the traveling salesman problem and the cutting stock problem* [3], [4], [5], *the development of corner polyhedra [6],* [7], [8], *and his work with E.L. Johnson on group problems* [9]. *More recently he also developed, together with W.J. Baumol, an economic theory related to international trade* [10], [11], [12].

Gomory received his Ph.D. degree in 1954 from Princeton University. From 1954 to 1957 he served in the U.S. Navy, and in November 1957, he became a Lecturer in Mathematics at Princeton University. In 1959 he joined IBM where in 1970 he became Director of Research, a post that he held until 1989. He then became President of the Alfred P. Sloan Foundation, a non-profit institution supporting various research programs in science and technology through grants and fellowships.

During the interview I asked him about his previous and current scientific work, what it is like to be president of research of a huge company such as IBM, his view on industrial research, and the role of the mathematical programming community.

-KAREN AARDAL

*interview*

# RALPH E. GOMORY

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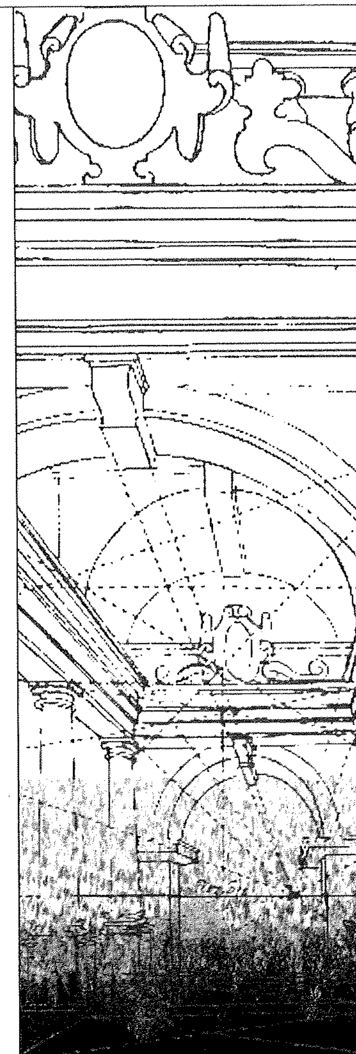

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OPTIMA

*interview*

# Ralph E. Gomory

OPTIMA: ***You were in the navy, physics branch. After a while you spent some time in the OR Department. What triggered your interest in OR at that time?***

**RG:** I have always been very interested in what people refer to as the "applications of mathematics," but I never thought about it in that way. I primarily got interested in mathematics as a way to understand how things work rather than as a "thing" in itself. The OR people were just 40 yards down the hall, and I knew that they were working on practical systems. At that time that meant weapons systems, and that was the sort of things that I had in mind to do myself, so I wanted to learn how it was done. I was not actually planning on doing weapons systems, but I thought that when I was getting out of the navy then I might do OR as a career. That struck me as about right, given my interest.

OPTIMA: ***At that time you started to think about integer programming. In particular you developed "the fractional cut method" as you called it yourself. Was an algorithm for integer programming a big open question at that time?***

**RG:** No, I did not even know that there was such a question. By that time I had gotten out of the navy and I was at Princeton, but the navy people had asked me to stay on as a consultant. So, every few weeks I would go down there and consult with them on problems. One of these problems was actually an integer programming problem, but it was formulated as a linear programming problem, and the variables came out noninteger. This was very awkward since they represented the number of aircraft carriers. The answer 2.2 would not actually be so bad, but 0.6 is awful because you then have to ask if you need any aircraft carriers at all. As soon as I saw that I thought that in solving integer linear *equations* there is a diophantine way, so probably in solving integer linear *inequalities* there should be a diophantine way as well. Of course I was wrong, but that is how I started my work. We needed the answer. Of course we could round it pretty well, but it is not the same.

OPTIMA: ***It is interesting to see today that both you, and Dantzig, Fulkerson and Johnson in their work on the traveling salesman problem* [13]*, used the Simplex method as the basis for an integer programming algorithm.***

**RG:** That was a method we were all using at that time. It was a very natural choice. If you are using the Simplex method and you have any contact with practical situations, you are likely to run into situations where you want integral answers. Dantzig et al., ran into the traveling salesman problem, and I ran into the navy problem. The navy was already using the Simplex method to solve a lot of problems, so it was very natural to ask whether or not you could adapt the method such that it produced integer answers. Actually, things are often much more straightforward than they appear!

OPTIMA: ***You also developed another algorithm for integer programming, "the all-integer method"* [14]*. You commented once that it was computationally inferior to the "fractional cut method." Why was that?***

**RG:** At that time it was inferior, but at some point I would like to get back to it and take a look to improve my understanding of it. I wrote all my programs myself at that time, and the computers then were very primitive. If you did the computations using the fractional cut method, what you saw was (assuming you started out with a problem with integer coefficients) that the constraint matrix first stopped being integer. The determinant of the matrix appears in the denominator when inverting the matrix, and this determinant could be very big. However, once you start to use fractional cuts, there is a tendency for the determinant to become small again, because if you pivot on a fraction, then the new determinant is the old determinant times the fraction, so it gets smaller. So, very often it happened that if you did a sequence of pivots, then the matrix would again become integral, simply by reducing the determinant, through a series of fractional cuts, down to one again. So everything turned "magically" back to integers. So, of course, this suggested to me that, "We want integers answers, and we start with integer coefficients. Why are we going around through all these non-integers?" So I looked around and tried to come up with a method in which it was possible to do a Simplex series of steps but in which everything would remain integer. If you think about it a little bit, this means that the pivot element always needs to be equal to one. So, the problem was how to arrange that. To my surprise I did solve it, and I was really thrilled! I thought, "Wow, this is a dream come true; this is like diophantine equations, which I originally thought to use here. We can work in the domain of integers and get integer answers. This is wonderful! I know it is going to be very good computationally!" But it was not... The reason it did not work was that the integers you got became absurdly large. So, it just did not perform. I am not sure this has to be the case, but I did not know anything about numerical analysis. None of us knew what was happening at all in these integer programming problems. One of the things I want to do eventually is to go back and look at this using modern computers. It would be so easy to make three-dimensional pictures and see what actually happens. The fact that the method was not computationally successful then was a great source of regret to me because I always felt that, inherently, it really ought to be like this. But sometimes those feelings can be wrong.

OPTIMA: ***You have also mentioned that you find it a bit sad that corner polyhedra have not been more explored for use in computations such as, for instance, to generate a natural sequence of test problems. What do you mean exactly by such test problems?***

**RG:** I certainly do not mean test problems for big programs but test problems for understanding what is happening. What is the simplest integer programming problem really? I assert, but this is just an intuitive assertion, that the simplest integer programming problem is defined by a cone. But, then you still have a lot of cones in the world that you can make! So, how would you order all possible cones in order of "difficulty"? A corner polyhedron is basically the convex hull of the lattice points inside a cone, but these polyhedra are naturally arranged in an order of increasing complexity, because to each such cone (I am leaving out one step here, so what I say is only "approximately" true) there corresponds an Abelian group. The size of the group is given by the determinant of the constraint coefficients. So, if we were very lucky and the determinant was one, everything would come out in integers automatically. But if the determinant was equal to 10, then the group would have 10 elements in it, including zero. So, you can arrange, so to speak, all cones in an ascending sequence of "complexity," first, the ones corresponding to groups with two elements, then the ones with three elements and so on. To each of these corresponds a single polyhedron. As you get more and more elements, the polyhedra get more and more complicated very rapidly. To me this suggests that there is a very useful thing to be understood here, which is: I have got this very simple cone at all times, but if I were doing integer programming, the "thing" I have to explore inside the cone rapidly gets more complex. In some sense, corner polyhedra form the natural link between linear programming and integer programming in the simplest possible form.

OPTIMA: *What happened with that line of research?*

**RG:** I really do not know because it was the last thing I did before I became the Director of Research at IBM. This particular piece of work suffered from the disadvantage of being harder than most things were then in integer programming. It was kind of an empirical subject at that time, and the theory of corner polyhedra was not. The papers were hard to read, and it probably was hard to see the point of it. Every now and then I do, however, run into people who are aware of this work.

As far as computing is concerned, to the extent that you would like to use cutting planes, these cutting planes can all be obtained using corner polyhedra. Suppose you are solving a linear programming problem, and you come to some vertex, and you would like to have all cutting planes that would reduce it, and suppose you "magically" had a list of the corner polyhedra for that vertex. It has got all the cutting planes that are the faces you need to give you an integer body. Clearly, most of the work would have to be done because, first of all, you cannot generate all those polyhedra to begin with, i.e., you cannot have such a list as I was referring to, but a very promising line of research in my opinion would be to start to look at this sequence of polyhedra and then simplify them in a way that makes them valid inequalities. It is a lot to be examined! For example, which of the facets of a certain polyhedron matter? It is quite possible that a reason the number of facets may be tremendous is that it has a few "big" ones in front and then, in addition, it has all kinds of "fancy stuff." I do not know if this is true or false.

OPTIMA: *That would agree with the experience from the study of the traveling salesman polytope done by Alan Hoffman and Harold Kuhn, who simulated that someone was sitting at the center of gravity of the traveling salesman polytope with a pistol firing at random. The result was that only the trivial facets $x_e=0$ were hit* **(see [15], page 118).**

**RG:** Yes, exactly. You see, if you had to make a guess, you would say that this is the way it probably is. With modern methods you could, in my opinion, just look at a few classes of facets and start to see whether they matter. If only a few mattered, then you would try and develop ways to get those few. I think the corner polyhedra are the simplest possible problems arranged in order of difficulty, and I think they would be very interesting to explore. If no one else does and if I am through with what I am now doing, then I will!

OPTIMA: *At the end of your 1966 paper on the traveling salesman problem* [15], *there is a discussion. One thing that surprised me with this discussion was the awareness of the existence of "difficult" and "easy" problems. In particular, Jack Edmonds posed the question of how problems can be characterized such that they get more tractable. The atmosphere seemed very optimistic, from what I could read out of the discussion, in the sense that people were fairly convinced that eventually such nice characterizations would be available.*

**RG:** I think it was the way people felt, and I guess that I still feel that way in spite of the complexity theory, at least from a "practical" viewpoint. Clearly, you cannot solve all problems in a systematic way, but you never have to. You just have to solve some class of problems that you are interested in, and very often you do not have to solve problems exactly. There may be some practical problems that are terribly difficult, and where we want the exact answer, but I think they form a tiny minority. So, depending on what you are trying to do, you should be very pessimistic or very optimistic. Even if we cannot compute certain things, there is very often a saving grace. One obvious example is to determine where the ball will end up in a roulette wheel. You cannot compute this well enough to tell between which pins the ball will end up, so this problem is, for all practical purposes, unsolvable. In this case, however, there is a related solvable problem, namely a "statistical" version. Typically, if you cannot solve a problem, there is a related class of problems that you can solve. Maybe being unable to solve a problem means that it is too delicate, so now we can employ a different technique that we, in the case of the roulette wheel, happen to call probability. Perhaps because I did not live through this evolution of all things you cannot do, I remain extremely optimistic!

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*interview*

# Ralph E. Gomory

OPTIMA: ***How did you get involved in your present work in economics*** ([10], [11], [12])***?***

**RG:** When I was Director of Research at IBM I used to travel a lot. In particular, I often went to Japan, which at that time was developing very rapidly. I was impressed by the rapidity with which they learned to do things, and they did things extremely well. People sometimes credit the Japanese government for being a major force behind this, but in my opinion mostly it was terrific work by the companies and the people. They just did things better. One question at that time was whether this was a good or a bad thing for the United States. It became clear at a certain point that the U.S. was losing its semiconductor industry to the Japanese competition, and it was a lot of fuss in the U.S. about that. I was on a government committee that was asked to look into the question whether something should be done. To help an industry like that is actually very "un-American." I went to a meeting with this committee in which there were some CEOs of semiconductor companies and some economists. The economists basically said, "Look, it does not matter if you lose this industry because something else will make up for it." There is an economic theory along those lines, and I now believe that what they said was a misinterpretation of that theory. The CEOs basically said that once you get out of this business, there is no way to get back in. So, the two groups talked right past each other. I was familiar with the economic theory to some extent, just from my general education, and I certainly knew what the CEOs were talking about because I understood something about that industry, and I said to myself, "Oh, boy, there is a real disconnect here." I was two years away from retirement at that time, so I thought, "One thing I am going to do is to look at this problem," because I was convinced that both parties had something to say, but both were wrong. As a matter of fact, when I left IBM and started my present job, I developed hepatitis, and Herb Scarf came to see me. He is a very good friend of mine since the time when we were both at Princeton. He has an interest in economies of scale, and I felt that economies of scale was the key to this situation. So, we decided to take a look at it, and Herb taught me about economic equilibria, and then I was able to go on from there and develop the outlines of a theory of international trade based on economies of scale. It turned out that you, miraculously, had to do a little integer programming to understand this because economies of scale has that quality - you either have a plant or you do not. Of course, this gave very different results compared to the ordinary theory, and it really said that if another country gets better and better it could harm you. Since then, and also in collaboration with Will Baumol, we developed a theory which does not require economies of scale. It is really the old theory all over again, but it allows for the fact, which people had not worked on, that capabilities of a country really do change over time, and then answers the following question: Suppose you are country one and you are dealing with country two, and country two is rather undeveloped, but then it starts to develop. Is that a good or a bad thing from your "selfish" point of view as country one? The answer is quite interesting, which is, in the beginning it is better for both countries that country two develops because they do better as they develop more goods, and you benefit because their labor is so cheap. But, after they developed to about a point where their real wage is about a third of yours, then their further development is negative for country one. So, we developed this theory, and I think it is going to have a lot of effect.

OPTIMA: ***What was it like to become the Director of Research for such a large and relatively diverse company as IBM?***

**RG:** You have the very important challenge of making the things that your people do, and we had really wonderful people, useful to IBM. We had a research group varying in size between two and three thousand people doing very different things. The problem was, how can you relate what they are doing and make it useful to IBM? It was clear to me from the very beginning that if something did not turn out to be useful to IBM, it would not survive. There were some people who felt that IBM would just pay for science, but I never believed that for a second.

OPTIMA: ***Nowadays you read much about how various industrial research labs have serious problems. What is your view on the future of industrial research?***

**RG:** I think that industrial research, if properly done, can be very productive. And I think that the Bell Labs experience and the IBM experience both show that. Within IBM, research has been cut less. IBM suffered a great deal; all parts of the company were cut down, but research perhaps the least. So I do not think that IBM has walked away from research. But, my general belief is that a mixture of theory and exposure to practice is good for both, and therefore I would say that the "case" for industrial research is that it is the milieu in which that can take place. If it is a milieu in which this cannot take place, then why bother? If it is just another group of people simply doing research out of contact with applications, you are not taking advantage of practical problems, or if it is just making the next product, you are not taking advantage of the possibilities that science gives you. So, I think these research labs can be very useful if they can manage, which is not easy.

OPTIMA: ***Can we afford it?***

**RG:** Sure we can!

OPTIMA: ***A controversial question related to the previous one is whether research should be done at all universities, or if we need a partition into research and teaching universities?***

**RG:** Why not? This is not a subject that I know a great deal about, but there is an interesting related question, namely, how much research is "enough"? Researchers think it should always be more, but it cannot be. So, there is a question of what is the correct level. I am currently quite active in pursuing that

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question [16], but I tend to agree with you. If we set the right level not every university will be doing research.

For instance, the place where I went to college was a very good college, but it was not a university, there was only a little research. Faculty did research in their spare time, but they were often wonderful teachers. And it worked fine. A lot of scientists came out of schools like that, because they got to know the professors, and they got to see how they thought about things, and it was a good atmosphere. I certainly do not believe that the only people that can teach are people who develop themselves very heavily in research.

OPTIMA: ***How does the mathematical programming community look to you at present? Do you think that we consider a good "mix" of theoretical and practical questions?***

**RG:** I will not comment on that simply for lack of knowing. My only exposure to the community lately was the International Symposium in Ann Arbor in 1994, which I enjoyed very much. There I saw a mixture of things. Rather than commenting on the community, it might be more sensible for me to say what I think is good. It is also quite possible that my prejudices are out of date, because this has not been my primary focus for a long time.

It happens in many subjects that they develop an abstract wing and an applied wing, as for example, computer science. There is quite a gap between people who develop the theory of algorithms, and the people who develop software systems. It is very hard to get the theory to deal with such a tangled mess. This is, in my opinion, the real distinction between engineering and science. In engineering you have to make the things work, even if you do not understand them. When you make disk drives for instance, a lot of that is just: "We will do what we did last year, and see if we can make these layers a bit thinner and get the bits packed a little closer". You could only do calculations about little pieces of this complex problem, but the whole thing is too complicated. So, in real life you almost always deal with things that are too complicated to scientifically analyze. The scientists deal more with what is understood. My view is that all these things are really worth doing, including the most theoretical, and the most applied, and it is too bad that they tend to look down on each other, because the people who do the theoretical work every now and then come up with something very helpful. Every year you can compute and understand a little more than last year, so in the long run they will be helpful to the people with the applied problems. The applied people are going to approach their problems somehow, and they keep running into things they cannot do, which form a fantastic source of interest and stimulation for the more theoretical people to work on. So, this whole chain is what I like to see in a society, and I do not know to which extent the mathematical programming society resembles the picture I have just sketched.

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# Conference Notes

- IFORS 96 14th Triennial Conference, Vancouver British Columbia, Canada July 8-12, 1996
- IRREGULAR 96 Santa Barbara, California Aug. 19-23, 1996
- International Conference on Nonlinear Programming Beijing, China Sept. 2-5, 1996
- Symposium on Operations Research (SOR96) Technical University Braunschweig, Germany Sept. 4-6, 1996
- Second International Symposium on Operations Research and its Applications (ISORA '96) Guilin, China Dec. 11-13, 1996
- Optimal Control: theory, algorithms, and applications Center for Applied Optimization University of Florida Feb. 27 - March 1, 1997
- XVI International Symposium on Mathematical Programming, Lausanne Switzerland, Aug. 1997

forthcoming conferences

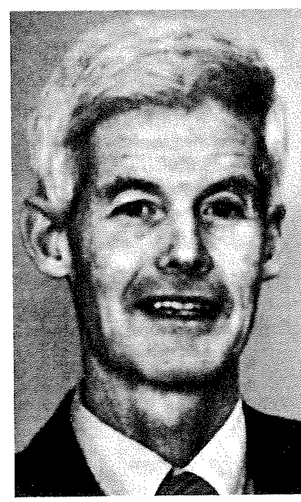
## Optimal Control: Theory, Algorithms, and Applications

**Center for Applied Optimization**
**University of Florida**
**Feb. 27-March 1, 1997**

The conference is to be held at the Center for Applied Optimization, University of Florida, Gainesville, FL. This meeting will provide a unique opportunity for researchers working on theory, algorithms, and applications of optimal control to exchange recent research advances, to establish a foundation for joint research cooperation, and to stimulate future research. Publication of a conference proceedings is planned. For more information, contact Bill Hager: hager@math.ufl.edu.

## *Report on the DIMACS Workshop on the Satisfiability (SAT) Problem*

**March 11-13, 1996**
**Rutgers University**



This conference was held at the NSF National Center for Discrete Mathematics and Theoretical Computer Science (DIMACS), Rutgers University. The workshop was organized by Ding-Zhu Du, Jun Gu, and Panos Pardalos. The advisory committee members included Bob Johnson, David Johnson, Christos Papadimitriou, Paul Purdom, and Benjamin Wah. David Johnson and Moshe Vardi, Chairman of the Special Year Committee, provided suggestions and comments on the early planning of the workshop.

More than 65 researchers from universities, governmental agencies, and industrial companies from 10 countries around the world attended the workshop which began with a welcome by DIMACS director, Fred Roberts. Following an introduction by David Johnson, Professor Steve Cook (pictured), who was the 1982 Turing Award winner, then delivered a distinguished lecture. A total of 34 technical talks were presented. The major topics covered included practical and industrial SAT problems and benchmarks, significant case studies and practical applications of SAT problems and SAT algorithms, new algorithms and improved techniques for satisfiability testing, specific data structures and implementation details of the SAT algorithms, and the theoretical study of the SAT problem and SAT algorithms.

The satisfiability problem is central in mathematical logic, computing theory, and many industrial application problems. There has been a strong relationship between the theory, the algorithms, and the applications of the SAT problem. This workshop brought together a group of distinguished theorists, algorithmists, and practitioners working on the SAT problem and on its industrial applications, which enhanced the interaction between the three research groups. As an important activity of the workshop, a set of SAT problem benchmarks derived from the practical industrial engineering applications has been provided for SAT algorithm benchmarking. Overall, the workshop was a great success. Proceedings of the workshop will be published later this year by the American Mathematical Society in the DIMACS Series.

-PANOS PARDALOS

OPTIMA

# Beale-Orchard-Hays PRIZE

**Call for Nominations:**

Nominations are being sought for the Mathematical Programming Society Beale-Orchard-Hays Prize for Excellence in Computational Mathematical Programming.

**Purpose:**

This award is dedicated to the memory of Martin Beale and William Orchard-Hays, pioneers in computational mathematical programming. To be eligible, a paper or a book must meet the following requirements:

1) It must be on computational mathematical programming. The topics to be considered include:
   a) experimental evaluations of one or more mathematical algorithms,
   b) the development of quality mathematical programming software (i.e. well-documented code capable of obtaining solutions to some important class of MP problems) coupled with documentation of the applications of the software to this class of problems (note: the award would be presented for the paper which describes this work and not for the software itself),
   c) the development of a new computational method that improves the state-of-the art in computer implementations of MP algorithms coupled with documentation of the experiment which showed the improvement, or
   d) the development of new methods for empirical testing of mathematical programming techniques (e.g., development of a new design for computational experiments, identification of new performance measures, methods for reducing the cost of empirical testing).
2) It must have appeared in the open literature.
3) If the paper or book is written in a language other than English, then an English translation must also be included.
4) Papers eligible for the 1997 award must have been published within the years 1993 through 1996.

These requirements are intended as guidelines to the screening committee but are not to be viewed as binding when work of exceptional merit comes close to satisfying them.

**Frequency and Amount of the Award:**

The prize is awarded every three years. The 1997 prize of $1500 and a plaque will be presented in August 1997, at the Swiss Federal Institute of Technology (EPFL), Lausanne Switzerland, at the Awards Session of the International Symposium on Mathematical Programming sponsored by the Mathematical Programming Society.

**Judgement criteria:**

Nominations will be judged on the following criteria:

1) Magnitude of the contribution to the advancement of computational and experimental mathematical programming.
2) Originality of ideas and methods.
3) Clarity and excellence of exposition.

**Nominations:**

Nominations must be in writing and include the title(s) of the paper(s) or book, the author(s), the place and date of publication and four copies of the material. Supporting justification and any supplementary materials are welcome but not mandatory. The awards committee reserves the right to request further supporting materials from the nominees.

**Nominations should be mailed to:**

Professor Robert J. Vanderbei
Dept. of Civ. Eng. and Operations Research
ACE-42 Engineering Quad
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Tel: 609-258-0876
Fax: 609-258-3796

rvdb@princeton.edu http://www.sor.princeton.edu/~rvdb/

**The deadline for submission of nominations is January 1, 1997.**

This call-for-nomination can be viewed online by visiting:

http://www.sor.princeton.edu/~rvdb/BOH97.html

# Stephen M. Robinson Receives Honorary Doctorate

The Faculty of Economics of the Unversity of Zurich awarded the Honorary Doctor's degree to **Stephen M. Robinson, University of Wisconsin-Madison** in recognition of his fundamental contributions to the theory of nonlinear optimization, in particular to the stability behaviour of optimization problems, and to the development of efficient and robust solution methods for nonlinear and stochastic optimization problems, as well as in appreciation of his permanent efforts for international cooperation in the OR community.

-PETER KALL

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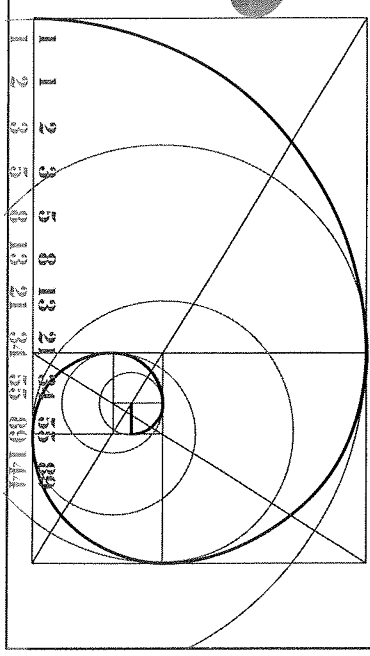
# JOURNALS



## Volume 72, Number 1

M. Fukushima, *The primal Douglas–Rachford splitting algorithm for a class of monotone mappings with application to the traffic equilibrium problem.*

W. Li, *A conjugate gradient method for the unconstrained minimization of strictly convex quadratic splines.*

M.S. Gowda, *On the extended linear complementarity problem.*

A. Ben-Tal and M. Teboulle, *Hidden convexity in some nonconvex quadratically constrained quadratic programming.*

R.D.C. Monteiro and S. Mehrotra, *A general parametric analysis approach and its implication to sensitivity analysis in interior point methods.*

M. Göthe-Lundgren, K. Jörnsten and P. Värbrand, *On the nucleolus of the basic vehicle routing game.*

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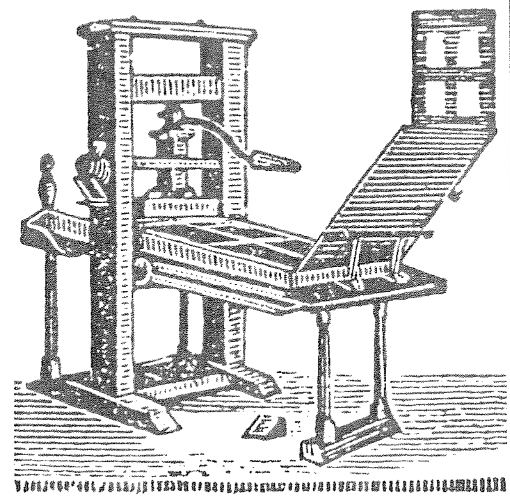

## *The Traveling Salesman, Computational Solutions for TSP Applications By G. Reinelt*

Lecture Notes in Computer Science 840 Springer-Verlag, Berlin, 1995

ISBN 3-540-58334-3

According to the author, "The aim of this monograph is to give a comprehensive survey on heuristic approaches to solving the traveling salesman problem and to motivate the development and implementation of possibly better algorithms."

The book begins with some chapters on basic material, treating basic concepts, including such geometric concepts as Voronoi Diagrams, Delaunay Triangulations and Convex Hulls. Although not intended as a text, I think these chapters make the book very usable in a course for graduate students.

The core of the book is given in chapters such as "Construction Heuristics," "Heuristics for Large Geometric Problems," and "Improving Solutions." Many computational results obtained by the author illustrate how the considered and analyzed heuristics are characterized as specific to the TSP.

In the chapter, "Further Heuristic Approaches," some non-specific algorithms are briefly described, without further computational results. A chapter, "Lower Bounds," gives results for a large number of problem instances and bounds. The last chapters give a case study of a production process and treat some issues on practical TSP solving.

The book is completed by an index and seven pages of references, indicating the pages where the references are used, thereby adding to the value of the book. The layout, both the text and tables with results, is straightforward and adds to the readability. Typing errors are very rare.

This book can be seen as an up-to-date and valuable extension of some chapters of the classical book on the TSP by Lawler et al. Without any doubt, I can recommend this book, both for practitioners as well as researchers, since it gives a concise and crisp overview of the heuristical approach to solving the TSP.

**Reference** Lawler, E.L., J.K. Lenstra, A.H.G. Rinnooy Kan and D.B. Shmoys (Eds.), *The Traveling Salesman Problem*, Wiley, Chichester, 1985.

-TON VOLGENANT

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## *Global Optimization in Engineering Design*

Nonconvex Optimization and Its Applications, Vol. 9
Edited by Ignasio E. Grossmann
Kluwer Academic Publishers, Dordrecht, 1996

ISBN 0-7923-3881-2

Global Optimization is concerned with the problem of computing globally optimal solutions of nonlinear functions. Although the application of local optimization methods is very popular in the field of engineering, the use of these techniques becomes inadequate in certain problem cases where the local solution either produces a significant cost penalty or results in an incorrect solution to a physical problem. Therefore, the need for global scope optimization methods becomes critical in the context of engineering design.

The chapters of this volume describe some of the most recent developments of deterministic approaches in global optimization and their use in engineering applications.

The first two chapters by Epperly and Swaney, present an *LP*-based branch and bound algorithm for finding the global solution of nonlinear programs in factorable form, useful for a variety of engineering applications such as phase and chemical equilibrium problems and flowsheet optimization problems.

The next two chapters, by Visweswaran and Floudas, describe the improvement of an original cutting plane algorithm GOP designed by the same authors, now presented in a branch and bound framework. Additionally, an implementation of the new GOP algorithm and computational results related to various problems in chemical engineering design and control and mathematical programming are presented in detail.

Chapter five describes the implementation of interval analysis optimization methods for solving global optimization problems, with emphasis on issues related to process design. Chapter six introduces an algorithm based on interval analysis that employs procedures for search acceleration and fast elimination of infeasible search spaces. The algorithm is applied for solving nonconvex mixed integer nonlinear programs.

The last six chapters emphasize applications in engineering design, such as planning of process networks, stochastic planning and scheduling models, heat exchanger networks, layout design, design of truss structures, batch design, water distribution systems, and general process models.

### Chapter titles and authors are listed below:

Epperly, T.G.W. and R.E. Swaney, "Branch and Bound for Global NLP: New Bounding LP"

Epperly, T.G.W. and R.E. Swaney, "Branch and Bound for Global NLP: Iterative LP Algorithm and Results"

Visweswaran, V. and C.A. Floudas, "New Formulations and Branching Strategies for the GOP Algorithm"

Visweswaran, V. and C.A. Floudas, "Computational Results for an Efficient Implementation of the GOP Algorithm and Its Variants"

Byrne, R.P. and I.D.L. Bogle, "Solving Nonconvex Process Optimisation Problems Using Interval Subdivision Algorithms"

Vaidyanathan, R. and M. El-Halwagi, "Global Optimization of Nonconvex MINLP's by Interval Analysis"

Liu, M.-L. N.V. Sahinidis and J.Parker Shectman, "Planning of Chemical Process Networks via Global Concave Minimization"

Ierapetritou, M.G. and E.N. Pistikopoulos, "Global Optimization for Stochastic Planning, Scheduling and Design Problems"

Iyer, R.R. and I.E. Grossmann, "Global Optimization of Heat Exchanger Networks with Fixed Configuration for Multiperiod Design"

Quesada, I. and I.E. Grossmann, "Alternative Bounding Approximations for the Global Optimization of Various Engineering Design Problems"

Sherali, H.D., E.P. Smith and S. Kim, "A Pipe Reliability and Cost Model for an Integrated Approach Toward Designing Water Distribution Systems"

Smith, E.M.B. and C.C. Pantelides, "Global Optimisation of General Process Models"

This book is highly recommended for graduate students, researchers and practitioners in engineering, mathematical programming and operations research.

-PANOS PARDALOS

## *Iterative Methods for Linear and Nonlinear Equations*

By C. T. Kelley
Frontiers in Applied Mathematics 16
SIAM, Philadelphia, 1996

ISBN 0-89871-352-8

This book is devoted to giving a modern view of iterative methods for solving linear and nonlinear equations. The reader should have a basic knowledge of analysis and numerical linear algebra because most of the results are proved with mathematical rigor. The chapters are concluded with sets of examples and exercises. Throughout the text one can find motivating examples mainly from boundary value problems with partial differential equations. Many of the chapters contain links to MATLAB code which is provided per anonymous ftp from the author.

The first three chapters are devoted to the iterative solution of systems of linear equations. The first chapter contains a description of basic concepts for stationary iterative methods. Conjugate gradient methods are presented in the second chapter including preconditioning and the variants for solving normal equations. In the following chapter GMRES is discussed for nonsymmetric systems of equations. Other methods presented are Bi-CG, CGS, Bi-CGSTAB, and TFQMR. All these methods have been widely used for the solution of large scale linear systems. A chapter on fixed-point iterations introduces concepts for the solution of nonlinear equations.

Newton's method is discussed in depth including convergence rate analysis, implementation hints, and variants such as the chord method, finite difference method, and Shamanski's method.

Chapter 6 is a novelty in a textbook presentation. The concept of inexact Newton methods, where each Newton step is only computed up to a certain accuracy, is intertwined with iteration solvers from the earlier chapters. The question of how to control the residual error in the linear solver while retaining the fast convergence of Newton's method is raised, and it is shown in Chapter 7 that Broyden updates can be used in this context. They are applied to linear and nonlinear problems. All the previous convergence issues deal with local convergence results. Global convergence properties are the topic of the last chapter. Various implementations of Armijo's rule are given.

This book is an excellent textbook for mathematicians and engineers who want an insight into modern iterative methods for solving nonlinear equations.

-EKKEHARD SACHS

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number 50

**JUNE** 1996

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Student nominees are being sought for the Doctoral Colloquium at the Atlanta INFORMS Meeting, Nov. 1-3, 1996. Contact **donna.llewellyn@isye.gatech.edu**

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