Selected Papers of

Alan Hoffman

With Commentary

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Selected Papers of
Alan Hoffman
With Commentary

edited by

Charles A. Micchelli

State University of New York, Albany, USA

World Scientific

New Jersey • London • Singapore • Hong Kong

Published by

World Scientific Publishing Co. Pte. Ltd.

5 Toh Tuck Link, Singapore 596224

USA office: Suite 202, 1060 Main Street, River Edge, NJ 07661

UK office: 57 Shelton Street, Covent Garden, London WC2H 9HE

Library of Congress Cataloging-in-Publication Data

Hoffman, A. J. (Alan Jerome), 1924–

[Selections. 2003]

Selected papers of Alan Hoffman with commentary / edited by Charles Micchelli.

p. cm.

Includes bibliographical references.

ISBN 981-02-4198-4 (alk. paper)

1. Combinatorial analysis 2. Programming (Mathematics) I. Micchelli, Charles, A. II.

Title.

QA164 .H64 2003

2003053547

511'.60--dc21

British Library Cataloguing-in-Publication Data

A catalogue record for this book is available from the British Library.

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Printed by FuIsland Offset Printing (S) Pte Ltd, Singapore

This book is dedicated to Elinor,
who gives me love, laughter, joy and youth

Alan Hoffman
Greenwich, Connecticut
May 26, 2003

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Preface

Alan Hoffman was my mentor, colleague, and friend for nearly thirty years at the IBM T. J. Watson Research Center Yorktown Heights, New York. I often think of Mathematics as work, indeed hard work, albeit glorious work. Certainly, it is not easy work! Alan made me remember the fun of doing Mathematics and I am grateful to him for that. Editing this selection of his collected work was a joy. I am privileged to have been granted the opportunity to do it.

Charles A. Micchelli
Mohegan Lake, New York
June 12, 2003

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Biography

Born May 30, 1924 in New York City. Educated at Columbia (AB, 1947, PhD, 1950). Served in U.S. Army, 1943–46.

- 1950–51 Member, Institute for Advanced Study, Princeton
- 1951–56 Mathematician, National Bureau of Standards, Washington
- 1956–57 Scientific Liason Officer, Office of Naval Research, London, U.K.
- 1957–61 Consultant, Management Consultation Services, General Electric Company, New York
- 1961–2002 Research Staff Member, T. J. Watson Research Center, IBM, Yorktown Heights
- 2002– IBM Fellow Emeritus, T. J. Watson Research Center, IBM, Yorktown Heights

Adjunct or Visiting Professor at:

- Technion, Israel Institute of Technology, 1965
- City University of New York, 1965–1976
- Yale University, 1975–1985 and 1991
- Stanford University, 1980–1991
- Rutgers University, 1990–1996
- Georgia Institute of Technology, 1992–93

Students:

Fred Buckley, City University of New York,	1978
Michael Doob, City University of New York,	1969
Michael Gargano, City University of New York	1975
Allan Gewirtz, City University of New York	1967
Refael Hassin, Yale University	1977
Leonard Howes, City University of New York	1970
Basharat Jamil, City University of New York	1976
Sidney Jacobs, City University of New York	1971
Deborah Kornblum, City University of New York	1978
S. Thomas McCormick, Stanford University	1983
Louis Quintas, City University of New York	1967
Peter Rolland, City University of New York	1976
Howard Samowitz, City Univeristy of New York	1979
Robert Singleton, Princeton University	1962
Lennox Superville, City University of New York	1978

Present or past service on editorial boards of:

Linear Algebra and its Applications (founding editor)

Mathematics of Operations Research

Discrete Mathematics

Discrete Applied Mathematics

Naval Research Logistics Quarterly

Journal of Combinatorial Theory

Combinatorica

SIAM Journal of Discrete Mathematics

SIAM Journal of Applied Mathematics

Mathematics of Computation

International Computing Center Bulletin

Honors:

Member, National Academy of Sciences, 1982–

IBM Fellow, 1978–

DSc (hon.) Technion, 1986

Fellow, American Academy of Arts and Sciences, 1987–

Fellow, New York Academy of Sciences, 1975–

Phi Beta Kappa Lecturer, 1989–90

Special issue of Lin. Alg. Appl., 1989, for 65th birthday

von Neumann Prize (Operations Research Society &

Institute of Management Science), 1992

Founder's Award, Mathematical Programming Society, 2000

Fellow, Institute for Operations Research and Management Science, 2002–

List of Publications

1. On the foundations of inversion geometry, *Trans. Amer. Math. Soc.* **17**, 218–242 (1951).
2. A note on cross ratio, *Amer. Math. Monthly* **58**, 613–614 (1951).
3. Chains in the projective line, *Duke Math. J.* **18**, 827–830 (1951).
4. Cyclic affine planes, *Can. J. Math.* **4**, 295–301 (1952).
5. On approximate solutions of systems of linear inequalities, *J. Res. Natl. Bureau Stds.* **49**, 263–265 (1952).
6. (with H. W. Wielandt) The variation of the spectrum of a normal matrix, *Duke Math. J.* **20**, 37–39 (1953).
7. (with M. Mannos, D. Sokolowsky and N. Wiegmann) Computational experience in solving linear programs, *J. Soc. Ind. Appl. Math.* **1**, 17–33 (1953).
8. On a combinatorial theorem, *Natl. Bureau Stds. Rep.* 2377 (1953).
9. On an inequality of Hardy, Littlewood and Polya, *Natl. Bureau Stds. Rep.* 2974 (1953).
10. Cycling in the simplex algorithm, *Natl. Bureau Stds. Rep.* 2974 (1953).
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12. (with R. Bellman) On a theorem of Ostrowski and Taussky, *Arch. Math.* **5**, 123–127 (1954).
13. (with K. Fan) Lower bounds for the rank and location of the eigenvalues of a matrix, in *Contributions to the Solution of Systems of Linear Equations and the Determination of Eigenvalues*, *Appl. Math. Series No. 39*, 117–130, Washington (1954).
14. (with J. Gaddum and D. Sokolowsky) On the solution of the caterer problem, *Nav. Res. Logist. Quart.* **1**, 223–229 (1954).
15. (with W. Jacobs) Smooth patterns of production, *Management Sci.* **1**, 86–91 (1954).
16. (with H. Antosiewicz) A remark on the smoothing problem, *Management Sci.* **1**, 92–95 (1954).
17. (with K. Fan) Some metric inequalities in the space of matrices, *Proc. Amer. Math. Soc.* **6**, 111–116 (1955).
18. How to solve a linear programming problem, *Proc. Second Linear Programming Symposium*, 397–424, Washington (1955).
19. SEAC determines low bidders, *Res. Rev.* 11–12 (April, 1955).
20. (with G. Voegeli) Linear programming, *Res. Rev.* 23–27 (August, 1955).
21. Linear programming, *Appl. Mech. Rev.* **9**, 185–187 (1956).
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23. (with H. Kuhn) On systems of distinct representatives and linear programming, *Amer. Math. Monthly* **63**, 455–460 (1956).

24. (with J. Kruskal) Integral boundary points of convex polyhedra, *Ann. Math. Studies* **38**, 223–246, Princeton (1956).
25. (with G. Dantzig) Dilworth's theorem on partially ordered sets, *Ann. Math. Study* **38**, 207–214, Princeton (1956).
26. (with H. Kuhn) On systems of distinct representatives, *Ann. Math. Study* **38**, 199–202, Princeton (1956).
27. Generalization of a theorem of König, *J. Wash. Acad. Sci.* **46**, 211–212 (1956).
28. (with K. Fan and I. Glicksberg) Systems of inequalities involving convex functions, *Proc. Amer. Math. Soc.* **8**, 617–622 (1957).
29. Geometry, Chap. 8, 97–102, *Handbook of Physics*, McGraw-Hill (1958).
30. Linear programming, *McGraw-Hill Encyclopedia of Science and Technology* **7**, 522–523 (1960).
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33. On the exceptional case in a characterization of the arcs of a complete graph, *IBM J. Res. Dev.* **4**, 487–496 (1960).
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36. (with M. Richardson) Block design games, *Can. J. Math.* **13**, 110–128 (1961).
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111. (with C. Berge) Multicoloration dans les hypergraphes unimodulaires et matrices dont les coefficients sont des ensembles, *Colloques Internationaux C.N.R.S. 260, Problemes Combinatoire et Theorie des Graphes*, 27–30 (1977).
112. (with R. Graham and H. Hosoya) On the distance matrix of a graph, *J. Graph Theory* **1**, 85–88 (1977).
113. (with R. Oppenheim) Local unimodularity in the matching polytope, *Ann. Discrete Math.* **2**, 201–209 (1978).
114. (with D. E. Schwartz) On lattice polyhedra, *Proc. 5th Hungarian Colloquium on Combinatorics*, 593–598 (1978).
115. (co-editor, M. Balinski) *Polyhedral Combinatorics*, Mathematical Programming Study **8**, North-Holland (1978).
116. D. R. Fulkerson's contributions to polyhedral combinatorics, *Math. Programming Study* **8**, 17–23 (1978).
117. On lattice polyhedra III: Blockers and anti-blockers of lattice clutters, *Math. Programming Study* **8**, 197–207 (1978).
118. Helly numbers of some sets in R^n , *IBM Res. Rep.* RC7319 (1978).
119. Some greedy ideas, *IBM Res. Rep.* RC7279 (1978).
120. The role of unimodularity in applying linear inequalities to combinatorial theorems, *Ann. Discrete Math.* **4**, 73–84 (1979).
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Patent

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Autobiographical Notes

At a cocktail party, I am asked by a stranger, "What do you DO?"

"I do mathematics."

"Are you an accountant? Are you a programmer?"

"No. I prove theorems."

"Oh." The stranger searches the room wildly, hoping for rescue.

Who among us, my fellow mathematicians, has not had this approximate experience, not once but several times? Members of the general public, along with our spouses, siblings, parents and children, have not the foggiest idea of what we do and why we do it; but they must be curious about the mores of one of the world's oldest professions, and of its professors.

Why did we decide to become mathematicians, rather than lawyers or writers or entrepreneurs? What are the perils and pleasures of mathematical research?

Some of you may also have my taste for reminiscences. I learned from Ky Fan how Fréchet's students queued outside the professor's office one day a week waiting their turns to see him and report on their work. Alexander Ostrowski told me of the fierce competition among the students at Göttingen; Richard Courant told me anecdotes about its faculty. I heard from Magnus Hestenes and Adrian Albert about the old days at the University of Chicago, from Helmut Wielandt about moving from group theory to numerical analysis, from R. C. Bose how circumstances changed him from a geometer at a small women's college to the creator of much of the theory of experimental designs.

I love this mix of history and gossip. I also try to learn how and why certain topics started (why did George Dantzig create linear programming?) and what were the inspirations for particular theorems. In many respects I think of mathematics as a magnificent sport, of which I am a devoted player and fan.

Over the years I have been asked about the origin of some of my theorems and topics, and recently sat for a long video interview devoted to my activities in the early days of Optimization. These experiences led me to propose the inclusion, in this volume of Selected Papers, of comments about the origin or after-life of the papers included; I always seek such information from other mathematicians about their work. The major features of my education and career are sketched in these Autobiographical Notes. Let me say that to be a mathematician in the second half of the twentieth century was a spectacular opportunity. I hope this description of my small part in that great adventure will interest contemporary and future readers.

Early Years

Until I was almost 13, our family lived in Bensonhurst, a neighborhood in the borough of Brooklyn, New York City, not far from Gravesend Bay and Coney Island.

Boys played games after school: I remember being pretty good at boxball, marbles, stoopball and mumbletypeg, terrible at stickball and touch football. My older sister, Mildred, was vivacious and talented; our father, Jesse, was a manufacturer of women's dresses and subways daily to Manhattan; our mother, Muriel, managed the home. I was a good student in all subjects not requiring neatness or dexterity, and thought in a vague way that I would make my career in one of the learned professions. The idea that there was a profession called mathematician and an activity called mathematical research reached me before I was 12, because I had a mathematician cousin, George Comenetz, who had received his PhD from Columbia University in 1932.

Shortly before my thirteenth birthday, our family moved to the upper West Side of Manhattan, four blocks south of Columbia University. In the apartment next door was a retired actuary and passionate lover of mathematics, S. A. Joffe, who at this time was helping R. C. Archibald edit the journal *Mathematical Tables and Other Aids to Computation*. Mr. Joffe encouraged me to be a mathematician (and NOT to be an actuary) all the years I lived there.

I went to the wonderful George Washington High School, where my classmates and I studied geometry the old-fashioned way: we learned it, from a modernized Euclid, as a system of postulates and axioms from which we could deduce theorems. Sixty-five years later, I still get chills recalling that course. The concept that concrete information about tangible objects (triangles, trapezoids, etc.) could be found (and MUST be verified) by rigorous deduction from assumptions dazzled me like nothing I had ever seen before, and very little that I have seen since. Learning the concept of rigor was a stunning epiphany: what intellectual encounter is comparable? And you could make up proofs which differed from those in the text, and still be right; in fact, you could guess and try to prove your own theorems! I felt like King Arthur in Camelot! The teacher was Mr. Parelhoff (that's how I remember the spelling), and I will revere him forever.

Intermediate algebra was much inferior. I recall mild amusement at the introduction of strange Greek words like "polynomial", which I thought were designed to inhibit rather than advance learning. Trigonometry had some fun (proving miscellaneous identities), but wasn't in the same league as plane geometry. But the trigonometry course did have a distinctive and provocative feature: elaborate work with tables of functions. This forced me to confront the concept that mathematics could be useful as well as beautiful, and I was not so comfortable with that revelation. From Eric Temple Bell's "Men of Mathematics", I had concluded that useless mathematics was in principle nobler than useful mathematics. Pure mathematicians were monks in service to a higher religion whose secrets were known only to the residents of the monastery.

I graduated in January of 1940, and planned to start college (Columbia if I won a scholarship, City College of New York if I didn't) in the fall. Now was the time to choose a career. I loved mathematics, but I was also swept by other passions: the sonorities of poetry (large swaths of Kipling, Keats, Poe, Housman, even Alfred Noyes were committed to memory), the glamor of journalism (foreign correspondents interviewed diplomats and met beautiful women), indeed any activity closely or remotely connected to writing. I loved mathematics, but I think I loved literature more. Given the choice to be Shakespeare or Gauss, I would have instantly chosen Shakespeare; but knowing that I was neither Shakespeare nor Gauss, I felt that I

could pretend to be Gauss more deftly. I was also confident that, if I were unable to make my way in mathematics, I would have a reasonable chance in any scholarly profession.

So, when I began college, my first (but not only) choice of career was to be a professor of mathematics. I believed I had better than average talent for it, and I was not dissuaded because at that time the prospects for gainful employment were dismal. The one conceivable alternative was physics. Einstein was a physicist; ergo, physics was an important subject. But the elementary physics course at Columbia was intoned in uninspiring lectures in a huge auditorium. The smaller recitation sections, led by the brilliant but sleepy Willis Lamb and Martin Schwarzschild, didn't compensate.

Here is how I spent my time at Columbia before going off to the Army in February of 1943. I had the great good fortune to meet some upperclassmen (Fred Bagemihl, Bernie Gelbaum, ...) and graduate students (Leonard Gillman, Fritz Steinhart, ... and especially Ernst Straus) who took seriously my boyish enthusiasm for mathematics. I also had the good fortune to meet Alex Heller and Max Rosenlicht, who were respectively one and two years behind me, and far more gifted. Yet, even though Ernst, Alex and Max had much more firepower, I did not feel deficient in imagination; and I knew that mathematics is a subject in which it is just as meritorious to raise questions as to answer them, maybe even more so. Laymen tend to think mathematicians are just quintessential problem-solving wizards, but we members of the profession know they are wrong.

Also, a mathematics professor not only proves theorems (more accurately, conceives and proves theorems), but also lectures. I joined the Debate Council to overcome (or, at least, learn to live with) the fear of public speaking I had known in high school. I also became active in movements to support American aid for the Allies in the war against the Axis, and movements to get America to enter the war directly. I found the classes in the history of governments, philosophy and literature unbelievably stirring; these courses are the core of the Columbia College educational experience, and their memory a bond among alumni. (Amazing but true: at an evening meeting of the National Academy of Sciences where awards were made for different scientific achievements, I heard five of the honorees, all of them scientists, call this curriculum part of the foundation of their research careers.)

My teachers in mathematics were, principally, the incomparable J. F. Ritt, whose wit and pedagogy were legendary, and G. A. Pfeiffer. Most of what I know about teaching I learned from Ritt: how to demand the concentration of the class when necessary, how to break the tension of concentration with a witty aside, how to give students some whiff of the history behind mathematical ideas. I spent years trying to learn how to do these things gracefully.

Pfeiffer, on the other hand, was considered such a poor teacher by some of the students that, even in those ancient times when faculty received tremendous deference, a group of them had persuaded Dean Hawkes that Pfeiffer was unfit to teach calculus. But Pfeiffer was a marvelous teacher for serious students. Through a flaw in the course selection system, I took a challenging course in set theory (with a text by Hausdorff, in German), for which I was woefully unprepared mathematically and linguistically. Pfeiffer taught the course by reading the book and discussing whether in various places the author's arguments were flawed! To be invited to consider such questions, in a small class of less than 10 serious students, made me feel

that I had been invited to be a pledge by the august fraternity of mathematicians. Then I took from Pfeiffer a course in foundations of geometry, where the material was easier for me. And I liked this course so much that my thesis grew from it, as I explain in my comments a bit further down in this section, and in the first chapter of this collection of papers. Unfortunately my education in calculus was a little skimpy; and even to this day, though I use calculus occasionally, and sometimes with panache, I don't feel totally confident or trust my instincts.

I stayed multiply involved with clubs and organizations and studies even though (or maybe because) a terrible war was in progress and my own military service was imminent. In fact, I intentionally hastened that military service by not registering for the fall semester of my junior year. I kept going to classes and participating in other activities, but I just didn't register. Nobody seemed to notice except the Army, which called me to service in February 1943.

How can I describe my three years in the Army? It was the climactic event of my life, with adventure magnified by the sensibilities of youth, and I cannot recreate that experience in words. But I will describe how these three years of service intersected my mathematical career. In the early weeks of basic training at Fort Eustis, the anti-aircraft artillery school, deep in the Tidewater country of Virginia, I got an idea for what I hoped would be an interesting project in the foundations of geometry: to develop axioms for a geometry of circles. I was lucky, and found phenomena in the geometry of circles analogous to phenomena in the geometry of lines. It turned out that the analogies were better than I could have imagined. They were there, but they weren't exact: like a painting perfectly planned but slightly, and in accordance with the artist's intention, a little unbalanced to provoke aesthetic interest. Of course, pursuing this project took some effort. At one time, I carried in my head a swirling vision of a configuration in space containing ten points and nine circles arranged on seven spheres; and I had to construct this configuration gradually, because the construction was the fulfillment of a mathematical argument created in fits and starts, with errors that had to be discovered and corrected at every step. And I had to carry this in my head, because I can't draw. Fortunately, even in the midst of intense basic training, there was so much "hurry up and wait" that I could cultivate and memorize the diagram.

(I also began with this experience the practice of not writing down partial results until I am convinced I know enough to write a full paper about the topic I am studying. It is a ridiculous practice, which I recommend to no one. It has, however, one virtue: if I return to this topic, after an interval of months or years, I am not handicapped by having a record of the way I thought about it before. In *The Psychology of Invention in the Mathematical Field*, Hadamard argues, with supporting quotations from Poincaré and other savants, that the ability (sometimes) to solve a problem that had been intractable when first put aside months or years earlier is because the mind has been unconsciously working on it. I think it is equally (maybe more) plausible that returning to the problem after an interval allows you to pursue it with a smaller chance of following false paths that you are lucky you forgot.)

After a month, I was sent to the anti-aircraft artillery meteorology school, and after graduation became an instructor at the school. What I taught were some rudiments of trigonometry that were used in tracking balloons to plot winds aloft. I did not feel comfortable teaching, and I think I did it poorly. Later, as the anti-aircraft school was closing, I was sent to the University of Maine to study

electrical engineering. Spofford Kimball gave the mathematics courses I attended. I was astounded by the clarity of his lectures, and any potential interest in studying electrical engineering was blown outside to the cold winds of Orono and the Penobscot River. Besides, I had already in basic training done some work I considered interesting mathematics. How could the study of electrical engineering compete for my enthusiasm? I now had among my belongings (which I kept until they were stolen from me in June 1945 when I went on a weekend trip from Augsburg to Paris) the slim volume *Introduction to the Theory of Numbers* by Dickson, which I hardly ever opened, and the heavy two-volume *Projective Geometry*, by Veblen and Young, which I did open from time to time. Besides these stimuli, I received mail from Straus, and each letter contained about ten mathematical questions, research questions appropriate for my limited knowledge, not problems suitable for or taken from a student magazine. I made limited progress on only two or three per letter, and there were only a few letters, but what a joy to receive them.

After Maine, I moved to the Signal Corps: training at Fort Monmouth, NJ, in the rudiments of long-lines telephony; on to C Company, 3186th Signal Service Battalion; arrived Liverpool in early December 1944, eventually (with the war almost over) to southern France and southern Germany (I was in Nesselwang on the day the war in Europe ended), traveled from Marseilles to Manila (Hiroshima's bombing occurred when we were already west of the Panama Canal) to Yokohama, returned home in February 1946 almost exactly three years from the day I entered. I thought a little bit about mathematics during this year and a half, but I didn't DO mathematics. I did some teaching: when the battalion reassembled in Augsburg after various individuals and teams had operated division-to-corps (and higher echelon) radiotelephone communications all over Europe, some of us set up a little university and conducted short courses where we taught each other what we knew (the 3186th was a very unusual battalion). We later did the same in Manila. I wrote down my forays into circle geometry in little blue examination booklets, and thought about how I would show this work to my friends and to Prof. Pfeiffer.

A sudden case of pneumonia kept me from enrolling for the spring semester. But when I recovered, I discovered that Pfeiffer was no longer at Columbia and Prof. E. R. Lorch was now teaching the courses in foundations of geometry. I showed him my blue booklets, confident that he would find the material suitable for a master's essay. But he surprised me by suggesting that it could be the basis for a dissertation. That was great news. More great news was that I was asked to teach a mathematical survey course at the Columbia College of Pharmacy, even though I was still an undergraduate. Here was a chance to practice pedagogy. My short teaching career at Fort Eustis had not been a rousing success; I did slightly better in Augsburg and Manila; but I was very uncertain that I could function adequately in my chosen profession of teaching.

On the opening day of the fall semester, I wrote my name on the board, I described how homework would be assigned and exams scheduled, gave an outline of the material to be covered, answered questions like "do you grade on the curve?", answered a few mathematical questions at my desk after the bell had rung and most of the class had left the room; and realized that my pulse was racing at 120 beats a minute. I had held the attention of the class for the full 50 minutes, I hadn't panicked or felt fear of any kind, my sentences had been reasonably grammatical, I had given some clear answers to a few technical questions: in short, I had felt

comfortable as a teacher, and was totally confident that, if asked, I could do it forever. And, apart from my maladroitness handling of a case of cheating, I thought that year at Pharmacy went very well. Also, before the end of the academic year, which means before I had graduated from College, I had worked over the research I had done in the army sufficiently to be very confident that I could get a thesis from it. So my career was on track!

During the year, I had become very fond of Esther Walker, the sister of my army buddy Alex. Buoyed by my success in research and teaching, I proposed marriage. She accepted, and we were married on the same day that I graduated from college. And, in the fall, I began graduate studies at Columbia brimming with confidence.

But what I did not realize at first was that having the thesis more or less in hand before leaving college had deplorable consequences. In the first place, it gave me an incentive to stay on at Columbia rather than go elsewhere, where, I believe, I would have been forced to learn much more. Second, anxiety about a dissertation is a wonderful incentive to study; but I didn't have such an incentive, and I loafed. I loafed with such devotion and consistency that I failed my algebra examination for the doctorate on the first attempt.

Candor compels me to report another personal flaw that first appeared during my years as a graduate student: an almost childlike impatience. I found sitting in a chair for an hour and a half lecture almost unendurable. I found reading technical material for that long very hard. Over the years I have developed various strategies for coping with this impatience, but I am nevertheless amazed that I have been able to function as a scholar with this handicap.

Eventually I passed all my exams, defended my thesis and departed for a postdoctoral fellowship, sponsored by the Office of Naval Research, at the Institute for Advanced Study in Princeton. In those years, the Institute was the summit of the mathematical world (Morse, Weyl, Siegel, von Neumann, . . . , none of whom I ever engaged in any significant conversation. I did, however, spend much time with Oswald Veblen, who wrote the book I carried with me in the army). When we arrived at the Institute in midsummer, 1950, I realized it was now time, actually way past time, for me to start working diligently on my career. Besides my thesis, I had proved a few small theorems in the theory of complex variables which were perhaps publishable, but I felt they were probably a little too little. Now was the time to really get to work: to get up in the morning, have breakfast, walk from our miner's shack on Springdale Road to my office (next to Einstein) in a corner on the first floor of Fuld Hall, and work. As a student, the only significant time I had spent concentrating on mathematics were in occasional all-night sessions, lying in bed and worrying some problem like a dog with a bone. But I now realized that, whatever be the level of my talent and knowledge, my accomplishments would be proportional to the amount of time I devoted to my profession. And I wrote a few papers, one of which I have included in the first chapter; what was more important was that I established a rhythm of work. You are a mathematician, you do mathematics. Since that time, with one exception that I will describe later, I have been UNABLE (not unwilling, but unable) to enjoy two weeks free of mathematics; it has become an addiction.

Because this essay is pledged to my professional work, I refrain from detailing the host of experiences during that fabulous year 1950–51. I shall not describe how the intellectuals living in Institute housing left poisoned meat under the bottom

floor to persuade an odiferous skunk to prowl elsewhere. The skunk ate the meat and promptly died, forcing a lengthy evacuation of the building. But I should mention our friendships with the Estrins and Bigelows, who were then building the IAS computer, an activity in which I took not the slightest interest; how Marston Morse would comment at seminars how much the speaker's research was related to Morse's work many years earlier, which behavior, regrettably, I now find myself occasionally imitating; attending Artin's lectures at Princeton University (he was fabulously dramatic); appreciating the other attractions at the Institute besides science (Panofsky on the history of art and Kennan on government, for example, were fantastically brilliant; Scandinavian meteorologists and their spouses incredibly handsome).

National Bureau of Standards, Office of Naval Research (London), General Electric Company

I needed a job when the fellowship ended. I was not especially choosy, but I did not have any academic offer in a location where I wanted to live. Feeling desperate, and contrary to almost everyone's advice that working for the government was inappropriate and would mean abandoning research for money, I wrote to the National Bureau of Standards (NBS, now the National Institutes of Science and Technology, NIST), and two days later received a telephone call offering me a job! Even though I knew nothing, as I readily acknowledged, of applied mathematics! I was so impressed that they were so impressed by my résumé (and the letters of recommendation that had brought me to the Institute) that I accepted instantly. Nor have I ever regretted the decision. The entire arc of my career is based on the experiences of the five years I spent in Washington at NBS. Initially, I thought that, after some time at the Bureau, I could in some future year find the academic position for which I had prepared. I would wear tweed jackets with leather patches, smoke pipes, deliver witty lectures and, from time to time, discover and nurture or possibly romance some brilliant student. But that never happened.

The Applied Mathematics Division at the Bureau had a contract with the Office of the Air Controller of the United States Air Force to pursue a program of research and computing in a subject of which I had never heard, called "linear programming", and I was hired to help fulfill that contract. Since linear programming was very new, my ignorance of traditional applied mathematics was not an insuperable obstacle; and I found the new subject a delicious combination of challenge and fun. It was also a marvelous opportunity to contribute to the early development of a part of mathematics that has thrived in many contexts: practical operations research, applications to engineering and many other branches of science, applications to combinatorial theory and to diverse topics in numerical analysis. When a subject is new, any simple insight is potentially a fundamental theorem, and you may be lucky enough to have the theorem identified by your name. Another virtue of early entry into a priesthood is friendship with other acolytes and the joy of exchanging ideas and problems.

But these contacts with George Dantzig, the father of linear programming, and his colleagues at the Air Force, gave me much more than a mathematical playground. George and his buddies believed that they were developing a technique for helping to run an organization (at least part of an organization) through the

use of mathematical models. I am always skeptical, as a point of honor, of claims about the applicability of mathematics. But in this instance I was totally wrong, as I realized after a few months. And my experiences using linear programming in business contexts, while never my main interest, introduced me to the worlds of operations research, management consulting, manufacturing research, finance... a whole constellation of cultures I would otherwise have never encountered. I never felt exactly at home in these cultures, but I enjoyed the exposures.

At that time, most of the Bureau's laboratories were in a campus-like setting, with gorgeous azaleas and other flora, west of Connecticut Avenue in northwest Washington. From our apartment in Silver Spring, Maryland, a scenic drive through Rock Creek Park, including a ford over a small stream, brought me to the laboratory. Several of us, including the linear programming group, worked in a large room, bullpen style, so the atmosphere was very social. I loved the place. I loved the people and the work. I had infinite energy: I was writing papers, supervising calculations, working jointly with others on calculations and on formulating models, and I felt powerful. I collaborated with marvelous mathematicians (such as Morris Newman and Olga Taussky, who were also employees; visitors Ky Fan and Helmut Wielandt; colleagues elsewhere such as Dick Bellman, Joe Kruskal and Harold Kuhn). I learned fascinating concepts in experimental designs from colleagues in statistics, particularly Bill Connor and Marvin Zelen. (Of course, I wasn't becoming a professor, although I did some adjunct lecturing at American University, and George Washington University, polishing my skills in anticipation of the time when, in due course, I would leave the government for full-time academic employment.)

I also learned there were things I did not do well. I had no special skill in programming: I wrote a code in 1951 which just didn't run. It didn't make errors, it just never succeeded in starting the computer (our machine, SEAC, examined the first eight words of a code to find, among other things, instructions about continuing. But SEAC refused to accept my eight words). Though I believe to this day that the fault lay in the hardware, not my program, the experience was disheartening and I never wrote another program. For some calculations, I had a knack for choosing what to put in the computer and what to put on the magnetic tape; but I realized that I had no special skill or talent in numerical work generally. This was also obvious from conversations with John Todd, my boss, and with Jim Wilkinson, the world's expert on numerical linear algebra: that they had instincts and insights that I would never attain.

I owe a special debt to John Todd and to his wife Olga Taussky. Jack was fair, firm, friendly, and shielded our group from intermittent crises as best he could. Olga was employed as a consultant (probably because of nepotism rules), and she was counselor, den mother, mentor to me and to all the young mathematicians. She taught us the culture of the profession: how to conceive problems and questions, how to write papers, indeed the whole publication process (how to referee papers, how to respond to editors' comments). She was delighted (I am sure it was genuine, not feigned for effect) when one of our mathematical investigations went well, and sympathetic when it went poorly. I loved Olga. In later years, I have tried to do for others what she did for me.

By 1956 I felt I had done yeoman work, and I coveted what appeared to me to be the most glamorous job in mathematics: scientific liaison officer (mathematics) at the London branch of the Office of Naval Research. This office had been created

shortly after the end of World War II to reestablish connections between European and American scholars. The job of scientific liaison officer entailed travel around Britain and the continent, visiting laboratories and universities, attending meetings, writing reports of what you learned, and helping Europeans and Americans with similar interests get to know each other. The big attraction was the expatriate adventure, not as exotic as in the 1920's, but far less common than it is now. And living in England, the linguistic and historic homeland of Americans! I persuaded Joe Weyl at ONR headquarters that I was ideally suited for the position, and also was assured by Jack Todd that I would be welcomed back at NBS.

Our year and a half in London had its share of hardships, particularly since our daughter Eleanor was only two years old and Elizabeth less than 6 months when we arrived, but I think that on the whole the experience was positive for Esther and me. We had nice holidays in Devon, Cornwall and the Lake District; in Paris and in southern France; and in Rome, Venice, Naples and Taormina. The year and a half was a perfect sabbatical. A sabbatical (which was, essentially, what the ONR job was for me) is more satisfying than travelling to meetings. Mathematical conferences, the "leisure of the theory class", are delightful venues for listening and learning, for renewing and establishing friendships, for preaching (this is Paul Erdős' language for lecturing) and preening, even for a little (actually, remarkably little) lust. But on a sabbatical, you learn to think like, and live like, and really be a local. And in England! Where (for example), thanks to the courtesy of John Hammersley, I could feast with the Fellows of Trinity College, Oxford, in the Tower Room of the Senior Common Room, on wine, fruit and conversation!

I found that, although I was determined to concentrate on the job, and NOT do mathematics, I was unable to refrain. In fact, when we were crowded into a room of an apartment hotel near Marble Arch before finding our permanent place on Kensington Square, I started doing mathematics to drown out the wails of my crying baby and the screams of a warring couple across the courtyard: it is well-known in the profession that doing mathematics eases pain and sorrow. (That Marble Arch research, closely related to interests of Al Tucker at Princeton, went well. When I returned to the States and told my results to him, he suggested to the organizers of a symposium on combinatorial mathematics, to which he had already been invited, that I should speak in his place. I kept the title "Some recent applications of the theory of linear inequalities to extremal combinatorial analysis" which Al had already chosen, and the paper appears in Chap. 5 of this book. Tucker was amazingly generous.)

I also did mathematics in hotel rooms and bars and sleeping cars. Half asleep on a train to Frankfurt, I discovered a beautiful theorem connecting a topic in algebra to the geometry of circles; and I lectured on this work at Frankfurt and at Mainz. I thought this theorem is maybe too beautiful to be true, and this sentiment was reinforced when I got back to London and realized my proof was flawed. The theorem is true, however, as Jeff Kahn showed in his thesis many years later. The one place where I couldn't do mathematics was at the office. Julian Cole, the distinguished aerodynamicist, and I faced each other across identical abutting desks and it was impossible to concentrate.

Instead of returning to Washington, or looking for an academic position, I investigated two job opportunities in New York. One was with a fledgling mathematical research group in IBM at a beautiful location (Lamb Estate) in Northern

Westchester. But the group was tiny and I did not think it had much future; also, at NBS, we were always a little snooty about IBM for no good reason that I can remember. The other was at the headquarters of General Electric, teaching operations research to people in the various departments of the company and helping them in any appropriate way when they returned to their respective departments. This was an established operation, the location in midtown Manhattan very exciting, the salary very attractive, and the chance to see if I could (and if operations research could) succeed in business was intriguing. So I accepted.

The job was fascinating in three respects: (1) since we were, organizationally, close to the Chairman, I learned something of life at court, where the mood of the monarch is constantly assessed from clues offered by tone of voice, tilt of eyebrows and the like; (2) because GE was a very diverse company, I became friends with people making jet engines, steam irons, military electronics (light and heavy), steam turbines (large and medium), lamps, plastics, and so on; (3) our group's location within Management Consulting Service gave me a chance (really, an obligation) to observe the culture of Management, which was very different from any I had known. Peter Drucker was a frequent visitor and probably our intellectual leader. I found him wonderfully entertaining in conversations between adjacent urinals, but the discussions of management philosophy seemed (I want to say this respectfully) less profound than their reputation.

The job was very satisfying in many respects, although some part of me kept telling myself I belonged somewhere else. My boss Mel Hurni said it was OK for me to do mathematics if it didn't interfere with my assigned duties. But it was clear that he wasn't thrilled by this research, almost all of which was orthogonal to the mission of our group. I was, nevertheless, very active mathematically at this time even though it seemed incongruous to do such work high in an elegant office building in the heart of Manhattan at 57th St. and Park Avenue.

In the summer of 1960 the mathematics department at IBM Research, still at the Lamb Estate in Westchester, but now large and active, invited me to participate in a summer workshop on combinatorics. I was only able to come for a couple of days, but I was dazzled by the atmosphere. The campus reminded me of NBS, except that the Lamb Estate was prettier. There was a large lawn for frisbee and other games, and various small cottages, no two alike, with fireplaces and attics adjacent to offices. And people all around doing mathematics! It was time, I concluded, to quit GE. I had been invited to join IBM by Herman Goldstine, Herb Greenberg and Ralph Gomory several times over the years, and in 1961 I accepted. In the back of my mind I thought: this seems like a great place to work, but it probably won't last. So I will stay here a couple of years and get myself a proper position as professor in some nice place when the atmosphere sours. But that time never came.

Alan Hoffman at Columbia, New York, 1948

Ernst Straus, Cambridge, 1950

Alan, Esther, Liz and Eleanor Hoffman at Brandeis, Waltham, 1977

Emilie Haynsworth, Alan, Olga Taussky and Gene Golub, Gatlinburg, 1964

Alan lecturing at conference celebrating George Dantzig's 85th birthday, Philadelphia, 1999

Alan with Elinor Hershaft, Greenwich, 2002

Alan, Phil Wolfe and Jack Edmonds at Georgia Tech, Atlanta, 2000

Founder's Award ceremony, Mathematical Programming Society, Atlanta, 2000. (From left) Phil Wolfe, Harold Kuhn, Harry Markowitz, Ralph Gomory, George Dantzig, Alan, Guus Zoutendijk and William Davidon.

Don Coppersmith and Alan at IBM, Yorktown Heights, 2003

IBM, CUNY, LAA, Stanford

My first act on joining IBM was to leave for a summer workshop on combinatorics at the Rand Corporation in Santa Monica. Tucker was chairman of the workshop, and I was delighted to discover that his working definition of combinatorial mathematics, so far as I could tell, was all (well, almost all) things of interest to me. So I felt very much at home; I knew most of the participants from other places, and I understood almost everything that was discussed (mathematics has become so specialized that it is possible to attend a conference even in one's specialty and not understand some parts of what other people are discussing). I also met the young Jack Edmonds, who was at the start of his brilliant career; and Claude Berge (sculptor, writer, discoverer and collector of primitive art, mathematician, . . .), incredibly versatile but at that point in his life very lonely. After the summer, I came back to Westchester and to a windowless Saarinen building which, several miles from the bucolic Lamb Estate, was the new home of IBM Research.

At NBS, ONR London and GE, I had always been among the youngest. When I joined the mathematics department at IBM Research, I was immediately among the oldest. Most of my colleagues were recent Ph.D.'s, in the act of establishing themselves and their careers; I had received my degree eleven years earlier, I had some reputation as a leader in my fields of interest, and I felt like a senior and acted like a senior. For several years I made it a practice to get to know all the mathematicians in the department and learn what they were doing. And what began as an emulation of what Olga did for me was great fun mathematically as well: my younger colleagues were very smart and were doing very interesting work.

I stopped the practice of entering stranger's offices invitationless a few years later, after a nine-month period as acting director of the Mathematical Sciences Department while Shmuel Winograd was away, when I thought my visits could be regarded as (and occasionally really were) management inquiries. This was not the only consequence of my acting directorship. Because I felt the position required full time attention, I vowed to do no mathematics whatever during my tenure. My explicit objective as director, which raised a few eyebrows, was to maximize the euphoria of members of the department, and that required all my attention. I abstained from research from September to the Christmas holiday, when a febrile convulsion from encephalitis put me in a coma for two days. I awoke in what was obviously a hospital room with a neurologist asking questions clearly intended to test my alertness and cognition; obviously, something bad had happened to me. Although I thought my answers coherent, I asked myself after the neurologist left if my brain could function on an adult level. And was I still capable of serious thinking? So I decided to test myself, did some mental arithmetic with two-digit numbers (I add, therefore I think), and concluded that maybe I could still function in my chosen profession. Then I asked myself: what was that intriguing mathematical question I had put aside four months earlier when I began as acting director, and I remembered, and the addiction to research resumed. For the rest of my tenure, research consumed a much larger fraction of my time and energy than I thought (and think) was appropriate.

My term ended with an annual report in verse, most of which was atrocious. But I was proud of the preamble, an adaptation of the opening of Coleridge's *Kubla Khan*. And the experience as director taught me to sympathize with all the

incumbents who had that responsibility, even when some of their actions strained my geniality.

I will devote the next section to miscellaneous reminiscences of IBM and my research. What happened to my intention of becoming a professor? I didn't make it, except as a visitor. But that turned out well for me in the following sense. When I was invited to teach at various places, the courses I taught were always in subjects where I had a special interest (why ask a visitor to teach elementary calculus?), so I have never had the fatiguing experience of teaching material I found boring. Pretty lucky for me. But I have always thought of myself as both mathematician and educator, and used both descriptors in *Who's Who in America*. And I have spent many hours lecturing in the classroom and advising Ph.D. students.

While at General Electric, and in my first years with IBM, I taught classes at New York University, Columbia, the New School, Yeshiva University and the Technion (Israel Institute of Technology). I had been a Zionist since high school, and teaching at the Technion, located in the beautiful northern Israeli city of Haifa, fulfilled a lifetime dream. After an initial hurricane, my family and I enjoyed wonderful weather and hospitality and touring. The course I taught there was on the basic facts of linear programming. I was proud of the notes I prepared, which I think treated that material better than any text I have seen, but I never published them.

When I returned from Israel in 1965, I began teaching at the Graduate Center of City University of New York. All the teaching I had done in the preceding ten years had been in linear programming, but my CUNY courses were in combinatorics; and they were, for me, as fine a teaching experience as I could have imagined. My first year there was the best. I had about seven students (all of whom became professionals) and two professors, the graduate mathematics program was very new, there was an air of excitement in the classroom. And, starting that year, I worked with a corps of students for a long enough time that, even though I was an adjunct professor, I supervised theses. That is a precious obligation and opportunity: getting a degree in mathematics can (and generally does) change a person's life, and guiding a student towards that degree is, for the teacher as well, a moving experience. I owe a debt to my students for the satisfaction they have given me, and I am proud of their character as well as their scholarship. I am especially proud of my first CUNY student, Allan Gewirtz (after whom is named the Gewirtz graph), who, among other achievements, has an unbelievable record of community service at Brooklyn College, where he taught mathematics and was also dean of general studies; in the ambulance corps and as president of a hospital in Monmouth County, New Jersey; and as a teacher in drug rehabilitation centers in upstate New York; among other activities.

Teaching and research are intertwined: a sizable fraction of what I teach comes from, or is closely related to my research; and a sizable fraction of my research is a consequence of some kind of academic encounter. I am sure that these facts were arguments used by Ralph Gomory when he shepherded through the bureaucracy permission for me and others to teach part-time even while employed full-time by IBM.

Eventually I stopped teaching at CUNY because New York City had financial troubles, and taught elsewhere. I had an amazingly large number of students at CUNY for an adjunct professor, which of course I attributed to my superior skills.

I never duplicated that success at Yale, Stanford, Georgia Tech or Rutgers, but I have many plausible explanations for this failure.

A few words about my experience as founding editor of *Linear Algebra and its Applications* (LAA). Alexander Ostrowski spent a sabbatical at IBM in 1967 and asked if I would consider becoming the editor-in-chief of a new mathematical journal specializing in linear algebra. He had already been in contact with the publishing house American Elsevier about the possibility of inaugurating such a journal, but did not wish to assume that editorial responsibility himself. Of course, I was thrilled to be asked and accepted without hesitation. I had been managing editor of the *Naval Research Logistics Quarterly* twelve years earlier, but starting a journal was a bigger undertaking.

Looking back, I think that Lore Henlein, the editor at Elsevier, and I did a splendid job in getting LAA started. My part was establishing a scope not too narrow and not too wide, recruiting a capable board, soliciting the first papers, and establishing a rhythm for the editorial activities. The first issue appeared in 1968. "Lore, Lore, hallelujah!", I wrote in a congratulatory note to Ms. Henlein. The journal still flourishes, but I ceased being editor in 1972. I was not able to maintain the routine of paying systematic attention to the fates of the papers that were submitted, or the responsiveness of the referees; and I was unable to recruit administrative help that could have done it for me. I would also agonize indecisively about various editorial disputes. My board urged me to quit, which I did eventually, although I should have done it much sooner. Hans Schneider, to whom I am eternally grateful, came to my rescue and was appointed the next editor-in-chief. He was later joined by Richard Brualdi, and most recently by Volker Mehrmann, as joint editors-in-chief.

Besides humility, I learned a lot from this experience about my own limitations. I also learned that friends were as willing to forgive as I was eager to apologize.

In 1980, hoping to escape from the cold New York winters which aggravated the miseries I was suffering from asthma, I started teaching mathematical topics at the Stanford University operations research department during the winter quarter, and continued for almost ten years. At Stanford, I experienced for the first time "student evaluations", and learned what they thought of my teaching. In general, they thought my handwriting illegible, my organization of the material rather sloppy, and my enthusiasm great; I could not have agreed more with these evaluations. (J. F. Ritt would have ranked tops in all criteria.) My energy was spent on conveying the joy of mathematics: how to think of questions to investigate, questions which are natural, or ingenious, or simply fun. And I did not mind if occasionally, but not too often, I lost my way and had difficulty reconstructing an argument. It is good to (1) show students how to identify and challenge an obstacle, which I would do aloud and invite audience participation, and (2) in this process, let them "hear" how your mind works, which is something they will never get from textbooks. So even your mistakes as a teacher are helpful. But I never was as shamelessly unprepared as Professor Paul Smith was at Columbia. Smith was constantly getting lost, and turning sheepishly to Alex Heller to rescue him. Students learned nothing from Smith's lectures except the lesson that even a brilliant mathematician can have a bad memory and think on his feet very slowly. In an odd way, that was good for our morale.

More about lecturing. Students generally give you clues about how well you are doing. If the material is difficult, they look puzzled, implicitly pleading for further or alternative expositions of the material. If they understand the material, they nod affirmatively and vigorously, encouraging you to move on to the next topic. The only time this system of indicators failed me was when I was an instructor at Barnard College, the women's college of Columbia University, in 1949–1950. The women's faces showed nothing, neither understanding nor confusion. It took me months to recognize tiny signals, like the twist of a neck or the adjustment of spectacles, for clues about the effect of a lecture. There was no such problem at Stanford: the reactions of the students were very easy to read.

These winters became the best part of each year. Our friends (Cottles, Dantzig's, Bermans, Mannes, Veinotts, Jacobsteins, Curtis Eaves, Gene Golub. . .) were incredibly hospitable, the University atmosphere exciting, the outdoors inviting (Foothill Park, Point Lobos, Half Moon Bay. . .).

In 1986, Esther contracted a fatal blood disease. We continued to visit Stanford, which she loved as much as I did, but her energy level gradually decreased. At Stanford, she typically would spend morning attending a class or visiting with friends, and the afternoons resting. At home she bravely continued her activities as much as she could. We even travelled together to some meetings. She died in the summer of 1988. I am comforted that she did not seem to be in physical pain; indeed, when last I saw her alive, at the hospital where she seemed to be recovering from a sudden strange shock to her blood chemistry, she was joking with the nurses. Her courage in the last years was incredible.

A year after Esther died, I met the young, beautiful and incandescent Elinor Hershaft, an accomplished interior designer. We married in September 1990, and have lived happily ever after in Greenwich, Connecticut. Ellie wasn't able to go away for the winter with me, and after a while I stopped going to warm places like Stanford and Atlanta (my IBM colleagues Earl Barnes and Ellis Johnson had become professors at Georgia Tech). I also began, at the invitation of my friend Peter Hammer, teaching one semester each year at Rutgers in New Jersey, but stopped about six years ago. Now that I am retired, I hope to resume teaching. Ellie has "youthanized" me, and I love the classroom dynamics.

Reminiscences

Finally, let me close these Notes with further reminiscences about my years at the IBM Research Center, and further comments about my research habits. First, some miscellaneous memories.

For about 15 years, a group of about eight of us played The Coin Game at lunch. The ostensible purpose was, via an elaborate set of rules involving holding coins in fists and guessing the sum of the number of coins collectively held, to decide who would fetch and pay for dessert for the group. But the real purpose of The Coin Game was the exchange of taunts and insults about the intellectual prowess of the other players, each taunt an expression of affection for the taunted. The Game faded away not long after Alan Konheim (who shared with Roy Adler the leadership of The Game) moved to University of California at Santa Barbara.

Courtesy of an old friend from the Bureau of Standards, Leon Nemerever, I worked at the election night programs for CBS in 1962 and 1964, representing

IBM on a committee which reviewed the computer output for sanity before passing its predictions to the correspondents on TV. Very late in the night in '64, our committee, giddy with weariness and boredom, decided on a victor in a Senate race for a state in the Southwest, even though the computer said the race was too close to call. And we were wrong! Fortunately, the correspondents were at that hour equally tired and never broadcast our error.

I remember coming into the office early one morning and seeing Bryant Tuckerman jump four feet into the air when he looked at the results of the previous night's computation and saw that he had found the largest number known to be prime. Although the mathematics of the algorithm used was standard, Bryant's code, designed to exploit every feature of the computer, was outstanding. Bryant also had a flair for cryptography. Incidentally, I knew his father, L. B. Tuckerman, one of the world's experts on metrology at the Bureau of Standards; and I remember an eerie feeling watching Bryant once write on a blackboard an equation identical to one I had seen his father write two decades earlier (Bryant's equation dealt with cryptography, his father's with the measurement of gravity!).

I remember being at the Johnson Space Center near Houston, Texas, where Gerry Rubin and I worked on a proposal for scheduling the experiments to be carried by the space shuttle (they were then thinking of flights occurring as often as once a month; by the way, we didn't get the contract). Suddenly Bob Brayton called me to say that a summer student, Don Coppersmith, had completed the proof of our paper on self-orthogonal latin squares (in Chap. 2 of this book). I remember H. V. Smith demonstrating that checkers was a really really deep game during social moments when he and I were working together on a financial planning project for IBM that turned out to be hugely successful. I remember a discussion with an engineer from our plant at Poughkeepsie who tried to convince me that his method for scheduling the manufacture of a certain part was valid because of Hoffman's circulation theorem; I told him I thought he was wrong, but I never told him I was Hoffman.

John Cocke, a leader in the development of RISC architecture, compiler optimization and other innovations in computing software and hardware, was the glue that held the different parts of the laboratory together. I remember seeing him shuffle down our aisle, cigarette ashes dropping like snow in his path, to pick Alan Konheim's brain; and Alan responding with an ingenious generating function approach to the problem Cocke proposed. David Sayre, who had a dual career both in programming (he was, for example, in the original Fortran team), and in crystallography (where he had an international reputation) gave me wonderful private lectures on the art and science of imaging small objects. Similarly, Dick Toupin, whose knowledge of mechanics, electricity and magnetism was awesome, taught me how magnetic inks could be used in printing, the rigorous basis of Saint Venant's principle in mechanics, and other wonders.

Probably my favorite recollection of other people's work was that of Roy Adler and his friends (Coppersmith, Bruce Kitchens, Martin Hassner) creating new sliding block codes for magnetic recording, based on what Roy and Brian Marcus had done years earlier on symbolic dynamical systems. It is a prime example showing that the distinctions between pure and applied mathematics (which had captivated me when I was in high school) are very difficult to sustain in contemporary mathematics. Furthermore, it's a losing game to preach that there is an essential difference, to split

hairs on the distinctions between applied mathematics and applicable mathematics, and so on.

Next, some technical comments about my work. I will try to answer questions about why I do what I do, and how I do it. When I left the Bureau of Standards in 1956, I knew well the vocabulary and issues then current in the use of computers. When I joined IBM in 1961, almost all of that knowledge was obsolete. I would go to meetings where acronyms were confidently exchanged around the table, and I grew weary constantly asking for definitions. This made me a little uncertain about how useful I could be in computing work involving linear programming, and certainly had an impact (I don't know whether for better or worse) on my contribution to the company. Five years earlier, I probably shared with the economist Ragnar Frisch the distinction of having supervised more calculations in that subject than anyone else; and I had presumed that this experience was a principal reason I was hired. Fortunately, Ralph Gomory had such a spectacular grasp of all issues involving the practice and theory of linear programming that I did not feel guilty about my deficiencies. In fact, as Phil Wolfe, Ellis Johnson, and others joined our organization, I always felt that work was in hands more capable than mine, so I did not feel at all guilty as I rotated among other research interests.

What were these other interests? When I began my career, I was happy to swing at any problem anyone threw at me. Maybe I was too gregarious. In fact, when I was acting director of the department, I discovered in my own personnel file(!) a letter of recommendation, written about me before I was hired, which cited my willingness to work on other people's problems and failure to specialize as a possibly negative attribute to complement the compliments in the rest of the letter. But over the years, the combined influences of Straus, Ryser, Dantzig and Taussky made me principally interested in the interplay among ideas in three mathematical subjects: combinatorial mathematics, linear programming and matrix theory. So I have belonged to those three mathematical communities, felt welcome in each, but for many reasons feel most at home with linear programmers.

Did I think of myself as a member of the community of mathematicians, or the community of IBM employees? My locus of loyalty was unequivocally the Department of Mathematical Sciences at the Research Center. I had feelings of kinship with other people in IBM, particularly mathematicians; and I certainly considered mathematicians around the world, particularly in my areas of research, as brothers. But mainly I identified with the Department. I have known some of my colleagues for forty years (Phil Wolfe more than fifty years), many for twenty to thirty years. They have tolerated my idiopathic humming and punning with only the mildest complaints. I have the strongest sentimental attachments to my Department colleagues, past and present, and even to the Department as an institution. We had such a good time! I wish the Department's history were better known and appropriately celebrated.

How do I do my research? Well, each question or problem has its own scenario. Most of my attempts, say 90%, to do something end in failure. The big attraction of mathematics is that it IS hard, and I have learned to live with the experience of making errors and following trails that lead nowhere, and consequently really enjoy the rare occasions when I succeed. Also, I like to keep several problems juggling more or less simultaneously, so I do not linger too long depressed by failure to progress in one of them.

One magic September, all the mathematics on my mind at the time was resolved! I had the ideas for about seven papers on a variety of topics suddenly succeed. Of course I didn't write them up at once. At other times, I have gone for as long as eight months with no success.

Of course, what is most delicious for me is to spend months or years intermittently on a mathematical question, occasionally adding more to my knowledge of what is characteristic of the situation, and finally reach a point where I think I know, or I almost know, the theorem or theorems I want to prove and think I can prove. Then I have a choice of working very very hard to finish the project, or sitting back and savoring the pleasure of anticipating the joy I will have when I finally prove what I believe to be a correct description of the mathematical question and its answers. And accompanying this anticipation, planning already how I will introduce the mathematical issue when I give a lecture about it, and what jokes and anecdotes will enliven the talk. Writing is not as big a pleasure, and I have a backlog of about a dozen papers essentially completed but not yet written in satisfactory form. But I will: doing mathematics and writing papers is what I do. I am also committed to preparing with Uri Rothblum a monograph on aspects of matrix theory inspired by Gersgorin's theorem.

All of my collaborators are my friends, each and every one; you will find their names in my list of publications. But I want to make special mention of: George Dantzig and Olga Taussky, responsible respectively for inspiring my interests in linear programming and in matrix theory; Helmut Wielandt and Ky Fan: when I was a rookie, they invited me to participate in interesting aspects of linear algebra they were creating; Harold Kuhn and David Gale, in fond recollection of the early '50s, when we taught each other to use the ostensibly practical subject of linear programming to prove aesthetic combinatorial theorems that were ostentatiously useless; Dijen Ray-Chaudhuri and Heinz Gröflin, because each developed one of my ideas into much more than I had thought possible; Earl Barnes and Uri Rothblum, because with each I had several times the fun arising when one of us has an insight in some mathematical situation and the other made it much clearer and more general; Ralph Gomory (not only for his creative mathematics: he has the unusual gift of thinking about issues of the practical world with the same creative intensity); and Don Coppersmith, about whom the least I can say is that his talent is incredible, so is his thoughtfulness, and I have come to depend on both.

Two other mathematicians influenced me enormously. Herb Ryser's work showed me what a powerful weapon matrices were for proving combinatorial theorems; in particular, the Bruck–Ryser–Chowla theorem blew my mind. Jaap Seidel and I were simpatico in taste and outlook, and boosted each other in the early days of research on the spectrum of graphs.

I tried to make a more complete list of the mathematicians who influenced my scholarship even if we never published anything (or published very little) together. There were about seventy names already on the list when I was only halfway through my memory bank, so I stopped the effort. The world is not burdened with an excess of mathematicians, and I have known many of them: I started doing mathematics when I was 18, I will soon be 79, and I have had wonderful colleagues. There is no way I can find words to describe my gratitude and feelings of kinship.

This project was created and nurtured by Professor Charles Micchelli of the State University of New York, The University at Albany, who was my colleague

for thirty years at IBM, and by Ms. E. H. Chionh of World Scientific Publishing Company, Singapore. I thank them for their initiative, wisdom and patience.

Greenwich, Connecticut
May 26, 2003