FIVE GOLDEN RULES

Great Theories of 20th-Century Mathematics —and Why They Matter

JOHN L. CASTI

author of *PARADIGMS LOST*

Five Golden Rules

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To Ken Canfield—who asked for it

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Preface

The lifeblood sustaining any field of intellectual endeavor is the infusion of a steady stream of important, unsolved (but in principle solvable) problems. Projective geometry, for example, once a flourishing corner of the mathematical forest, is nowadays about as dead as the dodo bird for the simple reason that the wellspring of good problems ran dry about a hundred years ago. On the other hand, the currently fashionable rage for chaos was totally unknown to all but a few far-sighted adventurers and connoisseurs of the mathematically arcane until the rather recent work of Lorenz, Smale, Feigenbaum, Yorke, May, Rössler, and many others stimulated the outpouring of problems that sustain today's chaologists, their students, and camp followers. These examples illustrate clearly George Polya's well-known dictum that "Mathematics is the art of problem solving." But unlike scientists in other disciplines, mathematicians have a special word for the solution to one of their problems—they call it a *theorem.*

Mathematics is about theorems: how to find them; how to prove them; how to generalize them; how to use them; how to understand them. *Five Golden Rules* is intended to tell the general reader about mathematics by showcasing five of the finest achievements of the mathematician's art in this century. The overall plan of the book is to look at a few of the biggest problems mathematics has solved, how they've been solved and, most importantly, why the solutions matter—and not just to mathematicians. Thus, the goal of *Five Golden Rules* is to enlighten, entertain, and educate by example, rather than by precept.

Stanislaw Ulam once estimated that mathematicians publish more than 200,000 theorems every year. The overwhelming majority of these are completely ignored, and only a tiny fraction come to be understood and believed by any sizable group of mathematicians. Given the fact that mathematics has been practiced on this planet for several millennia, at first sight it seems a daunting prospect to try to single out the "greatest" theorems even of this century from a list that by now must number well into the millions. But the task can soon be cut down to size by the imposition of a small number of conditions, or "filters," separating the great theorems from the pretenders. To pinpoint the five jewels highlighted in this book, here are the criteria I employed:

- *Significance:* Did the theorem break a major logjam in the development of mathematics? Or did the result lead to the establishment of new fields of mathematical enquiry? *Example:* Morse's Theorem, which sparked off the development of singularity theory.
- *Beauty and Scope:* Is the theorem intrinsically "beautiful," in just the same sense that a poem or a painting is beautiful? Does it summarizes compactly a large body of knowledge? And does the theorem shed light on questions over a broad range of areas *inside* mathematics? *Example:* Brouwer's Fixed-Point Theorem, which enables us to establish the existence of solutions to equations under very general mathematical conditions in a wide variety of settings.
- *Applications:* Does the theorem find important applications *outside* mathematics? Do the mathematical structures whose existence the theorem underwrites provide the basis for a more complete understanding of the world of nature and/or humankind? *Example:* The Minimax Theorem, which forms the cornerstone of much of the mathematical work in economics and elsewhere on what it means to say the actions of decisionmakers are "rational."
- *Proof Method:* Did the proof of the theorem require the use of new techniques of logic or modes of reasoning? Could these methods be used to make major inroads on other important problems? *Example:* The Halting Theorem, whose proof focused attention on the idea of using an algorithm to establish mathematical truths.
- *Philosophical Implications:* Does the theorem tell us something important about human beings that we didn't know before? Do the

theorem's conclusions impose major restrictions or, conversely, open up new opportunities for us to obtain deeper insights into what it is we can know about the universe and about ourselves? *Example:* Gödel's Incompleteness Theorem, which imposes limitations upon the ability of the human mind to formalize real-world truths.

In order to qualify for inclusion on our roll call of honor, a theorem would have to score high in most, if not all, of these categories. It doesn't take too much imagination to see that employing these filters quickly whittles down Ulam's universe of millions of theorems to manageable size.

But great theorems do not stand in isolation; they lead to great theories. As indicated above, an important part of the significance of a theorem lies in the theories it contributes either to creating or in some way to nourishing. And for this reason, our focus here is at least as much on great *theories* of twentieth-century mathematics as it is on the great theorems themselves.

A quick glance at the book's contents might lead the reader to ask, Why are the theorems considered here so old? The most recent entry on the list of the Big Five is the Simplex Method, which dates back to around 1947, while the earliest is Brouwer's Fixed-Point Theorem, which was published in 1910. If it's modern, that is, twentieth-century, mathematics we're after, why is there nothing from the work of the past 50 years? This is especially puzzling when by common consensus more significant mathematical work has been done in the latter half of this century than in all previous centuries combined.

This is a pretty reasonable question, so it deserves a carefully considered reply. Basically, the answer resides in the fact that it's really the great *theories* we're after, not the great *theorems.* And great theories in mathematics are like great poems, great paintings, or great literature: it takes time for them to mature and be recognized as being "great." This brings to mind a remark made by Michael Faraday to a British prime minister who was visiting his workshop. When Faraday described his latest discovery in electricity, the distinguished gentleman asked, "What good is it?" Faraday replied, "What good is a newborn baby? You have to wait for it to grow up." And so it is with great theorems, as well. Generally speaking, it seems to take at least a generation or two for a great theorem to "grow up," that is, to be recognized as the seed from which a great theory has subsequently grown. So, what we see as a great theory today almost necessarily had its origin in results dating from the pre–World War II era. And I have no doubt that a similar book written ten years from now will focus on theorems of the 1960s and 1970s, which only now are starting to crystallize in the form of still more great theories. Let me note that sometimes a great theory requires advances in technology, too. For instance, I doubt seriously that two of the great theories treated in this book—optimization theory and the theory of computation—would have appeared in any such volume had it not been for the major advances that have taken place in computing technology over the past few decades.

When the idea for this book first struck me, I queried a number of friends and mathematical scholars as to what they would include in a volume addressing the great theorems and theories of twentieth-century mathematics. Someday I'd like to publish that list, which unfortunately is a bit too long to comfortably include here. But when I had made the final choices, someone asked me why the book was so "impure"; why were all the theories (with the possible exception of topology) in areas that some euphemistically (or pejoratively!) call "applied mathematics" (a term, incidentally, that I abhor). Why is there nothing here that might be termed "pure" mathematics? The reasons are twofold: (a) we're all prisoners of our tastes and background, and mathematically speaking, mine lean in the applied directions, and (b) I wanted the book to focus on why ordinary people (that is, nonacademics) should care about mathematics as a factor in their daily lives, a goal that again biased the material to the applied side of the house. I would certainly look forward to a similar book by someone with leanings different from mine, dealing perhaps with great modern results (and theories) such as the Atiyah-Singer Index Theorem (partial differential equations), The Classification Theorem for Finite, Simple Groups (group theory) and the Hahn-Banach Theorem (functional analysis). But I don't think I will be the person to write that book.

Since I've claimed that the book is for those who want to know about mathematics and why it matters, I'm at least implicitly saying that this is a book aimed at the nonmathematician. This fact requires some "deconstruction." At the outset, let me say that to write about mathematics using no mathematics *at all* is, in my opinion, a copout, doing a disservice both to an intelligent reader and to the field of mathematics itself. To write such "baby talk" about mathematics requires either treating only topics that lend themselves to drawing pictures, like geometry and fractals, or discussing puzzles involving the properties of numbers, simple probability theory or elementary logic that as often as not miss the excitement—and the content—of where the real mathematical action is taking place. So I have chosen a different, less journalistic—and far more dangerous—route. The path this book has taken was dictated long ago by Einstein, when he stated, "A theory should be as simple as possible—but no simpler." So allow me to try to translate this semicryptic remark into a statement about the reader at whom I am aiming this book.

The target reader for the material presented here will have a background in mathematics that I like to call *sophisticated.* This does *not* mean that he or she actually knows any mathematical techniques or procedures—there is very little by way of technical mathematics in this book (none, actually), but there is a *lot* by way of mathematical concepts, ideas, and chains of reasoning. Moreover, I do not believe in the well-chronicled statement by Stephen Hawking's editor to the effect that every equation in a book cuts its sales in half. My ideal reader won't believe it, either. The odd equation will turn up from time to time in the pages to follow, as will an occasional Greek symbol and even a graph or two. But a reader who cares about learning what mathematicians have achieved—and why it matters—won't be deterred in the least by such formalities. He or she will swat these low-level barriers aside as if they were nothing more than pesky mosquitos. So the book is for anyone who's not afraid to confront real mathematical *ideas*—head-on. Just about anyone who's had a course in high-school algebra or geometry, and who retains at least a modicum of enthusiasm for mathematical ideas fits into this category—even if the details of that long-ago course have faded from memory. It's the ideas and one's willingness to confront them that counts, not the technical details.

What about those who do have a more detailed background in mathematics? If comments on the draft versions of the book are anything to judge by, even many professional mathematicians will find material in the book to interest them. Of course, what they will *not* find is the kind of rigor and detailed proofs that one expects from a mathematical textbook or research monograph. This is not a textbook for "wannabe" mathematicians (although it has worked well as a text in undergraduate liberal arts courses of the "mathematics-for-poets" type). And it certainly is not a research monograph. It's pure exposition. But for those who care to dig deeper into the details of the arguments that are only sketched in broad outline here, I have liberally sprinkled the book's bibliography section with a number of more detailed accounts of each topic at various levels of mathematical difficulty.

As always in putting together a work such as this, information and encouragement from many sources was invaluable. So let me close this already overly long preface by performing that most pleasurable of all tasks associated with the writing of any book, namely, the tipping of my hat to those friends and colleagues who have given so generously of their time on behalf of the book. Ian Stewart, Phil Davis, Don Saari, Martin Shubik, Atlee Jackson, and Greg Chaitin offered their thoughts and wise counsel on both the content and style of the book. In this same regard, I wish to single out for special honors my former teacher, friend, and now colleague, Tom Kyner. His careful reading and comments on virtually every line of the original manuscript materially improved the final version, as well as saved me from several flat-out technical faux pas and other inanities and infelicities. For TEX typesetting consultations, it's always a pleasure to acknowledge Michael Vulis and Berthold Horn. And Jennifer Ballentine of Professional Book Center was a font of wisdom (no pun intended) and advice when it came to matters of book design.

Finally, accolades *magna cum laude* to the book's editor, Emily Loose, who has been a constant source of encouragement and eagle-eyed editing, both of which contributed mightily to a far better final product than I had any right to expect. My thanks to all of the above and my absolutions, as well, for any and all errors that managed somehow to creep into the final product. These, I'm sorry to say, remain solely my responsibility.

JLC
Santa Fe, New Mexico

Contents

CHAPTER

1

The Minimax Theorem

Game Theory

Deadly Games

In everyday conversation, a "game" is often thought of as a mere pastime for schoolchildren, a way to spend their day avoiding homework and piano lessons, perhaps playing instead something like blindman's bluff, tag, or hide-and-seek. But to many adults, the term also conjures up images of ascetic chess players hunched over boards in smoke-filled cafes or captains of industry in equally smoke-filled corporate boardrooms, all desperately seeking strategies that will give them an advantage over their opponent(s). These latter situations, in which the outcome of the game is determined by the strategies employed by the players, form the starting point of what we now term the mathematical *theory of games.* And the essential ingredient making game theory a "theory" rather than a collection of heuristics, rules of thumb, anecdotal evidence, and old wives' tales is the notion of a minimax point, a set of optimal strategies for all players in the game. Let's begin with a very real-world example illustrating the general idea.

In early 1943, the northern half of the island of New Guinea was controlled by the Japanese, while the Allies controlled the southern half. Intelligence reports indicated that the Japanese were assembling a convoy to reinforce their troops on the island. The convoy could take one of two different routes: (1) north of New Britain, where rain and bad visibility were predicted, or (2) south, where the weather was expected to be fair. It was estimated that the trip would take 3 days on either route.

Upon receiving these intelligence estimates, the Supreme Allied Commander, General Douglas MacArthur, ordered General George C. Kenney, commander of the Allied Air Forces in the Southwest Pacific Area, to inflict maximum possible damage on the Japanese convoy. Kenney had the choice of sending the bulk of his reconnaissance aircraft on either the southern or the northern route. His objective was to maximize the expected number of days the convoy could be bombed, so he wanted to have his aircraft find the convoy as quickly as possible. Consequently, Kenney had two choices: (1) use most of his aircraft to search the northern route, or (2) focus his search in the south. The payoff would then be measured by the expected number of days Kenney would have at his disposal to bomb the convoy. The overall situation facing the two commanders can be represented in the "game tree" of Figure 1.1, which summarizes what came to be termed the Battle of the Bismarck Sea.

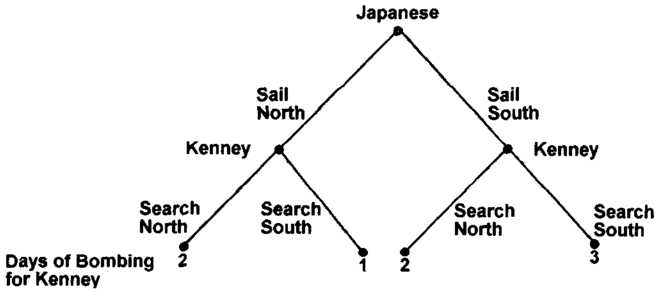

Figure 1.1 Game tree for the Battle of the Bismarck Sea.

(Technically, this kind of game tree is what game theorists call the *extensive form* of the game.)

The diagram should be read in the following way: Starting at the top node, the Japanese commander can choose either the left branch (Sail North) or the right branch (Sail South). Each of these branches leads to a node labeled "Kenney," indicating that these nodes are decision points for General Kenney. The choices for Kenney are now to take the left branch (Search North), or to select the right branch (Search South). After the two commanders have made their choices, the tree "bottoms out" at one of the numbers listed below each of the termination nodes. This number is the days of bombing intelligence estimates claim are available to Kenney if the decisions of the two commanders led to that particular endpoint. In reality, of course, the commanders did not make their choices in the sequential fashion suggested by the diagram. Rather, each chose his course of action independently, without knowledge of what the other was going to do.

It's clear that in making their decisions, General Kenney and the Japanese commander have diametrically opposed interests: What's good for General Kenney is bad for the Japanese commander, and vice versa. Thus, we measure the payoff to the Allies as the number of days of bombing, while we count the "reward" to the Japanese as the negative of this number. So what one side wins, the other side loses. This is an example of what's called a "zero-sum situation," since the payoffs to the two commanders add up to zero.

A more compact way of expressing the overall situation is to use what's termed a *payoff matrix,* which defines the *normal form* of the game. It is shown below for the Battle of the Bismarck Sea. The rows