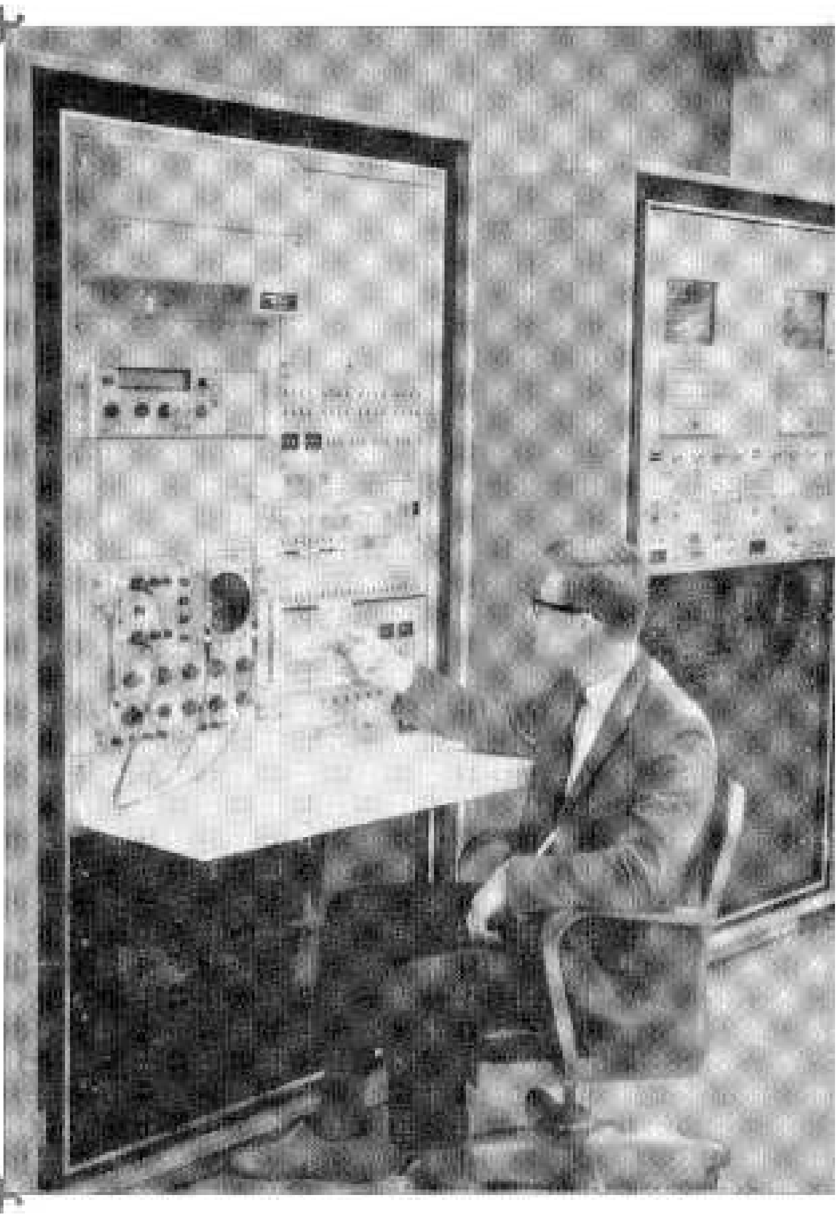
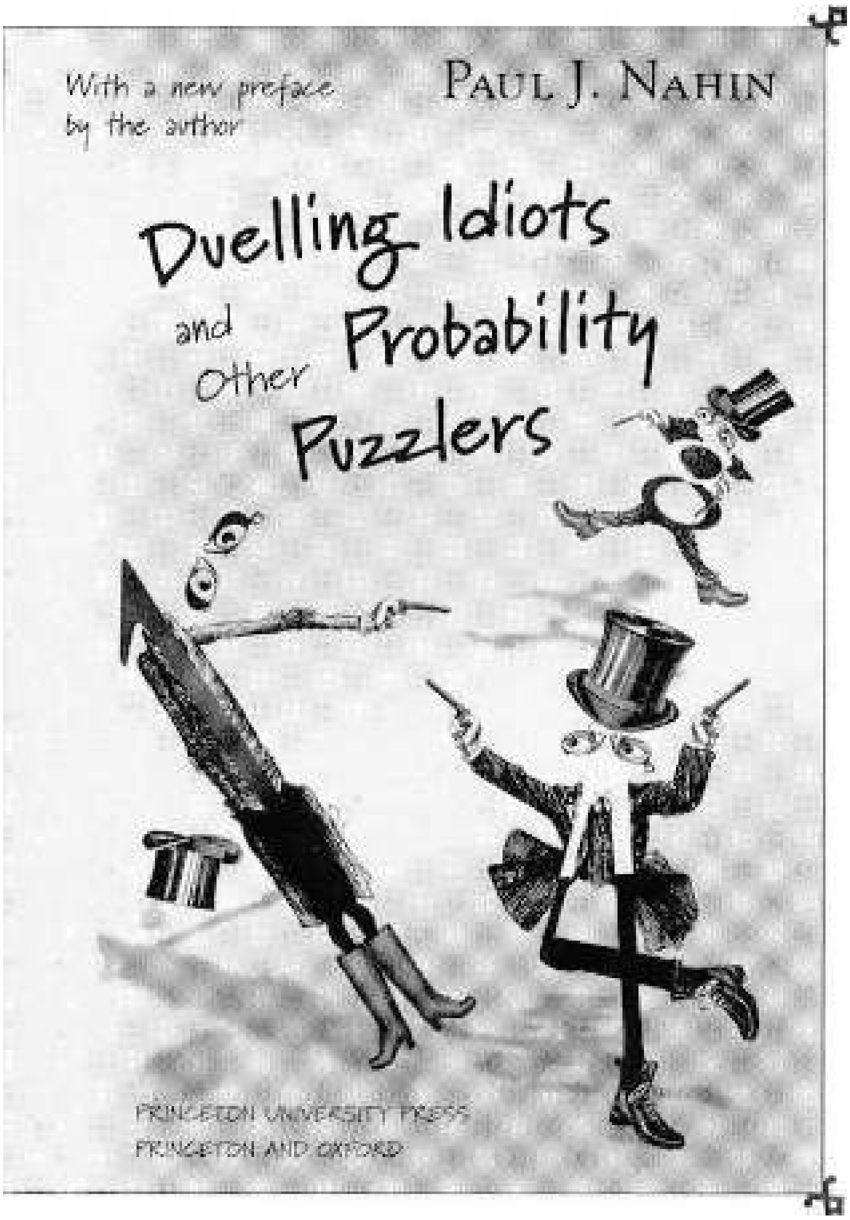
Duelling Idiots and Other Probability Puzzlers

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by the author
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Frontis. The author on May 25, 1964, at age twenty-three, sitting in front of his first completed digital design that wasn't a homework assignment (and it actually worked!). The huge cabinet was purchased by the U.S. Air Force for use as the programmable telemetry signal simulator of the Gemini space capsule. (Gemini was the second phase of the three-phase Mercury/Gemini/Apollo American space program.) Using DTL NAND logic running at a clock speed of one-quarter megahertz, and about 12,000 transistors (a single modern Pentium chip contains millions of transistors), the simulator had a one-half kilobyte magnetic core memory that took two people to lift. Modern operating systems like Windows were only a fantasy in Bill Gates's pre-kindergarten dreams, and the gadget was programmed in machine language. It had less computing power than most [illegible] today. Installed at the Eastern Test Range in Cape Canaveral, Florida, the simulator was built as part of a larger project by the [illegible] Systems Division of the Beckman Instruments Company in Fullerton, California, using the probabilistic management tool called PERT, which is discussed in the final problem of this book.

To Patricia Ann, who, forty-nine years ago, played an "idiots' duel" with me when we married (because, at age 22, who could possibly know what a chance they are taking?). I know I won that game, but she tells me she did, too—which is why I love her.

"I think you're begging the question," said Haydock, "and I can see looming ahead one of those terrible exercises in probability where six men have white hats and six men have black hats and you have to work it out by mathematics how likely it is that the hats will get mixed up and in what proportion. If you start thinking about things like that, you would go round the bend. Let me assure you of that!"

—from *The Mirror Crack'd*, by Agatha Christie (1962)

"The true logic of this world is the calculus of probabilities."

—James Clerk Maxwell (1831–1879), theoretical physicist extraordinaire, in a paraphrase of Pierre Simon de Laplace's (1749–1827) famous assertion in the introduction to his *Théorie Analytique des Probabilités* (various editions, 1812–1825).

"Fate, Time, Occasion, Chance and Change? To these
All things are subject . . . "

—from *Prometheus Unbound* (1820), Act II, Scene 4 (lines 119–120), by Percy Bysshe Shelley (1792–1822)

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Acknowledgments

There are a number of individuals who greatly aided my writing of this book: Nan Collins at the University of New Hampshire, who typed it; Trevor Lipscombe, my editor at Princeton University Press who sent me a contract; my students at the Universities of Virginia and New Hampshire, who worked through many of the problems as homework and exam questions; my wonderful cats Heaviside and Maxwell, who sat, slept, and purred on the working manuscript (and so kept me company) while I was writing it; David Rutledge at Caltech and John Molinder at Harvey Mudd College, who read the book in typescript and gave me the benefit of their comments; Deborah Wenger, who copy-edited the manuscript; Bob Brown at Princeton, who guided the book through final production; and my wife Patricia, who had to listen to me bellow in frustration every time I misplaced a MATLAB file on the hard drive because I am a clumsy Windows user. I thank them all.

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Preface to the Paperback Edition

A book going from hardcover to a corrected paperback printing is a wonderful event for the author of a technical work. What a glorious opportunity to banish embarrassing errors! It is so different for writers of fiction. After all, would you complain if, while reading a novel, you came across a sentence with a repeated word word? Of course not, because the author's meaning is still perfectly clear. But in a book with equations dripping off of every page, just a single misplaced or missing sign, or a brief but awkward turn of phrase when explaining an abstract point, can cause otherwise kind and gentle readers to turn surly. I am proud of this book, but it is not perfect, and a number of e-mails I received from sharp-eyed readers drove that point home. I was lucky because, with a single exception,[1] all were gracious (or pretty much so) communications from one probability enthusiast to another. I saved them all (except for the one), and as I write these words for the paperback edition I am glad I did, even though the stack of e-mail print-outs in front of me is a rude reminder of my missteps.

Elementary copy-editing oversights, such as a lowercase letter that should be uppercase, or a minus sign that morphed into an equal sign, have been quietly corrected with a sigh of relief at their departure from the book. But, you may rightfully be wondering, have all such errors been removed? Of course I hope so, but I doubt it. Probability theory itself suggests there are "probably" even more errors as yet undetected. I must admit I find comfort from that chilling thought in knowing that even writers of great mathematics have also worried over this same issue. George Polya (see p. 62), for example, motivated by his distress over the survivability of printing errors, created a charming little problem, which he posed essentially as follows:

> Two proofreaders, A and B, independently read through the same manuscript and make a list of the errors they find. A's list records *a* errors, while B's list records *b* errors. The two lists record *c* errors in common. Estimate the number of errors that have gone undetected by both A and B.[3]

This problem has an easy solution, involving only arithmetic, but a discouraging answer for perfectionists. See if you can work it out for yourself, but if not, the answer is given at the end of this new preface.[5] Notice that the solution is independent of the length of the manuscript (as long as it is "long enough"). As a specific example to check your work, if $a = 30$ errors, $b = 20$ errors, and $c = 5$ (common) errors, then there are (probably) something like 75(!) errors still lurking in the manuscript. As Krusty the Clown from *The Simpsons* would ask, Is there no relief?

Copy-editing errors are only the start, however. Other issues remain—ones that struck me as vaguely dishonest to simply put right without comment. So, let me now work through the following list of mea culpas.

1. In the final example in part II of the introduction, I erroneously claimed that P ([illegible]) = 0.5839 and P ([illegible])

$= 0.4161$. The correct values of $P\left(\frac{1}{2}\right)$ and $P\left(\frac{2}{3}\right)$ are, in fact,

$$P\left(\frac{1}{2}\right) = 0.000709722\ldots.$$

$$P\left(\frac{2}{3}\right) = 0.000505778\ldots.$$

I was thinking of the probability that $p = \frac{1}{2}$ (or that $p = \frac{2}{3}$), and lost track of what I really wanted to calculate. One correspondent who brought this to my attention (Jorge Aarao, professor of mathematics at Claremont McKenna College, Claremont, California) was kind enough to say "I know what you mean," but others were not so forgiving. They described their mental state after encountering this confusion as having been left "flabbergasted" or "stunned." But they were not, please believe me, as flabbergasted or as stunned as I was. All has now been put right.

2. Sid Kolpas and Steve Marsden, professors of mathematics at Glendale College, California, sent me a copy of their paper "An Unexpected Proof of An Unexpected Occurrence of e" (*The AMATYC Review* 18 [Spring 1997]: 29–33). It bears an uncanny resemblance to Problem 5 in this book, "The Curious Case of the Snowy Birthdays," and although their letter was as friendly as one could ask, I worried that Sid and Steve might have thought I had "borrowed" their work without attribution. So, I immediately wrote back to tell them I had, in fact, gotten my inspiration for the problem from the "rainy Christmas day" problem discussed on page 146 in the textbook Fundamen-

tals of Probability by Saeed Ghahramani (Upper Saddle River, New Jersey: Prentice Hall, 1996). In addition, the problem as I modified it from that discussion involves the evaluation of an n-dimensional integral, which is also in Sid and Steve's paper. But, once again, I had found my guidance for that calculation in an entirely different source: Problem 18 on page 137 and pages 372–373 of the book *Second Year Calculus* by David M. Bressoud (New York: Springer, 1991). Both books pre-date Sid and Steve's paper, which just goes to show (as I wrote them) that a good problem simply cannot go unrepeated. In any case, I am pleased to be able to put matters straight here and to formally cite the Kolpas/Marsden paper as well as the Ghahramani and Bressoud books.

3. An even less likely case of convoluted inspiration is in Problem 14 of this book. After reading it, Lowell T. Van Tassel sent me a copy of his note on random roots from the February 1964 edition of *School Science and Mathematics*, page 159. Lowell, now retired from the math department at San Diego City College, was very gracious in his letter, but, once again, I was concerned over his possible belief I had used his work without attribution. So, I wrote back to tell him that, in fact, my inspiration had been one of the problems in Frederick Mosteller's classic book *Fifty Challenging Problems in Probability with Solutions* (Reading, Massachusetts: Addison-Wesley, 1965). In his book's discussion (p. 81) of the solution to the problem of determining the probability that the quadratic x^2

$+ 2bx + c = 0$ has real roots, Professor Mosteller makes the comment (without proof) that the answer to the same question about $ax^2 + bx + c = 0$ is more difficult to calculate. That is, we cannot simply divide through by a because "$\frac{b}{a}$ and $\frac{c}{a}$ are neither uniform nor independently distributed." The independence part of that remark struck me as non-intuitive, and that was what prompted Problem 24 in my book. So, that's what I wrote back to Lowell. But this story did not end there.

Lowell quickly replied to say that, after reading my note, he was now pretty sure his work was the source of inspiration for Professor Mosteller! As Lowell related the sequence of events from decades ago, he was teaching high school algebra in 1960 when he introduced his students to a computer program for finding the roots to a quadratic equation (see his paper "Digital Computer Programming in High School Classes," *The Mathematics Teacher* 54 [April 1961]: 217–219). The question of how often the program would branch to the "real roots" logic section of the program came up in class, and Lowell decided to find out. He wrote to Professor Mosteller at Harvard, who replied with some helpful comments, and then Lowell solved the problem himself. Hence, the note to *School Science and Mathematics*, which Lowell sent to Professor Mosteller. The next year saw the publication of Professor Mosteller's book, and so I would say the linkage and inspiration are pretty clear. But all this is still not the end of this tale.

Lowell concluded his second letter with the comment that even his own early work was not the origin of the problem of determining the probability of real solutions to a quadratic; he had since found it in the well-known book by the English statistician Maurice G. Kendall, *The Advanced Theory of Statistics*, first published in 1943. I found the third edition, and, sure enough, there it was as Problem 7.6 (p. 195), asking for the probability that $x^2 - ax + b = 0$ has two real roots, if a and b are uniform over the intervals 0 to 6 and 0 to 9, respectively. (I presume a and b are to be taken as independent, as well, even though that is not stated, because the answer is given as $\frac{1}{3}$, which is what I get with that assumption.) At the end of his letter Lowell wrote, "A wild guess on my part is that it, or a related problem, conceivably might have been a British Tripos [he is referring to the infamous brain-smashing math exam at Cambridge, no longer given] problem of years (and years?) earlier." I think Lowell is almost surely correct, and the problem's "real roots" (pun absolutely intended) are in some long-forgotten nineteenth-century English examination.

4. In my discussion on the early history of Monte Carlo in the original preface, while I believe all I say there is correct, there are two errors of omission. During my original bibliographic search I missed an important historical paper by Cuthbert C. Hurd, "A Note on Early Monte Carlo Computations and Scientific Meetings" (*Annals of the History of Computing* 7 [April 1985]: 141–155). That paper includes several photographs of Stanislaw Ulam and John von Neumann, and some commentary on the first formal appearance of the term "Monte Carlo," from a 1949 paper coau-

thored by Ulam and Nicholas Metropolis (1915–1999) in the *Journal of the American Statistical Association*. More importantly, it also reproduces the entire little-known 1947 Los Alamos report (only eight copies were made of this secret report, and it was not declassified until 1959) titled "Statistical Methods in Neutron Diffusion." Coauthored by von Neumann, that report includes a listing of the first Monte Carlo computer simulation (written in pseudo-code) for use on an electronic computer. Von Neumann's program will make you appreciate the beauty of MATLAB! He estimated that following just 100 neutrons, each through 100 collisions, would require a time (including reading, punching, and sorting the paper punchcards of that day) of five *hours* on the ENIAC, then the most advanced computer in the world.

Nicholas Metropolis was, after Ulam and von Neumann, one of the most significant early contributors to the Monte Carlo method. He was one of the reviewers of Hurd's paper, and, almost surely, that motivated him to write, two years later, about his own memories of those pioneering days in "The Beginnings of the Monte Carlo Method" (*Los Alamos Science* 15 [1987: special issue]: 125–130). It is fascinating reading.

5. My remark on page 99, about MATLAB not having a function for evaluating binomial coefficients, is wrong. Professor N. J. Higham, Richardson Professor of Applied Mathematics at the University of Manchester, England, and coauthor of MATLAB Guide (Philadelphia: SIAM, 2000), kindly set me

straight by bringing the function NCHOOSEK to my attention. As he wrote, "[I]t is fairly immune to overflow (your BINOMIAL is a bit dodgy in that respect)." I suspect Professor Higham was just being polite; the MATLAB function is the better choice, and it should replace BINOMIAL in the codes for Problems 6 and 7. It was, by the way, with NCHOOSEK that I evaluated $P\left(\frac{4}{7}\right)$ and $P\left(\frac{3}{7}\right)$ for item (1) in this list. All of the numerical results given on page 100, however, using my BINOMIAL, are the same as you'll get using NCHOOSEK (I tried it). And finally, if you are wondering about NCHOOSEK's odd name, just recall that the binomial coefficient $\binom{N}{K}$ is the number of ways you can choose K things from N things, i.e., "N choose K." Who says the computer scientists at MathWorks don't have a sense of humor!

And finally, my discussion of random number generators at the end of the book is incomplete in one historical detail. I attributed the middle-square algorithm for generating random numbers to von Neumann, but he apparently only rediscovered it. According to Ivar Ekeland's book *The Broken Dice* (Chicago: University of Chicago Press, 1993), the algorithm can be found in a manuscript written during the years 1240 to 1250 by a Franciscan friar (known only as Brother Edvin) living then in a monastery in Norway.

I would like to thank Vickie Kearn, Senior Editor for Mathematics at Princeton University Press, for allowing me the opportunity to include this new material in the paperback edition of the book, as well as for arranging for all of the MATLAB files to be placed on the Press web site

in downloadable form. You can find them at *http://www.pupress.princeton.edu/matlab/duel.zip*.

Durham, New Hampshire
December 2001

Notes and References

1. The lone exception was from a reader who opened his e-mail with the complaint "I can't read the MATLAB programs because some of the statements end with a semi-colon and others don't." I thought he was joking. After all, his assertion, while true, is at the same level as the observation that some of the statements start with the letter f and others don't. When he later claimed, in a rather unpleasant way, that the expression for pi on page 37 is wrong (Leibniz's famous infinite series, which has been known for three centuries and is printed correctly!), I realized he was actually serious, as well as seriously misinformed.

2. "Probabilities in Proofreading," *American Mathematical Monthly* 83 (January 1976): 47.

3. The answer to Polya's problem is [illegible]. The manuscript must be "long enough" to justify using the relative frequency interpretation of probability. For more on the curious pre-Polya history of the proofreading problem, see the editorial commentary on page 801 of the December 1976 issue of *The American Mathematical Monthly*.

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Preface

In 1965, after spending nearly three years as a designer of digital machines in the Systems Division of Beckman Instruments, Inc., in Fullerton, California, my employment suddenly ceased. The sad reason for this was euphemistically called by upper management a "change of business opportunities." That is, the digital machinery product line was immediately terminated for a lack of any new customers and I was, at the age of twenty-five, looking for a new job.

Fortunately for me, Hughes Aircraft Company had its Ground Systems Group (the division that made ground- and ship-based radars) in the same town, and its management was hiring young electrical engineers. So in December 1965, I became a member of the technical staff at Hughes—where I quickly learned that Boolean algebra and sequential switching theory, which had held me in good stead as a digital systems designer at Beckman, simply wasn't going to be enough for long-term survival as a radar systems analyst at Hughes. I needed to learn some more math. I needed to learn probability theory, and I needed to learn it fast.

As astonishing as it is now to look back on those days, in 1962 I had graduated from Stanford University—one of the world's great schools—with a B.S. degree in electrical engineering without having taken a single course in probability theory. It wasn't that I was lazy, as no one in my class of electrical engineers (EEs) took such a course. It simply wasn't required, and my advisor had never brought it up, even as a suggestion, because everybody thought of probability theory as graduate-level course work. And when, in 1963, I graduated with my master's degree from Caltech, which most people consider to be a veritable hothouse of techno-nerds, it had been neither required nor suggested that a first-year graduate student in electrical engineering study probability.

It was only when I started my doctoral studies in electrical engineering at the Irvine campus of the University of California as a Howard Hughes Staff Doctoral Fellow in 1968 that I took a formal probability course in a degree program. But by then I had been at Hughes for nearly three years and had already started such studies myself, for the most practical of reasons: in order to keep my job.

Actually, even while still at Beckman, I had been exposed to a famous probability question, although I hadn't recognized it as such at the time. It was a twice-a-day routine for groups of the engineering staff to take what was jokingly called a "roach-coach" break. That is, several of us (let's say N people) would, morning and afternoon, take ten minutes to walk out to the parking lot and buy donuts and coffee from the visiting lunch van. Rather than each of us individually paying for our own purchases, however, we played a game called "odd-man-out": each of us would simultaneously flip a coin and then show all the others what we had gotten. If it turned out that everybody but one had the same result ($N-1$ heads and one tail, or $N-1$ tails and one head) then the "odd" person paid for everyone. Otherwise we all flipped again, and so on until we got an odd man out.

There are several interesting questions about this game, but one of immediate practical interest concerns how long it will take, as a function of *N*, to get an odd man out. That is, how many flipping attempts (on average) will it take to reach a decision on who pays? And what if one of the coins is biased? You will see in Problem 9 that it is actually quite easy to calculate the answers once we have established some fundamental theoretical results.

I played "odd-man-out" from 1963 to 1965, all through my stay at Beckman (Hughes was a much more formal place, and I never saw anybody play for donuts and coffee during my six years there), but it never even occurred to me that such questions had answers. And if it had I wouldn't have known how to find them. Today, of course, such an admission would be considered tragic. I presently teach sophomore electrical engineering students at the University of New Hampshire the same material that is in this book, which I didn't see until years after getting a Caltech master's degree. So educational times have changed for the better.

Even easier than calculating the answer, however, is using a computer to simulate the physical situation. By the time you have finished this book, you'll have seen a number of examples of just how to do probabilistic simulation. Indeed, a major, two-part thesis of mine that forms the central pedagogical theme of the book is the following:

PART I: *No matter how smart you are, there will always be a problem harder than one you can solve analytically.*

PART II: *If you know how to use a computer application*[1] *like MATLAB,*[2] *you may still be able to solve that "too-hard" problem by simulation.*

I arrived at this thesis the hard way, by direct experience, described in Problem 17. That problem had its origin at