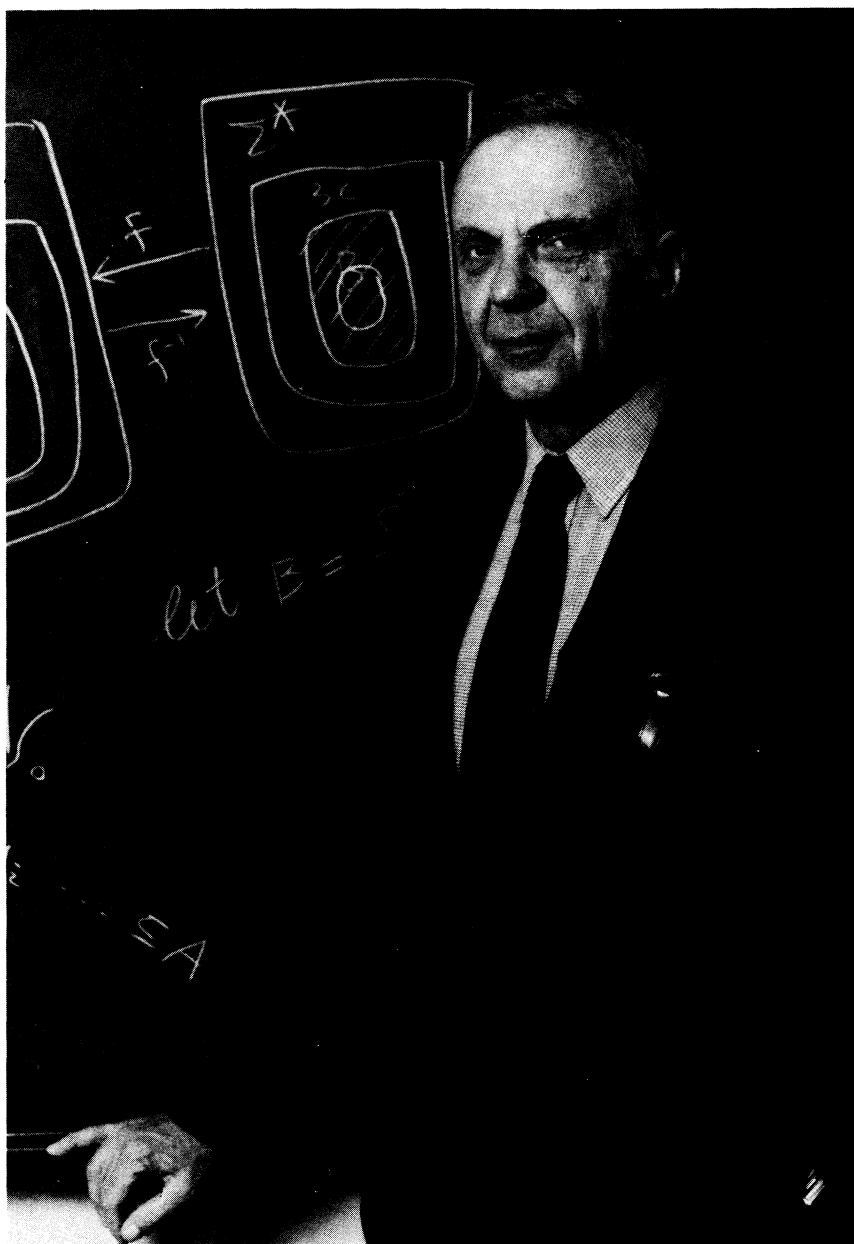

An Interview with Herbert Robbins

by
Warren Page



Herbert Robbins

Herbert Robbins is best known to most mathematicians for his collaboration with Richard Courant in writing the classic *What is Mathematics?*, but in the following forty years, Robbins' work has earned him the reputation as one of the world's leading statisticians. Jerzy Neyman once indicated how rare it was for a professor to place on a high pedestal one of his colleagues—of necessity, one of his “competitors.” And yet, in Neyman's two survey papers on major advances in statistics during the second half of this century, the main breakthroughs cited by Neyman were all obtained by Robbins. (See “Breakthroughs in Statistical Methodologies” following this interview.)

Robbins is the Higgins Professor of Mathematical Statistics at Columbia University, and is known among friends for combining a cynical humor with a deep involvement in humanitarian causes. Among his well-known sayings are:

“No good deed shall go unpunished,”

and (when asked by a university administrator if they might be taking a risk in recommending a junior faculty member for tenure):

“If he washes out in his research, we can always make him a dean.”

In 1978 Jack Kiefer of Cornell wrote that “Robbins (then 63 years old) does not seem to have slowed down at all, and he is as lively and original now as in the past.” That's still true. In 1982, I interviewed him at his home in East Setauket, New York, and it was immediately clear that he loves people and conversation. His broad interests, his many friendships, and meaningful involvements—like good vintage wine—seem to grow fuller and richer with the passing time. All this, of course, made our meeting rewarding and memorable. It also made my job very difficult: trying to confine him to a few specific issues was like trying to pin down an enthusiastic, animated, intellectual octopus. The following is only a sample of what was retrieved from the occasion.

* * *

Page: *You first became nationally known in 1941 as the co-author, with Richard Courant, of “What is Mathematics?”. So let's begin there. “What is Mathematics?” has been translated into several languages, and more than 100,000 copies have been sold thus far. What has made this such a mathematical best seller?*

Robbins: A Russian translation of *What Is Mathematics?* was published shortly after World War II, and it provoked the Russians to produce their own version—some of them call it the Anti-Courant & Robbins—because aspects of Courant and Robbins are not in harmony with certain Soviet mathematical tendencies. This Russian work presumably reflected the correct line on mathematics—its history, the contributions of Russian mathematicians, its current state, its importance, and its Marxist foundations. When I looked at the English translation, for the first time I realized what a good book *What Is Mathematics?* really is. It wasn't written by a committee, and it seemed to have no great practical importance to governments or their defense departments. It was written by two people who collaborated in an intense manner on a subject that concerned them very deeply all their lives. When I started working on *What Is Mathematics?* I was twenty-four, one year out from my

Ph.D. It represented what I had learned about mathematics, what I hoped it would become, and where I wanted to find my place. For Courant, it represented a summation of what he had already done during his rich life as a mathematician and, in his later years, as an administrator and promoter of mathematical institutions. Courant and Robbins spoke for mathematics at a particular time (1939–1941) that can never be repeated. It would be impossible to write that book now; mathematics has changed so much, and the nature of the mathematical enterprise is so different. I would classify *What Is Mathematics?* as more a literary than a scientific work. It belongs to the tradition that started in French intellectual circles when Newton and Leibnitz discovered the calculus. People without formal scientific education wanted to understand this new notion. Salons were held, and philosophers gave lectures to crowned heads and rich bourgeois about the calculus. *What Is Mathematics?* belongs to that tradition of high vulgarization, as the French call it.

Page: *How did a young topologist like yourself come to work with Courant?*

Robbins: (*Laughter*) That's like asking, "What's a nice girl like you doing in a place like this?" Well, I had just earned my Ph.D. and I was beginning a one-year appointment at the Institute for Advanced Study in Princeton as Marston Morse's assistant. I needed a permanent job. When Courant came by looking for someone to work at New York University, Morse suggested me and Courant offered me a job. He had the book in mind, but I didn't know that at the time. When I began teaching at NYU in 1939, I was supporting my mother and young sister. For me, money was *sine qua non*. My salary as an instructor at NYU remained fixed at \$2,500 a year during 1939–1942. That was my sole support; there were no NSF grants then. Some time during the beginning of my first year at NYU, Courant said to me: "I've been given a little money to work up some old course material into a book on mathematics for the general public. Would you like to help me with it? I can pay you \$700–\$800 for your assistance." I was in no position to turn down extra money for a legitimate enterprise, and the idea of communicating my ideas about mathematics to the educated layman appealed to me.

Page: *What was it like working with Courant?*

Robbins: I thought of him as an accomplished and worldly person who, in the fullness of his career, decided to devote some time to explaining to the world what mathematics was, rather than to write another research paper. I felt that I could help him in this. As work on the book progressed, however, the amount of time I had to devote to it became larger and larger, and I soon came to feel that it was interfering with any future career I might have in research. I used to commute regularly to his house in New Rochelle and, in a way, I became a member of the family for a while. In fact, I actually lived nearby for some time so that I could work with him when he wasn't busy. For about two years we worked very closely together exchanging drafts of chapters. But, as you probably know, the whole thing came to an abrupt and grinding halt in a rather dramatic confrontation described in Constance Reid's book *Courant in Göttingen and New York*.

Page: *Did your confrontation with Courant come right after the book had been written?*

Robbins: That's right. As Reid indicated, Courant felt that my collaboration was so helpful that he came to me early in our arrangement to propose joint authorship. He wasn't going to pay me any more, however, because as the joint author I'd probably want to spend even more time on the book. I agreed, since I had already become engrossed in writing the book. My first indication of what was really going on came when I went to the printers, to go over the final page proofs, and the last page I saw was the title page '*What Is Mathematics?* by *Richard Courant*.' This was like being doused with a bucket of ice water. "My God," I thought, "what's going on here? The man's a crook!" By then the book had been written, except that Courant never showed me the preface in which he thanked me for my collaboration. The dedication page to his children was also written without my collaboration.

You mentioned earlier that more than 100,000 copies of *What Is Mathematics?* had been sold. That may be, but when I recently asked Oxford University Press how many copies have been sold they told me I had no right to know. Courant copyrighted the book in his own name without my knowledge. He had a wealthy friend who paid for having the plates made, and he got Oxford to agree to distribute the book. It was a unique arrangement in which he retained the copyright and received a much larger portion of royalties. After this had been done, Courant informed me that he completely controlled the book and he would remit to me, from time to time, a portion of his royalties. And so every year, for a number of years, I used to get a note from Courant saying, "Dear Herbert, enclosed is a check for such and such an amount representing your share of royalties from *What Is Mathematics?*." I never knew how many copies were sold or how much he got, and I still don't. This arrangement continued up to the time of his death a few years ago, when his son Ernest became his legatee.

Page: *What happened then?*

Robbins: Three or four years ago, *What Is Mathematics?* appeared in paperback. Just prior to that, in order to simplify matters, Ernest offered to buy out my share, and I agreed to renounce all further claims on the book if we could set some reasonable figure for my doing so. But he never went through with this, although the sum had been agreed on—and when the book came out in paperback, I stopped getting anything at all. In fact, on the jacket of the hard cover edition (Robbins taking out his edition: copyright renewed, 16th printing, 1977) here you see something about the late Richard Courant and here's something about the present Herbert Robbins. But on the paperback edition, one finds that the mysterious Herbert Robbins appears only on the title page as co-author; on the back cover it looks as though it's entirely Courant's book. So, even after his death, there has been an intensification of the campaign not merely to deny my financial rights in the book, but even to conceal the fact that I was its co-author.

Page: *Were you ever given an explanation why Courant didn't treat you as might have been expected?*

Robbins: Some of Courant's friends came to me and said, "You see, in Europe, it's quite customary for a younger man to do the work while the older man is credited with being the formal author. This has happened before with many people and, in particular, with Courant. Don't be upset, etc." As a non-European, not acquainted with this tradition, I refused. "It wasn't fair! I had taken his word; I wouldn't have put the effort I did into this unless it was going to be a joint book." The drama continued, with more visits by Courant's emissaries, including some distinguished European mathematicians whom he had brought here. But I was adamant and wouldn't agree to be quiet. I threatened to make a fuss if Courant didn't include my name on the title page. Courant finally agreed to do so.

Page: *Were there any mathematicians who gave you guidance and encouragement during critical periods of your professional development?*

Robbins: No. What they gave me was something perhaps more important. The leading mathematicians I encountered made me want to tell them: "You son-of-a-bitch, you think that you're smart and I'm dumb. I'll show you that I can do it too!" It was like being the new kid in the neighborhood. You go out into the street and the first guy you meet walks up to you and knocks you down. Well, that's not exactly guidance or encouragement. But it has an effect.

Page: *Who have been the most impressive mathematicians you've known?*

Robbins: The first mathematician who impressed me was William Fogg Osgood, author of *Funktionentheorie*, because he had a beautiful white beard. I was a freshman at Harvard and, being from a little town, I had never seen anyone like him. I was also impressed by Julian Lowell Coolidge because he spoke with a lisp that sounded very upper class. There are many ways to be impressive. These people impressed me as personae; I thought it must have taken several generations to produce people like them. The first mathematician I met who impressed me as a mathematician was Marston Morse, and I regard him as one of the two or three most powerful mathematicians America has produced. Morse was not a wide-ranging mathematician of the Hilbert type, but he created the theory of the calculus of variations in the large, and whatever he needed he learned, borrowed, or created for himself.

Page: *How did you come to meet Morse?*

Robbins: In the 1931 Harvard–Army football game, Harvard was losing at half-time. During the intermission, Harvard's President A. Lawrence Lowell said to the cadets' commandant: "Your boys may be able to beat us in football, but I'll bet we can beat you in mathematics." The commandant accepted the challenge, and it was agreed that Army and Harvard would have a mathematics competition the following year. Since cadets had only two years of mathematics at West Point, Harvard limited its team membership to sophomores. Lowell's relative, William Lowell Putnam, agreed to put up a prize—the forerunner of today's Putnam Prize in mathematics. In 1931, I was taking freshman calculus. Having just entered Harvard with practically no high school mathematics, I knew calculus would be useful if I ever wanted to study any of the sciences. At the end of my freshman year, much to my surprise I was asked by the mathematics department to join the Harvard math team. Marston Morse was our coach. We met with him on several occasions to

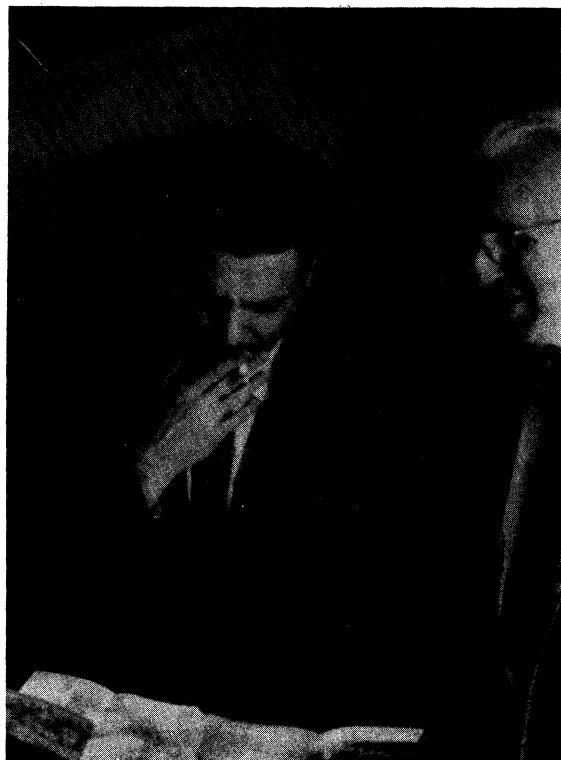
prepare for the competition, and that's how I first met Marston. As it happened, incidentally, Army won that mathematics competition.

Page: *How and when did you make the decision to become a mathematician?*

Robbins: Morse, G. D. Birkhoff, and Whitney were the three mathematicians who most influenced me because I got to know them quite well for short periods of time and, in very different ways, they formed my image of what a mathematician was. Meeting these three early in my education turned my thoughts to mathematics as a possible career.

Page: *I'm sure you also had great teachers in other subjects. What, in particular, did these three convey to you about mathematics?*

Robbins: One of my professors at Harvard, a famous literary critic, used to walk in with a briefcase full of books and lecture on the Romance Poets. He'd take out a book, read a poem, and then comment on it. Now this represented real scholarship that left me totally cold. To my mind, this wasn't being creative. He was talking about what others had done. I would rather have done these things. He talked about Coleridge; I would like to have written "The Rime of the Ancient Mariner." On the other hand, Marston Morse impressed me deeply. Even though what he was talking about meant nothing to me—I didn't know the first thing about the Betti numbers of a complex and the number of critical points of a function defined on it—I could see that he was on fire with creation. There was something going on in his mind of a totally different nature from anything I'd seen before. That's what appealed to me.



H. Robbins and Marston Morse

Page: *Morse seems to have played a pretty prominent role in your life.*

Robbins: At that time, Marston's life was pretty much at low ebb. His wife had left him, and he was living as a bachelor at Harvard. I pitied him almost . . . but in a way, I didn't pity him; I was scared stiff of him intellectually. I was an undergraduate and, although I had never taken a course with him, I got to know him since he was living at the college. One day he said to me, "I'm leaving Harvard and going to the Institute for Advanced Study. You stay here and when you get your Ph.D. in mathematics, come to the Institute to be my assistant." Six years later—I hadn't seen him since—I sent him a telegram: "HAVE PH.D. IN MATHEMATICS." He immediately wired back: "YOU ARE MY ASSISTANT STARTING SEPTEMBER 1." Marston was, in a way, the type of person I would like to have been. He was a father figure to me—my own father died when I was thirteen. Marston and I were about as different as two people could be; we disagreed on practically everything. And yet, there was something that attracted me to Marston that transcended anything I knew. I suppose it was his creative, driving impulse—this feeling that your house could be on fire, but if there was something you had to complete, then you had to keep at it no matter what.

Page: *What was it that originally attracted you to topology?*

Robbins: My affair with topology was rather accidental. Hassler Whitney had come back from a topology conference in Moscow around 1936, and in a talk at Harvard on some of the topics discussed at the conference, he mentioned an unsolved problem that seemed to be important. Since I was then a graduate student looking for a special field to work in—not particularly topology, since I hadn't even taken a course in the subject—I asked Whitney to let me work on it. That's how I got started. I had set myself a time limit from the beginning: if I didn't get my Ph.D. within three years after starting graduate work, I would leave the field of mathematics. Midway through my third year, when they asked me whether I wanted to continue my fellowship for another year, I told them that I wouldn't be coming back next year. Although I did manage to complete my thesis that year, I didn't feel that I had become a topologist; I thought I had become a Ph.D.—a kind of generalized mathematician.

Page: *How did you become a statistician?*

Robbins: My first contact with statistics came when I was teaching at NYU. Courant had invited Willy Feller to give a course in probability and statistics, but at the last minute Feller couldn't come. The course had been advertised, but now there was no one at NYU with any interest in probability or statistics. As the youngest and most defenseless person in the department, I was assigned to teach the course. It must have been a pretty terrible course because I knew nothing about either subject. This was just before I joined the Navy in World War II, not as a mathematician but as a reasonably able-bodied person.

It was in the Navy, in a rather strange way, that my future career in statistics originated. I was reading in a room, close to two naval officers who were discussing the problem of bombing accuracy. In no way could I keep from overhearing their conversation: "We're dropping lots of bombs on an airstrip in order to knock it out,

but the bomb impacts overlap in a random manner, and it doesn't do any good to obliterate the same area seventeen times. Once is enough." They were trying to decide how many bombs were necessary to knock out maybe 90% of an area, taking into account the randomness of impact patterns. The two officers suspected that some research groups working on the problem were probably dropping poker chips on the floor in order to trace them out and measure the total area they covered. Anyway, I finally stopped trying to read and asked myself what really does happen when you do that? Having scribbled something on a piece of paper, I walked over to the officers and offered them a suggestion for attacking the problem. Since I wasn't engaged in war research, they were not empowered to discuss it with me. So I wrote up a short note and sent it off to one of the two officers. In due course, it



Robbins and Courant, 1942

came to the attention of some mathematical research group working on the problem. However, I had no clearance to discuss classified matters, so there was a real communications problem: how were they going to find out my ideas without telling me something I shouldn't know? (What I shouldn't know was, in fact, the Normandy invasion plans.) Well, in some mysterious way, what I had done came to the attention of Marston Morse, and he saw to it that my note reached the right people. Shortly afterward, S. S. Wilks, then editor of the *Annals of Mathematical Statistics*, asked me to referee a paper by Jerzy Neyman and Jacob Bronowski (author of *The Ascent of Man*) on this very same problem. I recommended rejecting their paper as: "a rather unsuccessful attempt at solving a problem that is easily solved if it's done the right way, and here's how to do it." Wilks wrote back that he had to publish the paper because Neyman was one of the authors. But he also wanted me to publish a paper on what I'd written to him. So, after the war in Europe ended, there's an issue of the *Annals* containing the paper by Neyman and Bronowski, followed immediately by my paper which, so to speak, says "Please disregard the preceding paper. Here's the solution to the problem that they can't solve." That was my first publication in the field of statistics. But even then I had no idea that I would become a statistician. What I had been doing was not statistics, but some rather elementary probability theory.

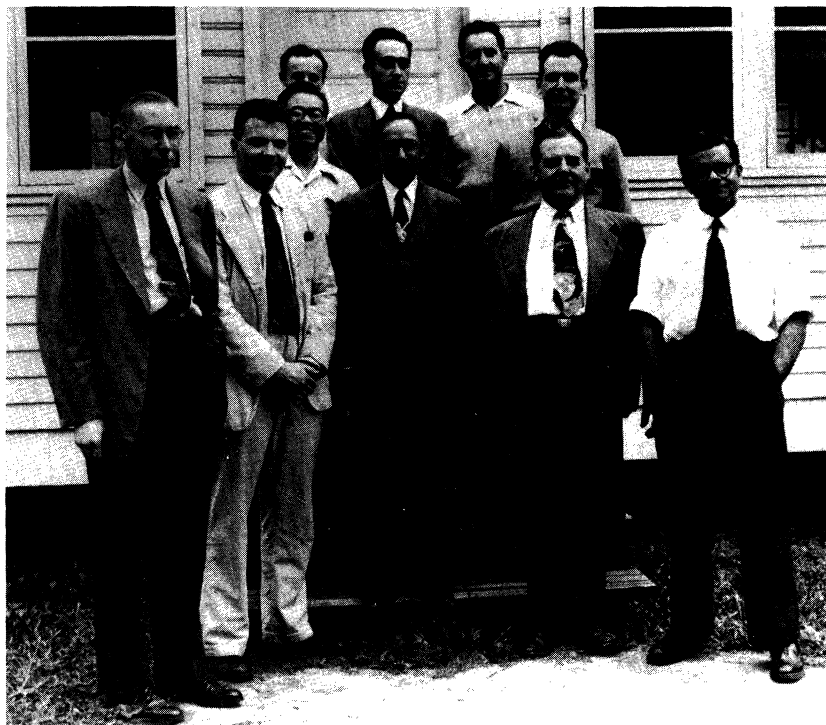


J. Neyman and H. Robbins

Page: *What did you do after four years in the Navy?*

Robbins: I had a career crisis. My pre-war job had been as an instructor at NYU, and I had already burned my bridges there. Jobs were scarce, so with my back pay

from the Navy I bought a farm in Vermont. I went there with my wife—I had gotten married during the war—to figure out what to do next. I thought I was going to leave mathematics and the academic profession completely. Then fate struck again with a telephone call from Harold Hotelling of Columbia University's Economics Department. Hotelling's primary interest was in mathematical statistics. Since Columbia had not allowed him to create a department of mathematical statistics, Hotelling had just accepted an offer to do so at Chapel Hill. The idea of such a department was being promoted at the University of North Carolina by a very energetic statistician, Gertrude Cox. Hotelling offered me an associate professorship in this newly created department. I thought he'd telephoned the wrong Robbins, and I offered to get out my AMS directory to find the Robbins he'd intended to call. Hotelling insisted that there was no mistake, even though I told him that I knew nothing about statistics. He didn't need me as a statistician; he wanted me to teach measure theory, probability, analytic methods, etc. to the department's graduate students. Having read my paper in the *Annals of Mathematical Statistics*, Hotelling felt that I was just the sort of person he was looking for. "Don't question it any further," he insisted. "The salary will be \$5,000 a year." That was in 1946, and it was double my salary at NYU four years earlier. It was a very good salary at that time. So, with some trepidation, I agreed. At Chapel Hill, I attended seminars and got to know several very eminent statisticians. Soon I began to get some idea about what was going on in that subject and finally, at age thirty-two, I became really interested in statistics.



Chapel Hill, 1946

Front row, left to right: W. Hoeffding, H. Robbins, R. C. Bose, H. Hotelling, S. N. Roy



Chapel Hill, 1950

H. Robbins and R. A. Fisher

The Creative Process

Page: *Herman Chernoff characterizes your innovations as having been based mainly on extra-mathematical insight, and Mark Kac describes your contributions as marked by power, great originality, and equally great elegance. Is there anything you can share with us about the creative process—your feelings and experiences—during the germination of some new insight or breakthrough?*

Robbins: I'm always pleased to hear my work praised, but the things I've been associated with that are really important have not been done by me at all. I've merely been the vehicle by which something has done them. When something significant is happening, I have a feeling of being used—my fingers are writing, but there's a lot of noise and it's hard for me to get the message. Most of the time I'm just sitting there, in an almost detached manner, thinking: "Well, here's another day's wastebasket full of paper. Nothing's come through. Maybe another day. Maybe I should stay up tonight and try some more." I stay up nights when my wife and children have gone to sleep. Over and over again I keep working at it, trying to understand something which after months or even years turns out to be so simple that I should have seen it in the first ten minutes. Why does it take so long? Why haven't I done ten times as much as I have? Why do I bother over and over again trying the wrong way when the right way was staring me in the face all the time? I don't know.

Page: *How do you feel after having made a discovery?*

Robbins: I feel like someone who has climbed a little mountain the wrong way. Once I get the message, so to speak, I try to write it up as clearly as possible. Then I want to get away from it. I didn't do it. I don't want to see it again; I had enough trouble with it. I want to push this onto the rest of the world: "Look, there's lots more that has to be done, but don't expect me to do it. I've done my duty. I've contributed to the Community Chest. Now let somebody else carry on."

Page: *The fact is that you reached the summit, you made the discoveries.*

Robbins: Yes, I take some pride in that. Had I not lived, certain things would not have been done. But a world consisting of lots of me's would be intolerable. One is enough.

I always look for something terribly simple, because very simple things are often overlooked. In a way, my strengths are due to my weaknesses. Others are technically much better than I, but it never occurs to them to do the dumb kinds of things that occur to me. A good example is stochastic approximation. Lots of people said: "My God, we can generalize that; we can do it under much weaker restrictions; etc." And I thought, "Yes, that's true. But why didn't somebody do it sixty years ago?"

Page: *Let's stay with the creative process. What do you do when you're blocked or stymied?*

Robbins: There's nothing I can do except try not to get panicky about it. If I live long enough, one of these days I will stop and never get another idea. I have no idea when that will come, but it will come . . . or maybe it has already.

Page: *Why? I want an existence proof.*

Robbins: Look at Einstein. He seems not to have had an inspiration in physics during the last thirty or so years of his life—at least none that could be compared with the great ones he had from 1905 to 1920. Here was a man who had perhaps the greatest intellect that God ever created and, in the last years of his life, nothing much came of it. It wasn't because Einstein was frivolous or dissipating his energy; he had done what he could and there came an end. As I watched him at the Institute one year, he never complained about it and no one mentioned it, but everyone knew that he was essentially finished as a scientist. And Newton? The same thing. From about age thirty on, he did absolutely nothing in science. He had a career as Master of the Mint, he carried on a great deal of activity with friends—controversies over who invented the calculus, and so on—but the last half of his life was totally sterile from a scientific viewpoint.

Page: *Isn't this fear of "drying up" something every researcher and, in fact, every creative person has?*

Robbins: Yes, but it doesn't end here. Take the guy who cracked the genetic code, for instance. "What's he done recently?" In this country, the question is always: "What have you done recently?" . . . "Oh yes, you did such-and-such, but how about last year?" It's not so much that others are asking this question, we're taught here to ask it of ourselves. I constantly find myself asking what have I done during the last year or so, and how does it compare with what I did thirty years ago?

Page: *That sounds like sequential doom. Are you saying that we've come to expect an ever-increasing sequence of better and better encores of ourselves?*

Robbins: Right. And this creates a lot of frustration and anxiety. If I were a promising young tennis player, I'd hope to get better and better, and finally to win at Wimbledon. And then I'd become a teacher—one can't go on forever playing competitively with twenty-year-olds. No one would think less of me if I didn't enter Wimbledon at age 65. But what I'm doing is not tennis. As a mathematician, I'm using my brain, and there's no reason why it shouldn't be as good, if not better, than it was thirty years ago. "So why is it not?" I ask myself. I've been in statistics now for thirty-five years and I would like to try something else. Why don't I try going into molecular biology? Or sociology, or economics? Am I incapable of the mental effort, or am I just too weary? These complicated questions, raised by increasing expectations, I can't answer.

Page: *Although physical prowess—say reaction time—is crucial in sports, it makes no significant difference if one's insight into a mathematics problem takes a second or a year. Perhaps we should consider the mathematician's personal drive to succeed and the price he's willing to pay for success.*

Robbins: Younger mathematicians have a greater desire to become known and make a reputation. This weakens with age, either through frustration if they don't succeed or through satiation if they do. And even if one does make it to the top, was it worthwhile? Is it worth continuing to strive for more? The really successful mathematician, if he's honest, must assess his life in terms of having foregone meaningful relations with others—wife, children, colleagues, friends, etc. As one becomes older, he becomes less likely to want to pay the price for new successes. The theorems that I've proved aren't going to be much good or as comforting to me as would be close friends when I'm old and perhaps infirmed.

Page: *Having been both a topologist and a statistician, have you perceived any difference between those involved in these two fields or, more generally, between those in pure mathematics and those in applied areas?*

Robbins: I don't think I can distinguish any behavioral or personality differences. However, I recall that when I started out, applied mathematicians were looked down on by pure mathematicians. If you got a Ph.D. in mathematics and your professors thought you weren't really very good, then they'd suggest that you would do well to get an actuarial job in an insurance company, or an applied job with an industrial firm. If you weren't a pure mathematician, you weren't top drawer.

I remember a well-known mathematician, alive today, who started out in pure mathematics and then became interested in probability and statistics. While talking with him one day—I was quite young at the time—I asked what he thought was the most important work I might do during the next few years in the field of probability and statistics. To my amazement, he turned red with emotion and almost pleaded: “Robbins, the most important thing you can do is to show mathematicians that probability theory and mathematical statistics are really part of mathematics.” I was absolutely dumbfounded. Evidently his former colleagues had made him feel that he was no longer a member of the elite when he became involved with probability and statistics. He had never been able to survive the blow to his ego that his defection from the realm of pure mathematics had caused.

Page: *Are there feelings of jealousy and/or competitiveness among individuals working on the frontiers of developments in mathematics?*

Robbins: Competitiveness and jealousy seem to belong more to my generation than to the current one. When I was young, there was a great deal of it. Young people now don’t have as much. Although there’s a reasonably well-defined pecking order, I don’t see them motivated by the same burning desire to be Number One and to cast discredit on all their competitors. That was quite common when I was in my 20s and 30s. Maybe because it’s easier now. Mathematics is a way of making a living, like selling insurance. When I started out, to be mathematician was a rare choice: there weren’t many jobs, and one had to be prepared to give up certain things for the enjoyment of doing mathematics. Today, of course, mathematicians work everywhere.

Page: *Is a little bit of competitiveness healthy for those engaged in research?*

Robbins: Competitiveness, as far as I’m concerned, has an ambiguous quality. Sometimes I think that I’m the best in the world, since I’m the only one who looks at things the way I do. So, in this sense, I’m beyond competition. The other feeling is one of total ineptness. There are many fields of mathematics in which I don’t even know the elements. I’ve tried to learn them, but I can’t remember things from one day to the next. These are fields in which I just fall on my face every time I try. I can’t help being anxious about not really knowing what others are talking about. I should know these things because my students have to. Thank God I’m not being examined!

Mathematical Reflections and Projections

Page: *In what directions is the field of statistics evolving?*

Robbins: Let’s take just the field, called biostatistics, that deals with the application of statistical methods to human health and disease. The demand for trained biostatisticians is enormous, but there’s absolutely no supply. If I were given ten

million dollars to spend for advancing science, I could spend it trying to produce one or two good biostatisticians. Statistical methods that are currently being used were mostly developed in England for analyzing such things as agricultural experiments and industrial processes. Many of these techniques are being blindly applied to situations for which they are not adapted. The methodology for handling important problems in biostatistics does not exist. It's just beginning now; its Newton or Einstein has yet to appear.

Page: *How and where can one become trained as a biostatistician?*

Robbins: A mathematically capable student who wants to become directly involved with problems of human welfare, should be doing biostatistics. Unfortunately, there's very little encouragement to do this, and there's very few places now to learn biostatistics. A mathematics department would never think of advising anyone to study it. I would like to see a distinguished mathematics department in this country tell its students: "You are very capable and you could have a career in algebraic geometry, or whatever, but we would like to encourage you to go into biostatistics."

Page: *Since it's so demanding just to keep abreast of one's own field of specialization, is it possible to stay mathematically literate in general?*

Robbins: It's harder and harder. I am not totally illiterate in mathematics. If you're in a university, about the best you can do now is to go to as many seminars and listen to as many one-hour lectures as you can, and just hope that some of it will sink in. But to really keep up with the literature now is impossible. Even when I was a graduate student, in the 1930s, it was just barely possible to have a fairly good idea of most of what was going on. Maybe then somebody could still have said, "Anything that's going on in mathematics is of interest to me and with a little effort, if necessary, I'll read the latest paper on it." No more, it's not possible.

Page: *Is it better to be a mathematical specialist or a generalist, and how difficult is it to be either in a meaningful manner?*

Robbins: That's like asking if it's better to be a decathlon athlete or a high jumper. You do what's best for you. You do what God has given you the wherewithal to do it with. If you are pretty good in a lot of things without being world class in any one of them, you'll find some field or activity which requires exactly that, and nobody else will do it as well. No high jumper could win the decathlon. The person finds the problem. You can't decide what kind of mathematician to be.

Page: *How do today's mathematicians compare with those of earlier generations?*

Robbins: I once enunciated a law of human development: *The total amount of intelligence remains constant while the population increases exponentially.* If you ask who the great mathematicians of the present day are, and how they compare with those of fifty years ago, people will tell you that we've got so many bright people now who can do things which nobody could do fifty years ago. I take that with a

grain of salt. I don't believe we've got all these greatly gifted mathematicians and all these young geniuses. Hilbert is Hilbert, and there won't be another one like him for some time.

Page: *Is there anything society can do to help produce future Hilberts?*

Robbins: There's not much difference between creativity in music and in mathematics. We have not seen, nor been able to create, a modern music that compares with the Baroque, even though millions of dollars are spent annually on music instruction in the high schools, and seventy-eight Americans have won the international competition in this, that, and the other thing. What comes out is pedestrian and not of much interest. During the Sputnik era, the country became concerned with its technical capabilities and we decided to strengthen our scientific establishment. Mathematics became a national priority. Everyone was running around reforming mathematics instruction, creating the new math, rewriting textbooks. More student scholarships and faculty research grants were awarded. There's no doubt that the effect of all that was to produce more mathematics, but I don't know that anything significant came out of it. It may have produced a lot of utility-grade mathematicians who have written lots of mediocre stuff. We viewed the problem in the same sense as our annual output of steel. Maybe we're only number three in the world in annual steel output. Would it do us any good to be number one? Who cares? The point is that nobody knows how to produce a Bach or a Newton.

Page: *There seems to be a greater publish-or-perish pressure today than ever before. Has this resulted in an increased tendency for academicians to jump on new mathematical bandwagons in order to take advantage of greater opportunities for publication?*

Robbins: Journals have proliferated to the extent that there's no real problem in getting published somewhere, although there's still a distinction between publishing in refereed and nonrefereed journals. Many published papers are of no real interest or value. I've often thought that when I become enormously wealthy, I'll establish the very prestigious Herbert Robbins Prize in Mathematics. It would have one condition: the recipient shall never publish another paper. As to mathematicians jumping onto new bandwagons, I believe that most people place self-aggrandizement and obvious rewards ahead of duty to the truth, so to speak. I like to think that when I was young, one did something because that was what one wanted to do, regardless of whether anyone paid for it or listened to it. But this is probably an illusion of age and selective recall.

Page: *Is it easier to become better known in some fields of mathematics than in others?*

Robbins: In number theory, there are a number of classical conjectures—Goldbach's, Fermat's, etc. Anyone who makes a contribution to them gets instant fame because these are such famous problems—even though they seem to be somewhat outside the general domain of mathematics. An affirmative solution of Goldbach's conjecture would have no obvious consequences in any other domain of mathematics, or even in number theory itself. It's just a glorified champion Rubik's Cube puzzle. One could, of course, say: "I proved Goldbach's conjecture and that

makes me the greatest mathematician of our time.” That can be justified in the sense that some very powerful mathematicians have tried and failed. It’s like saying that you’re the first person to climb Mt. Everest. There are many conjectures hanging around—problems that nobody’s been able to prove or disprove—and they represent standing challenges for young mathematicians to try their muscles on.

Page: *But what about new breakthroughs or discoveries not rooted in historical precedent?*

Robbins: I wasn’t able to prove Goldbach’s conjecture, but I did invent empirical Bayes, stochastic approximation, and tests of power one. That’s something like saying, “I failed to win Wimbledon, but I invented a new game called clinker ball, and I was the local club champion when there were only a few others who played it.” Personally, I would rather have done some of the things I’ve done than some of those other things which I haven’t. In a sense, I’m simply saying that I love my wife, and I’d rather have married her than somebody else who might be more famous. To have proved that π and e are transcendental were great accomplishments. These were outstanding problems, and everybody knew that anyone who could solve them would be famous. But, in a sense, nothing much came of it; nothing was created that wasn’t there before. To have created some important new fields that didn’t exist before doesn’t make me a great mathematician, but it does contribute significantly to the general progress of the mathematical sciences.

Page: *So, in mathematics, there seem to be two different types of activities?*

Robbins: Yes. One is to find the answers to problems that have been raised earlier by others, and the other is to create techniques which will then find the problems to which they can be applied.

Page: *Let’s broaden our focus. How are mathematicians regarded by society at large, and how does this compare with the public’s view of physical and social scientists?*

Robbins: The public has a terrible fear of mathematics. I think it’s quite real, and it’s not going to be overcome by restructuring the curriculum or anything else—say, like painless dentistry. The ability and the desire to think abstractly and rigorously is not generally fostered in our society. Most people haven’t the faintest idea of what mathematicians do, how they think, or what they contribute to society. Mathematicians are regarded with a sort of awe that attaches to any scientist—although we’re not really scientists—because we are engaged in a very elusive form of activity.

Page: *How do you feel about being a mathematician?*

Robbins: Let me answer your question this way. Most people acquire a certain expertise, and they work in fields where their expertise can be used. I don’t have any expertise. If I were a Picasso, I could wake up in the morning and say: “Well, I think I’ll paint a Picasso today.” And by the end of the day I would have painted a

real, genuine Picasso. Although it may not be one of my best, it would be another Picasso and it would be discussed by art critics and sold to collectors, and so on. Another day, another painting. Now if I get up in the morning and say, "I think I'll do something in mathematical statistics," at the end of the day I've got a wastebasket full of paper and nothing to show for it. And likewise the next day, and the next. I cannot do something by willing myself to do it, and what I finally produce is usually complete junk. I've probably wasted more paper than any mathematician in the world. I have no idea whether I'll ever do anything worth talking about for the rest of my life. I'm not even like a dentist who comes home and can tell his wife: "Today I did three fillings and two root-canals, and I saved several people from serious tooth decay. Now let's have dinner." What did I do today? I talked to a few people. I tried to think about something and it came to nothing. Finally, I found that I was just repeating what some other researcher had already done. The day's been a total loss.

Page: *Doesn't this place a pretty severe burden on one's self esteem and character?*

Robbins: Most mathematicians are unable to cope with this. I see so many who have stopped working, or are just repeating themselves and basking in former glory. There are so many ways this emotional deprivation can get to you—the fact that you're just looking at the interior of your skull as though you were inside an egg, and there's no world except what you see inside. In most cases, there's no real contact with humanity, history, or culture in general.

Teaching and Learning Mathematics

Page: *Statements have been made to the effect that the good researcher who is a good teacher (undergraduate, or even graduate) is the exception. What has been your experience: are good researchers usually poor teachers?*

Robbins: Good researchers are often poor teachers; bad researchers are almost always poor teachers. The reason that you have poor teachers is that you have poor persons: undeveloped, ignorant, intellectually poverty-stricken individuals who have nothing to offer their students except the subject matter itself. They have no joie de vivre, enthusiasm, or curiosity for learning. They'd be poor in any profession.

Page: *Do you enjoy teaching? What, in particular, do you like and/or dislike about teaching?*

Robbins: I like to think that I'm a teacher by profession; research is what I do for fun. I want to show people what I've seen, that no one else has seen, so that they can share it with me. My teaching is like a man struggling with a bear. You don't

know how it's going to come out, the result is not preordained. But that can be very painful too. Teaching should be like a competition between two antagonists with the outcome really in doubt. And yet you don't want it to be a clumsy job. Things are never settled: every answer raises new questions and begins a new cycle in the subject.

Page: *Have students changed much during your forty-five year teaching career?*

Robbins: There seems to be a regression toward mediocrity: lots of fairly good students, but not as many really bright or as many really dumb students. I don't see many outstanding, dedicated, obsessed, self-motivated freaks. Right now everyone wants to get an MBA, or to get into medical school, or into computer science, or some other highly remunerative graduate field. When I went to college in the 30s, not that many people went on to graduate study to prepare for a job in some special field. If your parents could afford it, or if you were very bright and got a scholarship, you went to college to get an education. I don't want to set myself up as a critic of today's youth, comparing them to a utopia that I envisage in the past. Nevertheless, I have a feeling now of teaching in a trade school; I didn't have the feeling of being a student in a trade school.

Page: *People do learn to learn differently, and today's youth seem to be getting an increasing diet of television, videogames, computers, and other interactive modes of learning. Will future students find classroom lectures uninspiring, and textbook or informational reading unbearably dull?*

Robbins: I have three children who spend a lot of time watching television and damn little time reading books. I don't know if any of them is going to get into college, or what kind of college it will be if they do get in. All I know is that they're a lot different from what I was like. When I was their age, I used to go down to the public library after school, and come home with an armful of books. I'd read them all before the next day—I must have read every book in the library. I don't believe that expertise at computer programming and interactive this, that, and the other thing is any substitute for the written word and the human voice. I don't have a home computer myself, and I'm not anxious for children to learn programming at an early age. I'm still hoping they will learn to read, think, and interact with people rather than machines. Anyone who reads a newspaper will see that parents are now being told that computers are the secret of success. "Send your children to computer school on weekends so that they'll get that edge in the race for success." We could all be replaced by computers, I'm sure. This would be advantageous to the efficiency of computations, but it's not the kind of world that interests me.

Page: *Will any important subjects become much easier or much more difficult to teach in the future because of changing technology, student intellect, or societal values?*

Robbins: Roughly speaking, this is the same as asking what will be the world's record for the 100-meter dash in the year 2500. I think we've gone about as far as we can go (maybe someone can shave half a second or so from the record) unless we mutate into a strikingly different breed. There's just so much energy and so much time for training, and that's it. The subjects of mathematics? I do not see

them, as a result of efforts by some future Bourbaki, becoming simple and within the grasp of young children, so to speak, without painstaking introduction, slow step-by-step increments, and historical approaches. I don't see any reason to believe that there's going to be any great simplification or greater accessibility of mathematical knowledge in the future, no matter what amount of technology, training, or machinery is used. Nobody is going to run 100-meters in five seconds, no matter how much is invested in training and machines. The same can be said about using the brain. The human mind is no different now from what it was five thousand years ago. And when it comes to mathematics, you must realize that this is the human mind at an extreme limit of its capacity.

Page: *What about rapidly expanding scholarly disciplines such as mathematics: will more education and graduate study or training be required of future students who want to begin a meaningful career in mathematics?*

Robbins: I think less is being demanded now than used to be. You don't have to know any foreign languages, for example. If someone can write a doctoral thesis, all other deficiencies will be forgiven, and he'll get his Ph.D. even if he's never taught and has never convinced anybody that he knows the difference between mathematics and computer programming. There's a job market out there eager to swallow up such novitiates. They don't have to earn Ph.D's; an M.A. is fine, even a B.A. in mathematics is fine.

Knowledge and Power

Page: *We know that knowledge is power. But power is also knowledge insofar as prevailing political systems mold and determine what knowledge is created and utilized. Should scientists promote the creation and development of all kinds of knowledge for the sake of knowledge and the enlightenment of mankind, or should there be limitations—external or self-imposed—to the quest for knowledge?*

Robbins: Well, let me mention a remark that J. Robert Oppenheimer once made. As you know, his attitude toward the H-bomb changed from being opposed to being in favor of it. Oppenheimer said that originally he was opposed to the H-bomb because it served no useful purpose. But once a really clever way of making it had been proposed, it was so "sweet"—from the point of view of physics—that it was impossible not to try it. My blood ran cold when I read that. What kind of enterprise were we engaged in when something can be so technologically attractive that, even though it may involve the death of millions of people, a scientist must do it because of its scientific sweetness? One of the things I'm happy about is that I didn't work on nuclear weapons. I know many mathematicians who contributed to producing fission and fusion weapons. I'm glad I didn't. But then, nobody asked me to.

Page: *What about those who teach mathematical techniques that may be used for destructive purposes?*

Robbins: I hope that nothing I do will be used for purposes I don't approve of, but I know perfectly well that it will. It's inevitable. There's nothing I can do about it. As a teacher, I have become increasingly alienated from teaching because it gives me so little opportunity to explain to students that technique is not what it's all about: the desire to prove theorems is not what made me go into mathematics; there's more to life than learning how to get a Ph.D. I'm trying to tell them that the world we live in is not what it should be, and that they should spend most of their time not directly learning techniques, but rather learning what the world ought to be like, and how they should act to help make it so. What worries me is that, in my own student days, I had the benefit of contact with a very small number of people whose lives—not written words—influenced me profoundly. My students know nothing about me outside the classroom. They have no idea of how I live, why I'm doing what I'm doing, or what I think about the world. I feel frustrated. I can't turn my classroom into a pulpit; I'm in the wrong profession for that. But I damn well don't want to teach arc-welding to a bunch of robots who'll go out and arc-weld everything in sight for whoever's paying for it. So, in a sense, I might just as well admit that I'm not all that different from those who worked on nuclear weapons, because I'm teaching techniques to young people without knowing what use they'll make of them, or whether they understand what it's really all about. And I'm afraid they don't in most cases. How can I tell them?

Page: *Do you feel that scientists have a responsibility to become involved in issues of social concern?*

Robbins: Nobody has the responsibility to extend himself into a field beyond his competence. But if you feel this is something that concerns you, it's your duty to become involved. Otherwise, you'll be frustrated and bitter, and the world will be all the poorer. Proving theorems should be permitted to anyone. But if you're not an idiot in the Greek sense of being a private person, then you'll want to talk to others—scientists and nonscientists—about issues that concern everybody. And you will do so. Mathematicians have a very poor track record in this respect; the one exception being their participation in issues of human rights. The proportion of mathematicians defending human rights probably exceeds their proportion in the sciences as a whole. Perhaps I should mention that the chairman of the National Academy of Science's Committee on Human Rights is Lipman Bers. In Russia, many mathematicians try to support the Helsinki Accords.

Page: *To what do you attribute this?*

Robbins: Part of the reason may be that their original concern for issues of human rights led them into careers as mathematicians. People who have a predilection for resisting abusive social policies will tend to prefer activities which are not directly useful to their government's enactment of these policies. In Russia, for example, if you wanted to work in the sciences and not be controlled by the Party apparatus, you'd choose a science as far removed from practicality as possible. Pure mathematics would be a good career choice—in which case, the State would be more likely to place you at an institute not directly concerned with military matters. It seems quite unlikely that algebraic geometry can be used for military or political

purposes. The Soviet Union can afford to allow very capable mathematicians to do pure research since they'll bring credit to the State in an indirect manner. Most other scientists are directly involved with something that can be used by the State.

Page: *Have you ever used your mathematical expertise in matters of civil importance?*

Robbins: During the last ten years, I've become interested in the applications of probability and statistics to legal proceedings. Recent developments now make statistical evidence not only admissible in court, but preponderant in certain cases. In the past, someone would file a discrimination suit as an individual, citing direct anecdotal evidence of being denied fair treatment. Today, however, one files a legal suit as a member of a class, and the evidence is the data on how people are being hired or rewarded as a class. Thus, the evidence is statistical: although no single individual can be said to have been maltreated, the class as a whole may have been found to be treated unfairly.

A really serious problem that emerges is due to the fact our purely scientific statistical apparatus is not really well adapted for legal proceedings: statistical tools created for quality control in the chemical industry must not be misapplied in deciding issues of discrimination. This problem is exacerbated by the use of computer programs, since one can feed numbers into a computer and then interpret the output any way one wishes. Calling something evidence of discrimination doesn't really make it such. Imagine how difficult it must be for a judge and jury to interpret this type of evidence. As a consultant in legal matters, I find over and over again that "evidence"—results based on putting numbers into formulas and computer programs—is being misrepresented and totally perverted by statistical "experts."

New Horizons

Page: *Is there anything you still want to accomplish?*

Robbins: I'd give up my next five papers to write a good string quartet, but I've never been able to. In fact, I don't seem to have any choice in the matter. Sometimes, maybe at 2 a.m., I'm awakened by a feeling of someone knocking: "Hey Herb, you've been to the movies, you've taken your kids to the beach, and you've socialized with the neighbors. Now let's get back to business. You never really figured out what went on in this problem, and you don't even remember where you left it. But I remember, so please get up. We've got some work to do." Finally, I get up and start working, feeling as if I've been Cinderella at the ball and, now that midnight has struck, I've got to go back to cleaning up my mathematical house. After all, that's what I'm for. The last few years of my career have been

unusual—I've actually gone back to working on empirical Bayes and stochastic approximation after a thirty-year hiatus. I feel that I didn't do quite as much as I should have, and no one else has done them justice. Once I give them another push in the right direction, I'll be able to relax and not worry about them.

Page: *Albert Schweitzer once said that the great secret of success is to go through life "as a man who never gets used up." At age 67 and still going strong (more than eighteen publications in the last five years), what's your secret for not getting used up?*

Robbins: The one thing I must express is my great fortune in having found some wonderful young people to collaborate with. They've helped prolong my mathematical work far beyond what it would have been in isolation. Of course I'm not doing pure, abstract, postulate-theorem-proof mathematics. I'm involved with the mathematics that relates to the mysterious and fascinating phenomena of chance that I see around me. I'm trying to create methods for looking at the real world, and I'm better able to do this now than when I was young because I know more about the world and its problems. I'm trying now to solve some mathematical problems that I've been thinking about for fifty years.

Perhaps the real difficulty in answering your question stems from the fact that you're interviewing a sixteen-year old kid who happens to be inhabiting the body of a sixty-seven year old man. You're looking at the body, but I'm afraid you're listening to the kid.

Florence Nightingale 1820–1910

[Of her] Her statistics were more than a study, they were indeed her religion. For her Quetelet was the hero as scientist, and the presentation copy of his *Physique sociale* is annotated by her on every page. Florence Nightingale believed—and in all the actions of her life acted upon that belief—that the administrator could only be successful if he were guided by statistical knowledge. The legislator—to say nothing of the politician—too often failed for want of this knowledge. Nay, she went further; she held that the universe—including human communities—was evolving in accordance with a divine plan; that it was man's business to endeavour to understand this plan and guide his actions in sympathy with it. But to understand God's thoughts, she held we must study statistics, for these are the measure of His purpose. Thus the study of statistics was for her a religious duty.

in Karl Pearson *The Life, letters and labours of Francis Galton*, vol. 2, 1924 (London: Cambridge UP). *Isis* 8 186.