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A Conversation with Z. William Birnbaum

Albert W. Marshall

Abstract. Z. William Birnbaum was born the son of Isaac and Lina Birnbaum on October 18, 1903 in Lwów (also called Lemberg in German), Austria-Hungary. He received a Master of Law degree in 1925 and a Ph.D. in mathematics in 1929 from the University of Lwów, Poland (Lwów, pronounced "Lvo͞ov," became part of Poland after World War I, and then part of Soviet Ukraine after World War II). After two years in Göttingen, Germany, where he did postdoctoral research and obtained an Actuarial Certificate, he became first an actuary for the Phoenix Life Insurance Company in Vienna, and then chief actuary for the Feniks Life Insurance company in Lwów. He emigrated to the United States in 1937 and became a Research Assistant in Biometrics at New York University. In 1939 he was appointed an Assistant Professor of Mathematics at the University of Washington. Apart from visiting professorships at Stanford University, University of Rome, Hebrew University of Jerusalem, and at the University of Paris, he remained at the University of Washington until his retirement as Professor Emeritus of Statistics and Mathematics in 1974. He served as Editor of *The Annals of Mathematical Statistics* from 1967 to 1970, and as President of the Institute of Mathematical Statistics in 1963–64. In 1983, he was awarded the Wilks Medal by the American Statistical Association.

The following conversation took place in Seattle on December 2, 1989.

STUDYING IN POLAND

Marshall: Bill, how did you first become interested in mathematics and the applications of mathematics?

Birnbaum: There is a widely spread opinion that some people are very gifted for mathematics, some are totally nongifted, and of course everything in between. I am thoroughly convinced from experience that many people who are considered nongifted for mathematics are rather the results of nongifted or possibly unqualified teachers of mathematics. Each of us has had experience with children who came home from school and complained bitterly about how they were taught mathematics and we have little doubt it was very poor teaching, that the teacher himself disliked mathematics and conveyed his dislike to his pupils and that was the result. And that may also go back to the way teachers are being prepared for their profession. They quite often are required to take a very minimal amount of courses in the subject matter and simply don't know enough to become interested in the subject, in this case, mathematics, and very often don't know enough to be qualified to teach.

I was very fortunate to have in the last two years at the gymnasium a mathematics teacher who was very enthusiastic. He was, if I remember correctly, a doctoral candidate in mathematics and he deviated from the conventional curriculum to a considerable extent. In addition to the usual program in analytic geometry and trigonometry which he taught very well, he included reports on set theory and topology—fields which were in their beginning at that time. That time was about 1920 and it should be noted that set theory started only in the very early 1900s. The concept of the Lebesgue measure was, I think, the order of magnitude 1904. So those are fields that were very young, and that young mathematics teacher in the gymnasium was all intoxicated by the romantic quality of set theory and topology. The daily drill in using logarithm tables to solve problems in trigonometry was carried out quite well, but it didn't have any of the flavor, any of the adventure, any of the colorful perspectives that that man, that teacher, was able to show us by telling those fairy tales about the different kinds of infinity, about a geometry in which the objects could be bent

Albert W. Marshall is Professor Emeritus of Statistics at the University of British Columbia and Adjunct Professor of Mathematics at Western Washington University. He received his Ph.D. from the University of Washington in 1958 as a student of Z. W. Birnbaum. His mailing address is 2781 West Shore Drive, Lummi Island, Washington 98262.

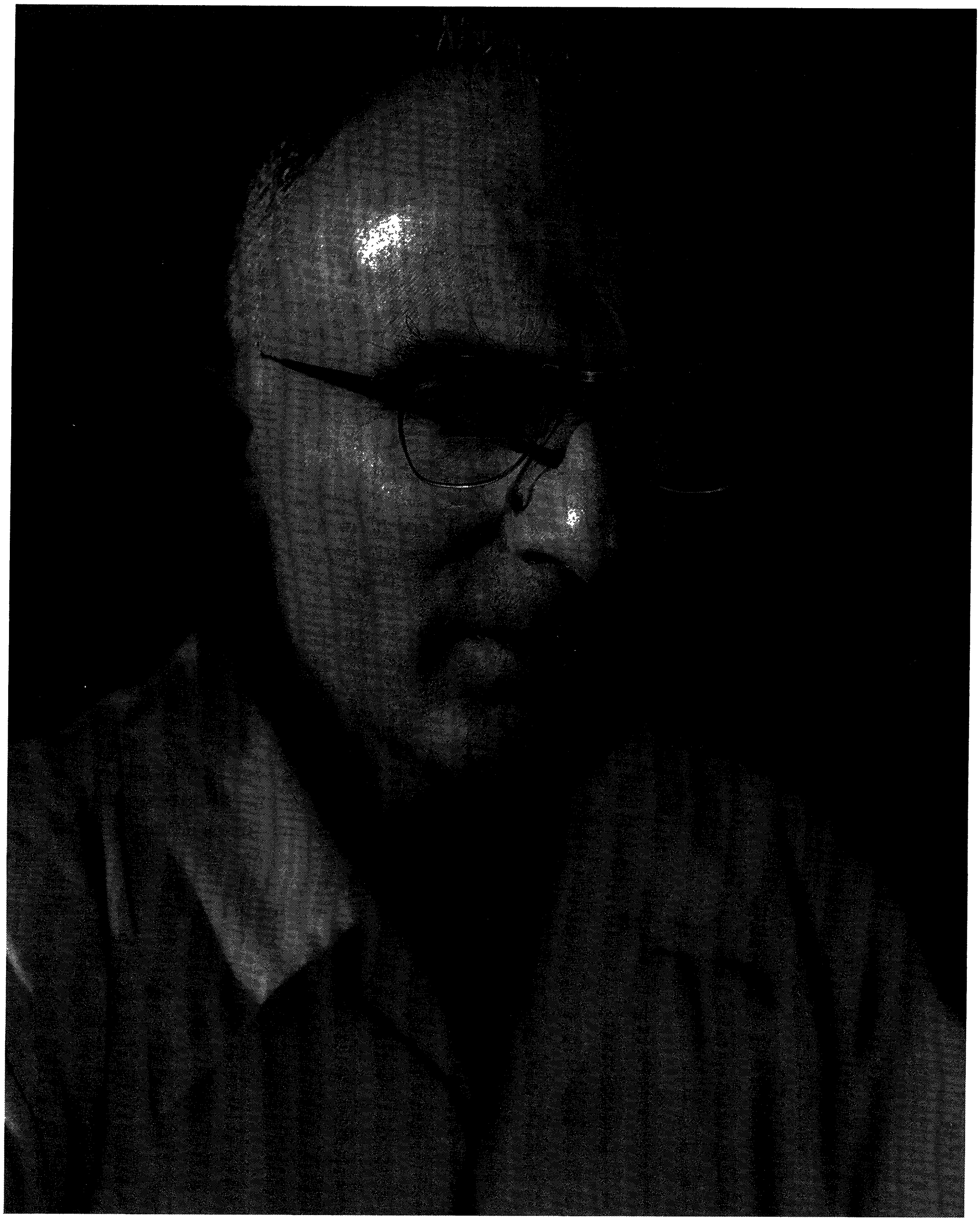

FIG. 1. *Z. W. Birnbaum; photo taken in 1965.*

and deformed and yet retain their character. All that was very exciting, and probably was the beginning of where my interest in mathematics came from.

Marshall: You mention the gymnasium; what was the structure of the school system that you attended?

Birnbaum: The public school system in which I was consisted of two main parts. One was the grade school, which started at age six and went through four years. The other was the gymnasium, which started at age eleven and lasted through eight years. The grade school was for everybody. The gymnasium was already intended for and attended by selected students, selected not so much by ability as by financial means. There were several types of gymnasium. One was the so-called Classical Gymnasium in which emphasis was on Latin and Greek, with some neglect of contemporary foreign languages, with emphasis on humanities. The other type, the other extreme, was the so-called Real Gymnasium which stressed science and contemporary, slightly more practical, subjects. There, only one classical language was taught; that was Latin. That was considered necessary because some students getting out of that gymnasium went into medical school. Others who went into law school needed it because one of the components of legal training was the study of Roman law. So there was only one classical language. But always the emphasis was on mathematics, which was carried through analytic geometry, trigonometry, descriptive geometry (in a pedantic way as a preliminary to possible study of engineering), and sciences—physics and chemistry. Chemistry was carried through to the level of organic chemistry. And usually at least one foreign language. In my case it was English—not that I learned much there.

By the way, I did not go through the usual eight years of the gymnasium, for reasons not quite depending on me. I started gymnasium in my native city of Lwów in the Polish language in 1914. When World War I broke out, my family wound up in the safest possible place they could find—in the capital of the Austro-Hungarian monarchy in Vienna, and we spent almost the entire time, almost the entire four years of the war in Vienna. There, gymnasium courses for Polish speaking refugees were established. I started attending that gymnasium for a while and found myself terribly bored. Material was being covered very slowly for my taste, and I finally convinced my parents that I didn't have to go to school regularly. With the help of a tutor who came to talk with me about an hour a day every day, I could cover the material and then go back to the existing school, take exams, and qualify for promotion to the next grade. In 1918, still before the end of the war, but when the city of Lwów was already liberated from the occupying Russian forces, we moved back to Lwów and I again started attending a public gymnasium. After a while I found myself bored to death. I dropped out, but kept in close touch with what was going on, passed the exams, and in the last year attended school full time. One of the reasons why I did that was this: the gymnasium period of going to school was concluded by taking a comprehensive examination called the *matura*, which was the qualifying exam for university studies. That ran through many subjects and was quite demanding. And there was a belief that unless one had attended school and knew the particular preferences and interests of the teachers in the gymnasium, who then were the examiners in the *matura*, it became very difficult to pass that final exam. So I attended school casually in the seventh grade and full-time in the eighth grade, where I became acquainted with that teacher of mathematics that I mentioned before. To my regret I don't remember his name.

Marshall: So you completed your gymnasium studies, and that would have been time for deciding what to do next.

Birnbaum: As everybody knows, life is a stochastic process where at different moments unpredictable events steer one left, right, or any other direction, and any change at any one of those points could have produced a completely different path. One such ramification point came in 1921 after the *matura*, when I was supposed to spend two or three months' vacation, before the start of academic studies, in the Carpathian Mountains where my father owned a lumber mill. I was to stay there in housing that was meant for the employees of the lumber mill. There was a great belief that things had to be done in a way that was healthy, and spending a vacation in the mountains was considered healthy, so I was supposed to go there. Before going for this vacation, I started shopping for enough books to keep me busy in the rather isolated mountain place. In fact, the location of the saw mill was connected by a narrow-gauge railroad track to the nearest real railroad station and was fairly inaccessible. The requirements I set myself in collecting books that I intended to read—I was a very serious, very ambitious, far from frivolous young man, also with very limited funds—were that those had to be second-hand books, not new books. So I started searching used book stores. They also had to be books that could not be read very fast, so that by taking a few volumes, I would assure myself I would have enough material to keep me busy for the two or more months I was supposed to spend in the mountains. One of my projects was to get a second-hand copy of Immanuel Kant's *Kritik der Reinen Vernunft* (*Critique of Pure Reason*). I had seen samples of that book. It was fascinating, it appealed to me by its rigorous reasoning, and it fitted one of my requirements because the German style of Immanuel Kant was such that when a sentence started on top of one page, it didn't finish until two pages later, where the first period occurred. It was slow reading indeed. I had to read sentences several times to hook them together. The trouble was I didn't find a second-hand copy of *Kritik der Reinen Vernunft*. Instead of that I found several books on mathematics. I don't remember any more the titles of those books, but I recall that one was some kind of a popular introduction to mathematics, and another one was a rather simple-minded introduction to calculus. So those were the slow reading books I took along to the mountains, and that was one of those turning points where the stochastic process directed me into mathematics. Had I found a second-hand copy of Kant's book, I may have become a philosopher.

Marshall: After that vacation, it was time to enter the University?

Birnbaum: After that vacation, the struggle began to enter one of the academic institutions. My original intent was to study medicine. At that time the universities in Poland—because the city of Lwów became part of the Polish republic after 1918—had a *numerus clausus*, a quota system which was intended to limit the number of admissions, particularly of Jewish applicants. I think the number of Jews to be admitted to the first year of medicine was either one or two each year, and I did apply, and was not admitted. My second choice was the Politechnika, which was a separate academic institution which would correspond to the School of Engineering at the third year through masters degree level of the American universities. There again, this system was somewhat different. One was not excluded by ethnic origin or religion, but there was an admission exam, and one of the questions that was decisive for admission was about one's status in military service. It so happened that during the Polish–Ukrainian war—one of those local wars that were fought in 1918—I had not served in the army. I was sixteen years old at that time and that deprived me of one requirement for admission, and I was not admitted to the Politechnika. It was known that admissions were still open to law school and to what was traditionally called the philosophical faculty. The central European universities had four faculties—medicine, law, philosophical faculty (which would correspond roughly to the Arts and Sciences in the American system), and theology. That was a hangover, a left-over of the medieval structure of the universities, but since in some countries—and Poland was one of them—the Catholic religion was by constitution the national religion, the theological faculty was the part of the university that trained Catholic theologians. Not being eligible for the faculty of theology, I had a choice between the philosophical faculty and the school of law. My parents insisted that something practical had to be studied and the faculty of philosophy was not considered anything practical. In particular, mathematics, which at that time I was quite interested in, was obviously one of those skills that did not provide any ways of surviving. So all that was left was law. I applied, was admitted, and started studying law.

I found the study of law fairly interesting, with the whole range from tedious to very, very interesting and tempting, depending on the lecturers, the professors. They ranged from deadwood people who kept reading the old lecture notes year after year after year for decades, to young dynamic people who made their courses quite interesting. But by and large, it somehow didn't fill my time, and with mathematics still being somewhere in my thoughts of what could be done and what is really attractive, I started attending courses in mathematics in the faculty of philosophy. As a matter of fact, it was part of the routine procedure of the university, part of the legal structure, that the student in one faculty could formally enroll in courses in another faculty. And I enrolled in some courses in mathematics. And while studying law, I spent quite a bit of time on mathematics. I became quite well acquainted with some of the faculty members—with mathematicians—and already during the second semester of my being at the university, one of them encouraged me to attend his seminar. And after a very short time I found myself pretty much abreast with people who were full-time students in mathematics and related subjects.

I went through the full course of law school, passed all the exams, finished with the degree of *Magister Utriusque Juris*, Master of Law degree, *utriusque* meaning *both of them*, and *both of them* meant the secular and the divine—the theological, the canonic law. At that time, 1925, I knew enough canonic law to be able to tell something about the marriage law of the Catholic Church and other such problems. I haven't used it ever since. But my law training came in handy in secular law. It came in handy many times.

The next step was to do something practical in the sense of my parents. I joined a law office as a concipient. That was a beginning practicing lawyer. There is no counterpart to it in the American legal profession. I didn't practice very much or very long, but I maintained my status as concipient for several years, and after concluding my law studies, promptly enrolled in the Faculty of Philosophy and from then on studied mathematics full-time.

Marshall: How did you support yourself as a student?

Birnbaum: As far as making a living was concerned, I already, as a law student, got a government fellowship in law. How that happened was, again, rather curious. I attended a seminar—I'm backtracking—I became acquainted with a professor of constitutional law who asked me to attend his seminar. In his seminar I gave a talk on a paper by George Pólya, dealing with voting systems. I don't remember any more what he did—what was in that paper. But having given that report, I became a recognized expert on high-powered mathematical methods in constitutional law. That somehow helped me get the fellowship, so that already, during the last year in law school, I was drawing that fellowship. And when I enrolled in the Faculty of Philosophy in mathematics, I was the equivalent of a fairly advanced student, since I had been participating throughout my law studies, and I received another fellowship that continued there until I finished. I finished my studies in mathematics by two different qualifications. One was a certificate of a teacher on the gymnasium level—that came first. At that time, I gave up the practice of law and got a job teaching mathematics in a gymnasium, first in a privately owned school, then in a public gymnasium. Three years later, in 1929, I received a Ph.D. in mathematics.

Marshall: I would be interested in hearing a little about the mathematical climate around the University of Lwów.

Birnbaum: It seems to be a phenomenon that happened several times in Central Europe in those years, that in places which otherwise were not noted for anything spectacular, there were one, two, or several people who stimulated mathematics to such an extent that there was a litter of a dozen or so creative mathematicians coming out of it every few years. In Lwów, almost immediately after the war, sometime in the very early 1920s, a young mathematician by the name of S. Janiszewski sparkplugged great interest in very contemporary mathematics. And very contemporary mathematics at that time meant, for example, such things as Lebesgue measure, the Lebesgue integral, the theory of orthogonal series, and of course, set theory and topology. There was also another slightly older mathematician whose name is much better known, Hugo Steinhaus. Steinhaus was a graduate of Göttingen in Germany, the recognized capital of mathematics in Europe. A very brilliant man. And before long, there was a cluster of extremely active mathematicians there. One of the best known was Stefan Banach. There were others whose names do not carry the same luster as Banach's, but they were very good, very creative. And in a few years, somewhere between 1925 and World War II, 1939, there was a number of very good people who got their doctoral degrees there, and a steady stream of visitors who lectured, taught, and actively contributed to the mathematical atmosphere in Lwów. Of the people who taught, besides Steinhaus and Banach, who were on the permanent faculty, one may want to mention such people as Stefan Kaczmarz, who with Steinhaus co-authored a classical monograph on orthogonal series. A man by the name of Juliusz P. Schauder became one of the faculty members; he got his Ph.D. in Lwów, and his great achievement was the use of fixed point theorems in function spaces for obtaining existence proofs for partial differential equations. Among visitors who spent sometimes a semester, sometimes a year, as active faculty members in Lwów, were such people as Kazimierz Kuratowski, the topologist, Wacław Sierpinski, a set theoretician, but having done lots of other things, and Stanisław Saks. At meetings or for short periods of time, people who drifted through included John von Neumann who came from the neighbor country of Hungary.

Marshall: These people must have had attracted a number of good students.

Birnbaum: The Ph.D.s produced during that span of lively activity in Lwów included Schauder, Mark Kac, Stanisław Ulam, Władysław Orlicz, Marceli Stark, H. Auerbach, S. Mazur, J. Schreier. And I was a member of that generation. Mathematics in this group of interested people was a kind of a fever. They were getting together at every possible location, at many different times of the day and night, talking mathematics. There was a large tile stove in a place which was a combination of seminar room and miniature mathematics library. That stove had three sides that were accessible and one that leaned against the wall. I remember hours when people stood around that stove in the cold of the night, leaning against those very warm tiles at the three sides that were accessible, and talking around the corner about mathematics. Better known to historians of mathematics are the daily and nightly gatherings of mathematicians in the legendary Scottish cafe, Kawiarnia Szkocka.

The exams certifying a candidate for teaching in a gymnasium consisted of an oral exam and a written exam. The written exam preceded the oral exam. It was monitored by a member of the philosophy faculty in a classroom where usually assembled were about thirty or so candidates, not all of them writing their written exam in the same subject. So there were mathematicians and historians and geographers and zoologists all mixed into that one room. That room was in an old medieval convent/monastery building with walls that were about a yard thick. One of my friends, a younger student who took mathematics only as a minor subject, was to sit for that exam, and he knew that he was not well prepared. He asked me to stand by, in the best way I could think of. So while he was sitting in a classroom with twenty-nine or thirty other people, monitored by the Dean of the philosophy faculty, who was sitting behind his desk discreetly reading a newspaper that covered up his face to such an extent that he couldn't see what was going on in front of him, I was standing in the corridor in front of the door, waiting for something to happen. The door was a single-wing door in the wall that was, as I mentioned before, about a yard deep, so that to walk through, one practically walked through a short tunnel. And the exam was scheduled, I think, for about three hours. I stood at the door and tried to see what was going on. Since it was a three-hour exam, once in a while one of the candidates had to leave, and asked the monitoring Dean's permission to go out, and then the door opened and I could get a glimpse of what was going on. And I saw through the door that my friend was sitting right in line with the door, and the Dean was sitting, as I mentioned before, behind his desk, covered up with his newspaper. I was beginning to lose hope that anything useful would happen, when somebody came out and very quickly gave me a piece of paper. It turned out that the piece of paper was a copy of the problems that my poor friend had to write his exam on. I took that problem sheet to the next desk. It took about twenty minutes to write up the results of the test. Then I stood again in front of the door, hoping for something to happen. I folded that piece of paper very tightly into a small package and waited. And then the door opened and I swung out and threw it directly at my friend. And as the little packet was in mid-air—it had just left my hand—I knew that something catastrophic had happened. The Dean, who had had enough sitting behind his desk, had just stood up, was taking a walk, was in the line of fire, and the little package landed in his beard! That was not the end of the story. My friend flunked the exam. But I was at that time in the process of completing my Ph.D. My dissertation was being reviewed by Banach and Steinhaus, and I was supposed to pass one of the final exams which were known as a *rigorosum*, a rigorous exam, a free roaming exam where examiners could ask questions on anything that came to their minds in the field of mathematics. The date and time for the exam was to be set by the Dean. So I went to the Dean's office and spoke to the clerk who was administering those formalities, and asked to be scheduled for my *rigorosum*. Through the open door the Dean saw me, and called for the clerk. The clerk went in, came out, and said, "The Dean wishes to see you." I walked in. By the way, I didn't give a description of the Dean. He was over six feet tall. He was very substantial in weight, a mountain of a man, a medieval historian, very impressive and very physically imposing figure of a man. Well, I walked in. The Dean was at the far end of the room. I walked the whole distance from the door, and stopped about two yards away from him. He stood, reached in his pocket, took something out, and started unfolding the piece of paper. By the time it was completely unfolded, he held it in front of me and said, "Mr. Birnbaum, do you know what this is?" I knew what it was. I said, "Yes." "Mr. Birnbaum, you are now applying for a *rigorosum* and intend to get your Ph.D. degree?" "Yes." "And you will probably want to teach in the future too?" "Yes." "And you know what happened at the written exam two weeks ago?" "Yes." "And you know that exams have to be conducted fairly and correctly?" "Yes." "You shouldn't have done it." "Yes." "But next time, do it more skillfully." "Yes."

I got my dissertation approved and received my Ph.D. degree in 1929. Meanwhile, I had saved up enough funds to fulfill something I had been dreaming about for a long time—to go to Göttingen. As a matter of fact, the day after I formally went through the ceremony of being "promoted," to a Doctor of Philosophy, I boarded a train and took off for Göttingen.

Marshall: What kind of mathematics had you been doing up to that time?

Birnbaum: I became quite interested in the theory of functions of a complex variable, and my first paper was written somewhere around the time of 1926. It was a very simple remark about Cauchy's formula in the theory of analytic functions. I showed it to Banach. He practically threw me out, telling me, "It's nonsense." The next day he called me in and apologized and asked me to prepare it for print. He had thought it was trivial; it was nontrivial. The dissertation was on conformal mapping, on univalent functions. I also tried my hand, not in the dissertation but in some futile attempts at approaches to Bieberbach's conjecture, and as is well known, that thing has not been resolved until very recently.

Marshall: That does not sound like a man destined to become a statistician.

Birnbaum: No, but my first contact with probability theory goes back to my days in Lwów where I attended a course by Steinhaus. He was using a textbook by Markov, but it was barely an auxiliary tool. The course was a lecture course, very vivid, very stimulating. Steinhaus was a man whose range of interests went anywhere from orthogonal series, to measure theory, through applications to tests of paternity, to tools of measurement and probability; all this was very much within his range of interests. As a matter of fact, he and Mark Kac developed the theory of independent functions, which is a probabilistic chapter of function theory.

VISITING GÖTTINGEN

Marshall: Now, about your time in Göttingen.

Birnbaum: I spent two years in Göttingen, from 1929 to 1931, coming back home to Lwów for vacations. In Göttingen I was mainly in the orbit of Edmund Landau because of my interest in complex variables, but other things became quite absorbing, too. While in Lwów, I had written a paper with W. Orlicz called "Über Approximation im Mittel!" that was published in 1930. A closely related, longer paper called "Über die Verallgemeinerung des Begriffes der zueinander konjugierten Funktionen" was written with Orlicz in Göttingen. These papers have been followed by a sequence of developments; Orlicz himself continued research in that area. For several decades I drifted away from it as I drifted away from very pure mathematics.

I was in Göttingen at my own expense. Those were funds saved from the times when I was teaching in Lwów, and I knew that I could stretch my stay there up to two years. And when it became quite clear that the money was running out, Landau offered me an assistantship, but he also warned me that this was a very risky, very insecure period of time. The Nazis were on the rise. I was a foreigner and a Jew. There may not have been a future to be hoped for, starting as his assistant in Germany. I continued studying, but at that time thought it may be necessary to prepare for a change of profession.

Now Göttingen had an Institute for Insurance Mathematics, a kind of a small department with some degree of autonomy. The director of that institute was Felix Bernstein. Bernstein was a professor of mathematics who had started as a very theoretical, very pure mathematician, somewhere at the beginnings of set theory. He was a co-author of the Bernstein-Cantor Theorem, which was one of the fundamental statements in set theory. Later on quite a few abstract mathematicians had difficulties getting promotions, and when the position of the director of the Institute for Insurance Mathematics became vacant, that was the opportunity for him. He became director of it, and from that time on he became very much interested in and active in probability and statistics. Well, he knew those things before, but they became then the center of his activities. He was active in statistics of genetics, in probabilistic genetics, in which he has to his credit a number of advances. He taught courses in various aspects of actuarial mathematics, which I attended. I participated in his seminars. And that was really my first introduction to mathematical statistics because that was part of the curriculum of actuarial mathematics.

Marshall: What kind of statistics were being discussed and taught?

Birnbaum: The kinds of statistics that were taught by Bernstein in Göttingen were pretty much at the avant-garde of what was known at that time. It was clearly, first of all, the kind of statistics inherited, let's say, from Lexis, but there was also quite a bit of emphasis on what was referred to as the law of small numbers. Reference was made to some faraway innovations in Sweden where something like random processes was being considered to describe the performance of a life insurance company. It was very early work on stochastic processes. The topics considered by Bernstein in his courses dealt with classical statistical concepts, but there were also some premonitions of coming things such as stochastic processes (not mentioned by that name), classification of probability distributions, curves that could be fitted, and specialized courses on actuarial mathematics. The usual material needed for actuarial examinations was taken over from Scottish and British actuaries, quite a bit of it dealing with mechanical procedures such as smoothing of data, quite a bit dealing with interpolating probability distributions to available data with adjustment of existing life distributions to all kinds of exceptional risks, such as risks increased by a diagnosed illness, and then the entire area of computing the mathematical reserves for a portfolio of life insurance policies. Those were the other things I learned in Bernstein's institute. By the time I was out of money and out of Göttingen I had obtained there a diploma as a certified actuary. It had a good German name, *Versicherungsmatematik Diplom.*

WORKING AS AN ACTUARY IN VIENNA

Marshall: So in the mathematical capital of Europe, you made your first real acquaintance with statistics.

Birnbaum: And the next question was how to make a living. I was finally offered a job at the Phoenix Life Insurance Company in Vienna—a large international concern—in their actuarial department. It was a good job because it was very well paid and was considered the beginning of a career. Of course, getting into practice had all the qualities of anybody who went through the academic training for a profession and then finds himself practicing that profession. That applies to a lawyer, a physician, and it applies to an actuary. In spite of my high sounding diploma of an actuary, I did not know lots of things that one can only learn in a hands-on situation. I arrived in Vienna in 1931 and was put under a preceptor in the actuarial department of the company. He was Edward Helly, a man to whom I am very grateful for his advice and his gentle and firm direction. There were two newcomers to the office who found themselves in the same situation, being assigned to Helly. One was Eugene Lukacs, the other was I. We entered the office on the same day, and our paths have crossed since then many times.

There were two people who supervised my work as a beginning actuarial practitioner. One was Helly and the other was a man in the mid-executive level comparable to Helly's in the actuarial department, whose name I don't remember right now. The other was a practitioner. Whenever a problem had to be solved, Helly didn't mind spending several hours trying to develop a theory for how to approach the problem, then prepare something that today would be a numerical program. It was a sequence of steps that could be handled on the hand-operated mechanical calculator, and that ranged from that piece of algebra written down on a piece of paper to a number of steps that led to numerical results. The other man had a different approach, and I went for advice, depending on practically the time of the day, or who was present, to the one or the other, whenever I got stuck. The other one had an approach which was this. Try a couple of numbers, just guesses, grind them out on the hand-operated calculator, and see how reasonable they look. If they don't, change the numbers, try again, and so just by experimenting, find numbers that are obviously a fitting solution to the problem. I mean, it was not even a successive approximation. It was using the relative speed of the calculator. It was at that time, crunching numbers; by trial and error, find something that satisfies the requirements. But Helly impressed me very much by his very careful, thoughtful approach. The first time in approaching a problem, that way took probably longer than the practitioner's, because you sit down and think through a number of steps and develop a piece of algebra. But before the second time, one had an apparatus ready that made it possible to answer a similar problem within a few minutes. Helly was, it is known, a brilliant mathematician. But his academic career was limited. He was Privatdozent at the University of Vienna, and couldn't get any further. It was again the limits imposed by anti-Semitism that kept him from being promoted to what was known as *Außerordentlicher Professor*—extraordinary professor. So as long as he was there, he was Privatdozent and made his living as an actuary.

So here in Vienna, again, something new started. It was not really statistics because the actuarial work, seen from the business side, made very little use of theoretical tools I became acquainted with in Göttingen. It was a matter of dealing with competition, preparing competing offers, evaluating some complex risks. The home office of a large company received enquiries mainly about some of the largest and most complicated cases, often dealing with insuring applicants who were not fully eligible for life insurance. Well, I remember a case of a maharaja—at that time they existed—who applied for a multimillion whatever it was—pounds or dollars or something else—policy. Either he applied, or an insurance agent got hold of him and insisted on selling it to him. It just so happens that the maharaja had all the illnesses on the book. It would have made him ineligible, so that if insured at all, he had to be insured at a very high premium. But the risk was so high that one had to parcel the poor maharaja into small pieces and reinsure those pieces in a dozen different insurance companies. All those things had very little to do with what from that point of view looked like very pure mathematical statistics that I studied in Göttingen.

Something else happened which, again, had very little to do with statistics. I looked at the periodical report of the company and noticed that something strange was happening to the results of setting aside reserves from premiums collected. The actuarial department was in charge of computing premium reserves, that is, setting aside each year for each policy amounts of money needed to provide funds for paying the insured sum when it becomes due, for example when an insured person dies. This is done by a fairly simple combination of discounting future receipts and payments at a given interest rate and computing their mathematical expectations according to accepted mortality tables. With practically all life insurance policies the premium reserve keeps increasing in time, because the time when the insured amount will become payable is getting closer. On the other hand, it should not be increasing by more than the premiums collected each year, because after all, the premium is the source from which one adds to the reserve. Those premium reserves were computed for each geographical/political area of operations of the company—for Czechoslovakia, for Hungary, for Poland, for Austria, at that time for France, for Britain, for Brazil, for India. I don't remember anymore the different countries in which the company was operating. And there were certain areas where the premium reserves were increasing faster than the premiums collected.

I went up to the chief actuary, Professor Berger, who was a professor at the University of Vienna, pointed it out to him, and he said "yes, we know that. We have no explanation for that. Would you like to look into it?" Of course I said "Yes. I don't have the slightest idea what to do, but I will look into it." So he gave me a month off just to try to track it down. And my research at that time consisted in walking up and down and up and down in the home offices of the company and trying to trace the movement of the papers. There were no computers at that time. A piece of paper went somewhere, duplicates were prepared, the duplicates went to different departments. In the end I tracked it down and found out what the errors were that were committed. It was exactly one place where file cards were being put into the wrong box. And that gave me a reputation as a trouble-shooter and promoted my career quite a bit—applied statistics!

Marshall: Did you find time for any theoretical mathematics in Vienna?

Birnbaum: Yes, one of my earliest contacts with probability theory was reflected in a paper I wrote jointly with J. Schreier entitled "Zum Starken Gesetz der Grossen Zahlen,"—"On the Strong Law of Large Numbers," published in 1933. That paper had something to do with my, for lack of a better term, Viennese period. In Vienna there was a very active, brilliant mathematician by the name of Richard von Mises who wrote a textbook on probability and statistics based on his own foundations of probability theory. These foundations of probability theory were based mainly on two axioms, one of which was so formulated that it was difficult to explain. It was called the *Regellosigkeits Axiom*, the Axiom of Lack of Regularity, and it was intended to convey the meaning that a random sequence is a sequence for which it is impossible to set up a system of picking out a subsequence which would not be completely lacking regularity. That's about as clearly as I can state it now, partly because I have already forgotten, partly because I am too lazy to look it up right now. I could. The paper by Schreier and myself was intended to show that starting with Komogorov's concepts of probability ... starting with a completely acceptable measure theoretical interpretation of probability ... one can prove a statement equivalent to that axiom by von Mises. In Vienna I met von Mises and had some conversations with him which usually ended with his refusing to try to explain his axiom any further. We were quite friendly. As a matter of fact, he was much taken by the fact that I told him that I had read his book. He claimed nobody else did.

Later on, the Polish insurance commissioner demanded that an independent company, a subsidiary of the Phoenix in Vienna, be established in Poland with its own chief actuary, and in 1932 I was transferred to Lwów as chief actuary for the Polish company. There I became again active in the group of mathematicians around the University of Lwów. My main occupation, of course, was my office job as an actuary, but I spent some time at the seminars and participating in the activities of the department.

In 1936 two mainly political complications caused the Phoenix to go bankrupt: the rise of Naziism and the—it's still not quite clear to me—financial manipulations in Vienna which emptied out the coffers of the insurance company to stave off the takeover by the Nazis. The Polish company was declared to be in failure, although the finances of the Polish subsidiary, were, it seemed, still all right. The liquidator who was imposed on the failed Polish company by the insurance commissioner rehired me as the chief actuary to handle it. But it was clear that this was a matter of sitting on a branch that I was cutting off myself. So I started thinking of what to do next.

EMIGRATION TO THE UNITED STATES

Marshall: Poland in 1936 was not a healthy place to be.

Birnbaum: A cousin of mine who was editor-in-chief of a very influential Polish newspaper appointed me correspondent in the United States. I received a visitor's visa, and in early June of 1937 I arrived in New York as correspondent, as a reporter for the Polish newspaper I have mentioned. Here a completely new life and new career opened for me. While I was trying to write those reports which I didn't know would ever get published, I also tried to find out what the opportunities would be for me to make a living as a mathematician or as an actuary. I had some savings which I estimated would keep me in groceries for an extended period of time. Living frugally, I thought it might last up to a year. I stayed at the International House on Riverside Drive, which was a Rockefeller-funded transit place for newcomers, and it took some time to get adjusted to the new environment. Meanwhile, quite a few of the people who had known me from Göttingen had wound up in the United States. Richard Courant was in the United States, in New York. I knew that Felix Bernstein was in New York, and then there were a number of younger people whose addresses and names I knew. Outside of New York there was Otto Neugebauer at Brown University, who knew me from Göttingen, and I made contact with him. After a short time he founded the *Mathematical Reviews*, which was to be the counterpart and the continuation of the *Zentralblatt für Matematik*, which also was originally co-founded by Neugebauer in Germany. And I started writing reviews for the *Mathematical Reviews*. That was my first activity of a mathematical nature in the United States. Courant was nice, very helpful, but could not recommend anything that looked like employment at that time.

Bernstein was out of town, but I left messages for him; when he came back he contacted me. He first sounded quite discouraging, then one day I found a message from him asking to come over; he may have something positive to tell me. And he retained me as a research assistant on a project of a statistical nature he was doing. He was at that time Professor of Biometry at New York University. The arrangements were made. My salary was to be twenty dollars a week. My obligations were to work on the research projects, and to attend his lectures and make some notes of them, altogether taking five hours a day. To get some order of magnitude of the cost of living, a full breakfast in the neighborhood cafeteria consisting of fruit juice or prunes, hot or dry cereal, two eggs and bacon, toast and coffee, was twenty-five cents.

Marshall: What was the nature of Bernstein's project?

Birnbaum: Bernstein's project at that time was a study of the connection between old age farsightedness, presbyopia and life length. Bernstein had, for a long time, a very strong curiosity about measurable physiological functions in humans that could be used as a yardstick for physiological age. He had arrived at the conclusion that presbyopia, the old age changing in the flexibility of the eye lens, was very strongly correlated with the all-around aging of the organism. It therefore extended some promise of being a predictor for life lengths. Whether that was realistic, I don't know. I suspected at that time that it was not so. But my main work on that research assistantship was to use data that Bernstein had collected and was still collecting for optometrists, trying to find if there is a dependence, by whatever statistical measures, between the age at an individual's death and the changes over a long period of time in the same individual's eye glasses prescription that corresponded with presbyopia. I helped in collecting the data, sorting the data, and evaluating the data, and it took up most of my time. But I also had to attend his lectures, had to make notes, and assist him in tests. It was a combination of a research assistantship and—I don't know—a hybrid position between a grader and a teaching assistant. I sometimes had to substitute for him in lectures as well. I don't think that during that time I learned much, maybe some experience in handling data, but otherwise no particular progress in knowing either the theory or the practice of statistical theory. After an academic year at NYU on that Bernstein project, or on the Bernstein assistantship, I was taken along to the marine biological laboratory at Woods Hole, Massachusetts, where Bernstein had some projects of his own, and I assisted him in that. Then it turned out that Bernstein's grant was terminated and that was the end of that job.

In my spare time already during that first year in New York, I started attending a seminar by Harold Hotelling at Columbia University, and that was, in many ways, a very profitable activity. There I learned quite a bit, and also got acquainted with Hotelling, who offered to be helpful to me in the future.

Marshall: After the Bernstein assistantship ran out, you were without an income?

Birnbaum: Yes, I found myself without an income, and I started a small consulting firm. It was a grandiose undertaking. I rented desk space in an office building on 42nd Street, and became acquainted with a quantitative anthropologist who was willing to work by the hour, if we ever got any project to work on. I equipped that one desk consulting firm with an old, used, electrically operated Monroe calculator, and was ready for business. I placed an advertisement in *Science* magazine, claiming that I was willing to handle statistical projects. And I got a few orders.

I had a few rather strange clients. One of them brought me a number of scatter diagrams and all he wanted was that I compute correlation coefficients and that I fit regression lines. He was somewhat secretive about it, and did not wish to tell me what it was about. I accepted the commission and computed correlation coefficients and fitted regression lines. After a few days he came back and I gave him the results. He held the drawings against the window, compared them with his other drawings, brought me a few more scatter diagrams of two dimensional data, and asked me to do the same for them. That went on for several weeks, with about one visit a week, until he finally came, took the whole stack of data, compared several of them against each other against the light, and said, "It's a great pity, it just doesn't work." It turned out he was trying to invent a system for investing in the stock market. I made up my mind not to accept any more commissions without knowing what it was about.

Marshall: Did you get any other interesting consulting work?

Birnbaum: Besides some minor computations dealing with data for people who were preparing papers for publication, there was nothing major except for one more project. There was an efficiency engineer who hired me on a major project. He claimed that his consulting in efficiency was worth more than a million dollars the last year. It loomed on me only later on that it was not his earnings of one million, but the value of the effect for the client that was about one million dollars. His project was to determine the dependence, computed by correlation coefficients or whatever I wanted, between the schooling—education, public school, private school, high school, undergraduate, graduate study, content and quality of study—and the performance of people in executive positions. I stood that for a couple of weeks and gave up. That was really too much garbage, so I gave up and did not continue on that.

My work in statistics consisted mainly of attending seminars of Hotelling. I also became acquainted for a short time with Sam Wilks at Princeton. I tried to learn as much as I could, and started intensively applying for jobs. It took me that one year of the work for Bernstein and several months of experimenting with that consulting firm to feel sufficiently comfortable in the new environment to muster my courage and start looking for a job—feeling that my language was by now adequate, and that I was not quite a lost stranger in new surroundings. There were some offers directed to me at the incentive of Stephan Warschawski, who at that time was at Cornell. There were some feelers by Professor J. D. Tamarkin at Brown. There were some very unattractive offers of instructorships at some southern universities at salaries of about one thousand dollars for an academic year.

THE UNIVERSITY OF WASHINGTON

When Professor Hotelling called my attention to an opening at the University of Washington, he described to me the location of the university, and thought that it would be quite an attractive position. He warned me not to use him as a reference since he was considered by the Department of Mathematics at the University of Washington as a black sheep. He had his masters degree in mathematics from the University of Washington. They were convinced that the only mathematics worthwhile was pure mathematics. He was a heretic who had left the University of Washington to go to the Stanford Food Research Institute and from then on became a statistician. A reference from him might not be just the right thing. But he did know they were looking for somebody who could teach a course in statistics, because there was a need for a service course for the university which the mathematics department had an obligation to offer. I did get other references. One of them was Albert Einstein, who really didn't know me, but one of his very close associates had known me quite well from the time I was in Vienna. I was introduced to him, had a conversation with him, and he wrote me a good letter of reference. I had a letter of recommendation from Courant, and also a letter of reference from Landau. I finally was given word from the chairman of the department at the University of Washington, Professor Carpenter, that they considered my application favorably, but I would have to be interviewed by somebody personally. They contacted two people, the chief executive officer of the then Sun Oil Company, and the then president of the New School of Social Research. I was interviewed by both of these gentlemen. Later I received a telephone call from Professor Carpenter saying that he had the reports of those, that they were favorable, but that there was a strong recommendation by one of them that I should try to improve my English accent. So Professor Carpenter suggested that I receive an offer of an assistant professorship, but would I please do something ... work on my English so that even undergraduates would understand me.

I bought a car, drove out to Burlington, Vermont, enrolled in courses of English for foreigners, of English composition for foreigners, and a few other things. I spent quite a bit of time swimming in Lake Champlain, practiced my English and some of the New England accent can still be heard on this recording. Professor Carpenter, on one of his trips East, drove by the place where I lived in Burlington. We got acquainted and I got the appointment to an assistant professorship at the University of Washington where I have been ever since, with the exception of certain years of leaves of absence.

Marshall: What did you find upon arrival at the University of Washington?

Birnbaum: At the time that I arrived at the University of Washington, there was only one course in statistics, a course in descriptive statistics. I think it was called Math 13. It was a service course meant for all kinds of fields that needed a minimum of statistics. There was a room called the statistical laboratory in which were housed about a dozen hand-operated heavy calculators, where students were grinding out those things needed in Math 13. But after about a year I started introducing new courses, and it very quickly developed into a full year's curriculum, which covered an introduction to probability and mathematical statistics. After a while, a bachelors degree in statistics was approved. I kept adding more specialized courses. New people joined the nucleus of our statistical group in the Department of Mathematics, and we had a fairly sizable team of mathematical statisticians which at its best days counted ten full-time faculty members. Some time after the war, in 1948, I obtained the first Office of Naval Research contract.

Marshall: What directions had your own interests in statistics taken at that time?

Birnbaum: Before I applied for the contract with the Office of Naval Research, I had been preoccupied with a question dealing with what effect constraints imposed on a random variable have on its distribution. It began with the simple question: if one has a bivariate normal distribution and truncates it in one of the coordinates, what are the changes in the marginal distribution of the other coordinate? This is, for example, the situation when one has an admission test and a performance test and one admits only candidates who score above a certain value in the admission test. How does that influence the probability distribution of their performance measure? That can be carried on with any multivariate distribution. And I proposed those studies among other examples of the programs I always wanted to deal with in my application to the Office of Naval Research.

Before I started working on those problems covered by the ONR contract, I still was very much interested in probabilistic inequalities. One of them, a very short publication entitled "An Inequality for Mill's Ratio" had a strange history. Walking up and down the shores of Lake Chelan, I figured out how to squeeze it onto half a page because it was too unimportant to deserve more. But the printers spread it over two pages. After WW II, when work done by Columbia Statistical Research Group was declassified, several papers by members of that group appeared in which that inequality was used. A somewhat larger paper entitled "A Generalization of Chebyshev's Inequality to Two Dimensions," co-authored with John Raymond and Herbert Zucherman, was presented at a meeting in Berkeley. Pólya was in the audience—and he got in touch with me after I presented the result, and told me, "It's trivial; everybody can see that." And the next day he got in touch with me again and did the same thing that Banach did years ago in Lwów. He really misjudged it. He thought it was far from trivial, it was quite new, would I please publish it, and so on.

Meanwhile, as a result of my work with Bernstein, a paper was published under the sensation-seeking title "Is or Is Not Cancer Dependent on Age?" It appeared in the *Journal for Cancer Research* and it dealt with data assembled under a huge research grant by physicians in one of the research institutions in New York. They were artificially inducing cancer in experimental animals—rats and mice—by planting some kind of larvae that later on produced liver cancer. Their crucial conclusion was somewhat surprising: that cancer of this kind is less likely to occur in older animals than in younger animals. Since that went counter to the accepted belief that cancer is more likely to develop with increasing age, Bernstein asked for the original data. We looked at the publications that resulted from that, and I prepared a study which really was a study in competing risks, except at that time I didn't know such a thing as competing risks existed. The data superficially looked as if cancer was decreasing with age. What really happened was that the agent that was to lead to the cancer was itself a fatal disease. So the animals into whom one planted those larvae were killed off by those larvae before they had time to develop cancer. And that result was published.

The concept of constraints imposed on probability distributions was followed up in several papers. As I mentioned, one paper was on the constraint imposed on one coordinate in a bivariate distribution. One was on the effect of selection performed on some coordinates of a multi-dimensional population, co-authored with Ed Paulson and Fred Andrews. Others were on the effect of linear truncation on a multinormal population, on the effect of the cutting score when selection is performed against a dichotomized criterion, and on optimum selections from multinormal populations. These were usually co-authored with some friends here over five or six years. The concept of peakedness of a probability distribution was introduced in one of my papers of about the same period. I was bothered by the standard measures of how pointed a probability density is, and I proposed something else. Incidentally, that concept keeps cropping up.

Another variety of distortion of a probability distribution by some constraint was studied in several papers jointly with Monroe Sirken. They dealt with the effect on sampling surveys of the factor of nonresponse or nonavailability. Suppose there is a probability distribution one wants to estimate in a population, and one tries to collect data by interviews. If there is a number of nonresponses that affects our knowledge of what we want to know, and if in addition to that the nonresponse is correlated with what we want to estimate, perhaps because people who don't want to talk about it don't respond, then the problem is how does one account for that handicap. Several papers were published on that subject.

Marshall: When did you get interested in distribution-free statistics?

Birnbaum: Sometime around 1950 I became interested in Kolmogorov–Smirnov statistics. At that time, I met Johnny von Neumann at a symposium sponsored by IBM. I told him about the Kolmogorov statistic and told him that Kolmogorov derived an asymptotic distribution for it. The main subject of that symposium of IBM was what von Neumann was deeply involved in—the sequential programmed computer. I proposed to von Neumann the computation of the exact distribution of the Kolmogorov statistic for finite sample sizes. He was very much interested in it, and later I obtained funds which I used to compute that on the Bureau of Standards Western Electronic Computer in the Bureau of Standards Center for Numerical Computation at UCLA. That was published in 1952. An exact closed formula for the distribution of the one-sided Kolmogorov-type statistic was obtained jointly with Fred Tingey, published in 1951. For several reasons, I was still very much interested in distribution-free statistics. I published a paper on the power of the one-sided Kolmogorov–Smirnov test in 1953. During my stay at Stanford, Herman Rubin and I formulated a conceptual definition of distribution-free statistics, and the characterization of distribution-free statistics, which started off several papers by other statisticians. A study of the variance of the Mann–Whitney statistic, co-authored with Orval Klose, followed. Some properties of the one-sided Kolmogoroff-type statistics were studied in a paper jointly with Ronald Pyke.

A somewhat different kind of statistic came about during some of my visits to the Bay Area. A quality-control man from Point Mugu Naval Base came to ask me about a problem which led me to a study of how to estimate the probability $P\{Y < X\}$ where X and Y are independent random variables whose distributions are not known. It's a very old problem, but it was a manufacturing man who came with a very concrete problem. Oversimplified, it looks as follows. One loads a propulsion charge X into a torpedo of terminal strength Y and shoots the torpedo off. Sometimes X is greater than Y, in which case the propulsion charge is so strong that it blows up the torpedo before it can do the damage it's supposed to do. How does one estimate the probability? Clearly not by producing 10,000 torpedoes, shooting them off, and dividing the number of blow-ups by 10,000. It's cheaper to test 10,000 propulsion charges on their own, and 10,000 strengths of the torpedos on their own, and compare them. This work, using some ideas about the Mann–Whitney statistic and Kolmogorov's statistic, led to a master's thesis. So that was about my phase with distribution-free statistics.

Then came the paper you and I wrote on multivariable Chebyshev inequalities. It keeps showing up once in a while in publications, and I find it quoted.

Marshall: As I recall, that was about the time that you started working on questions of reliability.

Birnbaum: Yes, that was a result of something that happened in the outside world. The British started building jet propulsion planes, the famous Comets, for passengers. Two of their planes blew up. They just exploded in the air. It was known from preliminary examination of the aircraft that there was no structural defect, that nothing specific could be said about the cause of failure. A royal commission was appointed, and it duplicated something that a similar royal commission had found exactly fifty years before. Fifty years before, a royal commission was appointed to examine catastrophic railroad accidents where railroad axles failed without any traceable cause. They arrived at the conclusion that the only cause they could think of is that frequent loading and unloading exerted on a piece of metal somehow changed the structure of the metal in a way which can be named "fatigue" of the metal and makes it prone to sudden failure. Almost literally the same conclusion was reached on the catastrophic events of the Comets. The Boeing Company here in Seattle became very interested in studying fatigue. I was drawn into that, and some kind of mathematical model for fatigue failure was prepared. The notion of a "coherent system" was introduced in a paper written with Jim Esary and Sam Saunders in 1961; that paper was the beginning of a long string of papers by various authors. One of those was the joint 1966 paper with you and Esary, in which the notion of increasing hazard rate average was introduced. One very primitive paper that seems to carry consequences is the paper on the importance of components in a system, published in 1968. And as you know, there were several other papers dealing first with fatigue, then with the reliability of multicomponent structures, then with the studies of life distributions of multicomponent structures.

The tendency to try to compute exact distributions for some statistics for which exact distributions were not known before, keeps reverting. There were two papers dealing with exact distributions from some Rényi-type statistics with B. Lienz.

Later on, my acquaintance with the existence of computers, not with the knowledge of how to operate them, led me to the concept of computer-aided statistical tests, and to write some papers on that topic.

So, while there were periods of my interest in inequalities, then in constrained distributions, then in distribution-free statistics, then in life distributions and reliability, then in computer-aided designs, and so on, those periods overlapped.

I also again spent quite a bit of time working with Monroe Sirken of the National Center for Health Statistics. There a paper called "Design of Sample Surveys to Estimate the Prevalence of Rare Diseases," published in 1965, turned out to become a very fruitful paper in a very specialized area. It was followed by many papers modifying the procedures proposed there, generalizing them, and developing a whole body of theory.

One of the recent areas of my interest was the problem of competing risks. I had some new results on the theory of competing risk, some minor new results. I became so interested that I prepared a monograph on it, which was published by the National Center for Health Statistics. That was in 1979.

Another study of interest to the National Center for Health Statistics was a study on infant mortality. Infant mortality has been used very often as an indicator of the health standards of a whole large population. For example, it has been pointed out that infant mortality in the United States is much higher than it is in many other industrialized countries. And the question of how correct the computation of infant mortality is as it is usually carried out came up, and the study of it showed up several serious sources of bias in the computation in the conventional way infant mortality is computed. That, again, has been picked up by various demographers.

Marshall: Bill, in addition to your research, you have done a fair amount of editorial work over the years. Tell me about your term as Editor of *The Annals of Mathematical Statistics*.

Birnbaum: In my experience of editing *The Annals of Mathematical Statistics*, I found that there was too much material for one journal. My parting shot, when ending my tenure in that function, was a strongly worded recommendation that two journals be created to replace *The Annals of Mathematical Statistics*. The recommendation was accepted, and two years later it was implemented by Ingram Olkin, so that now there are *The Annals of Statistics* and *The Annals of Probability*. It is my understanding, just by looking and weighing things in my hands, that each of those journals contains about as much material by itself as *The Annals of Mathematical Statistics* did in the days when I was Editor. My main feeling about being editor was that of relief when I could quit that job. It was time-consuming. Interesting, yes, but quite voluminous. I mean, the results were rather heavy, thick volumes.

Marshall: You participated in the functioning of the Institute of Mathematical Statistics in several ways besides editorial work.

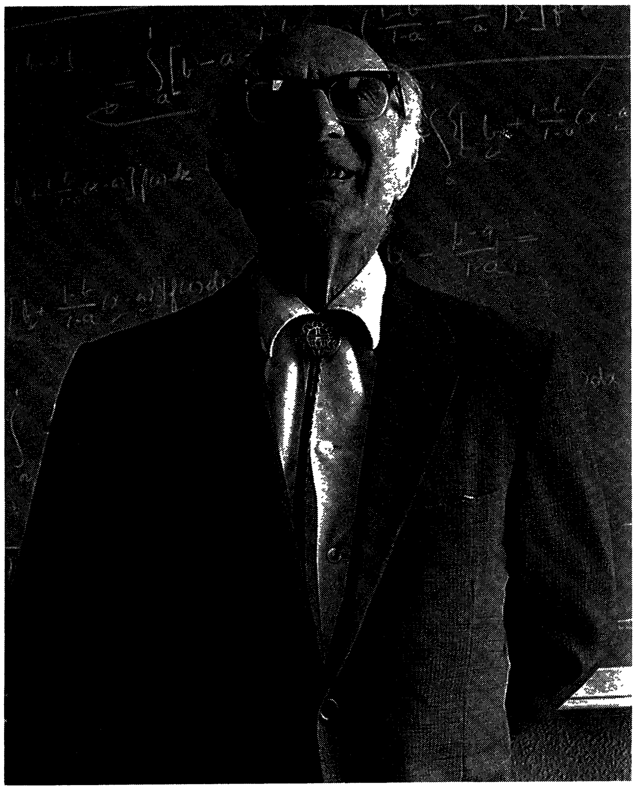

FIG. 2. *Z. W. Birnbaum; photo taken in 1983 in Seattle, Washington.*

Birnbaum: The Institute of Mathematical Statistics, at some time, had to take a stand facing the desegregation in the southern states. There were such shocking episodes as prominent Black statisticians being denied living quarters in dormitories, or not being admitted to meals scheduled as part of the meetings of the Institute of Mathematical Statistics. At one of the annual meetings of the Institute, the question was brought out in a debate. Professor Hotelling, who at that time was already in North Carolina, appealed to those assembled not to press the point too fast. He assured them that civilized people in the Southern states realize how bad the situation is, and how objectionable it is, and what is being done to remedy it. But he noted one cannot legislate morality and this will, he assured us, get improved in due course. I found his optimism, at that time, unrealistic, and tried to urge that some steps be taken. The result was that a committee, euphemistically named the Committee for Physical Facilities for Meetings, was established. As usually happens when one sticks out one's neck, I was named chairman of that committee. For several years that committee functioned so that when a meeting was scheduled, the committee had to explore the physical facilities for that meeting, obtaining assurances that there would be complete desegregation of living quarters, meeting facilities, and eating failities. It explored the segregation or desegregation climate of the place where the meeting was scheduled. The committee had quite a lot of power to enforce its conclusions—to the extent that one meeting was cancelled because we could not obtain sufficient assurances. This seems to have been the beginning of what then led to my being elected President of the Institute. Even during the time of my presidency, that committee still had to function. Fortunately enough, the whole problem is by now moot because this kind of racial segregation does not exist anymore.

Marshall: Bill, what do you like to do when you are not doing statistics or probability or mathematics?

Birnbaum: Well, generally, I like to walk, I like to swim. Now, in younger days I liked to ski, but those days are over. I have done a bit of low-grade, low-level politics on the grass-roots level, serving as precinct committeeman, as delegate to county and state conventions. I never had any ambitions to run for elected office.

In addition to things that I have been doing in my spare time, I have served for a considerable number of years on the board of the American Civil Liberties Union. In 1962–63 I participated in the loyalty oath suit at the University of Washington, and was the only witness whose testimony was cited in the favorable decision of the U.S. Supreme Court.

During my active years at the University of Washington, I served on the College Council, on the Faculty Senate, and chaired for many decades the Faculty Insurance and Retirement Committee. I practically wrote the Washington State statute setting up the faculty retirement system, which did not exist at all at the time when I arrived in Seattle at the University of Washington. So there were many occasions to spend time on something.

In the late 1940's my wife Hilde and I joined a new organization called Group Health Cooperative of Puget Sound, which meanwhile has grown into the largest consumer-owned and consumer-managed health maintenance organization in the United States. Both of us have been very active in it. I served on the original Board of Trustees of the organization. Hilde served later on for twenty-odd years, several years as president of the organization. I served on a number of committees, anything from the research committee, the committee negotiating contracts, the committee that handled appeals, complaints from the membership, as the highest court of appeal. Both of us like to travel. Our first priority is beautiful landscape, good climate, and rest. Our very close next priority is museums. Both of us are very much interested in art museums.

I am now spending a substantial amount of time trying to establish a friendly relationship with a computer, not to mention the fact that that implies also trying to maintain a peaceful co-existence within our ménage à trois, between Hilde, the computer, and myself. There is a certain interference between those three of us.

We are fortunate enough to have both of our children living in Seattle, and we have one grandchild. Between all those things, I think there is very little time left.

Marshall: Those of us who were fortunate to have attended your classes cannot forget your remarkable teaching skills. How did you feel about teaching?

Birnbaum: There are a couple of things I can say that are fairly obvious. One of them is that many things are best learned by teaching, meaning by making an effort before one tries to teach it to understand it, to digest it to the extent that one has few hazy areas left, then to try to explain them to someone else. This is the process of teaching. And during that explaining, during that teaching, those hazy areas sometimes clear up so that one knows them better.

The mandatory retirement, which stopped me from teaching, was quite resented and a regrettable event. I enjoyed teaching very much. I keep in mind the saying of my teacher, Professor Steinhaus, who claimed that one of the greatest joys of a mathematician is to have students who are better than he, and I regret that that was put to a stop. Teaching is a great source of satisfaction. It is something that, in the atmosphere where I grew up, was considered a very noble function. Being a teacher, being a scholar, was considered a very valuable, very worthwhile, way of going through life.

Marshall: Bill, by choosing such a life you have given remarkably to your profession, to your colleagues, and to your many grateful students. We all thank you very much.