HUGO STEINHAUS — A REMINISCENCE AND A TRIBUTE

MARK KAC

1. Hugo D. (for Dyonizy, a name he disliked and seldom used) Steinhaus who died on February 25, 1972, at the age of eighty-five years was unique as a mathematician and as a man.

His long and distinguished service to science and to his country was interrupted only during the years of the Second World War when he was forced to hide from the Nazi barbarians. And even then his sharp restless mind was at work on a multitude of ideas and projects.

He was one of the architects of the school of mathematics which flowered miraculously in Poland between the two wars and it was he who, perhaps more than any other individual, helped to raise Polish mathematics from the ashes to which it had been reduced by the Second World War to the position of new strength and respect which it now occupies.

He was a man of great culture and in the best sense of the word a product of Western civilization.

He was fluent in German, French, and English, knew Latin and Greek, and his knowledge of Polish (the language he truly loved) was as nearly perfect as is humanly possible.

He read prodigiously, had a phenomenal memory (I swear that he knew *Faust* by heart, since he could always be counted on to produce an appropriate quotation), and was intolerant of any kind of sloppiness.

He was convinced that no one could be considered to have been really educated without a thorough knowledge of Latin and was genuinely distressed when he discovered that I did not know Latin at all. The school I went to, the Lycée of Krzemieniec, though probably the best known of all Polish secondary schools, inexplicably was in my days of the type which did not offer or require Latin. He finally forgave me when he met my father and discovered that he not only had a commanding knowledge of Greek and Latin but was fluent in Hebrew and knew old church Slavonic to boot. On the basis of some kind of averaging, I was acceptable.

2. Except for one year in Lwów and a term in Munich, Steinhaus received his mathematical education in Goettingen where in 1911 he was granted the doctor's degree *summa cum laude* for a dissertation "Neue Anwendungen des Dirichlet'schen Prinzips" written under Hilbert.

His own work did not bear the imprint of Goettingen. I don't believe that following his doctoral dissertation he ever wrote anything directly connected with differential and integral equations or calculus of variations. He was at one time, though, interested in foundations of geometry, and one of the doctoral dissertations written under his direction was on this very subject. The influence of that great school on his overall mathematical outlook, however, was crucial—and no wonder. For during his student years he came in close contact not only with Hilbert and Klein but also with F. Bernstein, Carathéodory, Courant, Herglotz, Koebe, Landau (who succeeded Minkowski, who died a little earlier), Runge, Toeplitz, and Zermelo, not to mention scores of physicists, astronomers, and philosophers who also gravitated to Goettingen.

An episode from this period is worth recording. In the spring of 1911 Albert A. Michelson was invited to Goettingen, and he stayed with his family in the pension in which Steinhaus lived. The two men became acquainted and Michelson offered Steinhaus a job as his mathematical assistant.

The Goettingen experience strongly influenced his teaching: he was, for example, the first (and between the two wars the only one) in Poland to lecture regularly on numerical and graphical methods, an interest which is directly traceable to Runge's lectures on this subject. He also emulated Klein and offered from time to time a one-semester course on elementary mathematics (mainly geometry) from an advanced point of view.

But the decisive influence on his own scientific work did not come from Goettingen. It came from Paris where Henri Lebesgue, as yet unrecognized outside his native France, was setting mathematics on a new course which it would travel for many decades to come.

I do not know exactly when Steinhaus became aware of the work of Lebesgue. In the first installment of his memoirs (published in 1970 in the Polish monthly *Znak*), there is a casual mention that he was reading "Leçons sur les séries trigonométriques" in 1912 (or perhaps early in 1913), but there seems to be no mention of "Leçons sur l'intégration" which, of the two books, was certainly the more fundamental.

There is, however, no doubt that he mastered the new theory, for in 1918 he proved that every linear functional on $L(0,1)$ is of the form

$$\Lambda(f) = \int_0^1 f(x)m(x)\,dx, \qquad f \in L(0,1),$$

where $m(x)$ is bounded almost everywhere, thus completing a series of investigations on the forms of linear functionals which was begun by Fréchet and F. Riesz.

In 1920 he proved the beautiful theorem that the set of distances of a set of positive Lebesgue measure contains an interval; at about the same time he also became an extraordinary (Associate) Professor at the University of Lwów and offered a course on Lebesgue's theory, one of the earliest such courses given outside of France.

Somewhat earlier he had devoted himself to the theory of trigonometric and Fourier series and became, almost at once, a recognized master of this field.

He gave the first example of an everywhere divergent trigonometric series whose coefficients tend to zero and, more remarkably, an example of a trigonometric series which converged in one interval and diverged in another. This example led Alexander Rajchman to the creation of the theory of formal multiplication of trigonometric series, and it was Rajchman who in turn interested Zygmund in the field.

Steinhaus also constructed a trigonometric series which converges everywhere without being uniformly convergent in any interval and made numerous other contributions to the anatomy and pathology of trigonometric series.

His interest in trigonometric series led naturally to an interest in more general orthogonal series and to this field Steinhaus also made numerous important contributions culminating in the classic monograph *Theorie der Orthogonalreihen* (Monografje Matematyczne) written jointly with S. Kaczmarz which appeared in 1937.

The theory of orthogonal series had, from the start, been closely related to the theory of linear operators and linear spaces, but the full exploitation of the intimate relation between the two theories came as a result of the pioneering work of Banach.

Steinhaus met young Banach by chance when strolling one day in 1916 through a park in Cracow, he overheard a snatch of a conversation in which the term "Lebesgue integral" was used. Startled, he introduced himself to the two young men who were discussing this unlikely subject, and one of them was Banach. The other was Otton Nikodym.

Steinhaus and Banach wrote only two joint papers, but one of these "Sur le principe de la condensation des singularités" (*Fund. Math.* 9, 1927, pp. 50–61) became a classic and is known to every student of functional analysis.

The principal result is that if $\{F_{pq}(x)\}$ is a double sequence of linear operators on a Banach space E (with values in another Banach space F) such that for each $p = 1, 2, \cdots$

$$\limsup_{q \to \infty} \| F_{pq} \| = \infty$$

($\| F \|$ is the norm of the operator F) there exists a set $X \subset E$ of the second category such that for every $x \in X$

$$\limsup_{q \to \infty} \| F_{pq}(x) \| = \infty, \qquad p = 1, 2, \cdots,$$

($\| F(x) \|$ is the norm of the element $F(x) \in F$).

The collaboration between Banach and Steinhaus was however much closer than the two joint papers would indicate.

Many of Banach's early ideas were developed and first tested in a seminar conducted by Steinhaus, and the two men jointly founded *Studia Mathematica*, which became one of the major mathematical journals of the world.

Banach was Steinhaus' first doctoral student, and Steinhaus joked later on that Banach was his most important mathematical discovery.

In 1938 when the threat of war hung heavily over Poland and Steinhaus presided over the award of an honorary doctorate to Henri Lebesgue, he said to me after the ceremony, "It will not be a bad record to leave behind, to have had Banach as the first and Lebesgue as the last doctoral candidate."

3. In 1923 Steinhaus published in *Fundamenta Mathematica* (4, pp. 283–310) a remarkable memoir under the title "Probabilités dénombrables et leur rapport à la théorie de la mesure." It was the first rigorous axiomatic treatment of coin tossing based on measure theory. (However, almost simultaneously a similar axiomatization was given by A. Łomnicki, a Professor of Mathematics at the Engineering School (Polytechnicum) of Lwów. It should also be mentioned that a nephew of A. Łomnicki, Z. Łomnicki jointly with Ulam already gave in 1932 a most general axiomatic treatment of independence.) It contained the seeds of future developments, and it certainly influenced Kolmogorov's definitive axiomatization, which came ten years later.

It was a truly pioneering work for, at the time it was written, probability theory was not even of peripheral concern to most mathematicians and, in fact, was not generally considered to be a part of mathematics.

It was typical of the way Steinhaus approached mathematics that the main point of the paper was not the axiomatization, for he disliked axiomatization for its own sake, but the concrete question of what the probability is that a series

$$\sum_{k=1}^{\infty} \pm c_k,$$

with $\pm$ signs chosen "at random," converges.

Within the framework of the classical Laplacian probability theory, such a question cannot be properly formulated, and Steinhaus set himself the goal of extending and modifying this framework to make such questions well-posed.

He did it by constructing a measure on the set of all infinite sequences

$$(r_1, r_2, r_3, \cdots),$$

where each r_k is either $+1$ or -1, the measure reflecting the assumptions that the tosses are independent, and that in each toss the two alternatives are equiprobable.

This he accomplished by showing that the desired measure maps into the ordinary Lebesgue measure on (0, 1) by the map

$$(r_1, r_2, \cdots) \rightarrow t = \sum_{k=1}^{\infty} \frac{(2r_k + 1)}{2^{k+1}}.$$

Writing t as a binary expansion

$$t = \sum_{k=1}^{\infty} \frac{\varepsilon_k(t)}{2^k}$$

and setting

$$r_k(t) = \frac{2\varepsilon_k(t) - 1}{2},$$

we see at once that the question of the probability of convergence of the series $\Sigma \pm c_k$ becomes equivalent to the question of the measure of the set of convergence of the series

$$\sum_{k=1}^{\infty} c_k r_k(t).$$

This question was partially answered by Rademacher who had proved a few years before that if

$$\Sigma c_k^2 < \infty,$$

the series converges almost everywhere, and hence the desired probability is one.

Somewhat later Paley and Zygmund proved that if $\Sigma c_k^2 = \infty$, the probability in question is zero.

These results were clear forerunners of Kolmogorov's famed "three-series theorem" concerning convergence of sums of independent random variables.

To the same circle of ideas belongs also the result (see "Über die Wahrscheinlichkeit dafür, dass der Konvergenzkreiss einer Potenzreihe ihre natürliche Grenze ist," *Math. Zeit.*, 31 (1929) pp. 408–416) that if the power series

$$\sum_{1}^{\infty} c_n z^n$$

has radius of convergence r $(0 < r < \infty)$ then with probability one the circle $|z| = r$ is a natural boundary for the series

$$\sum_{1}^{\infty} c_n e^{2\pi i\theta_n} z^n,$$

where the θ_n are independent and uniformly distributed in $(0, 1)$.

In 1929 it was far from clear what "independent and uniformly distributed" meant, and Steinhaus had first to make these concepts precise. This he did by constructing a product measure in the Hilbert cube (i.e., the direct product of denumerably many unit intervals) with uniform measure on each component.

4. Let me now jump many years ahead and speak briefly of yet another of Steinhaus' achievements.

Logic and foundations were not among his main interests, and, as a matter of fact, his attitude toward them was mildly critical. He did as a young man write a paper in which he axiomatized the concept of limit, and he was, as mentioned above, in the midtwenties interested in foundations of geometry, but that was about all. Thus he did not appear to be a likely candidate to challenge the axiom of choice and propose a substitute. But this is exactly what he did, and in a manner characteristic of the way in which he approached mathematics, and it had to do with his interest in games and the problems they pose.

Games always fascinated Steinhaus, and he was among the first to define and discuss the concept of strategy. This he did in a little-known note in Polish, published in an obscure non-scientific Polish journal, in 1925, fortunately saved for posterity through a translation into English ("Definitions for a theory of games and pursuits" in *Naval Research Logistics Quarterly* 7, 1960, pp. 105–108). He was aware of the following theorem: Let G be a two-person game with perfect information, terminating in a *finite number* of moves in a win by one of the players. Then there must exist a winning strategy for either one or the other adversary.

The proof which I heard him give in a lecture at Rockefeller University in 1962 is unforgettably simple:

Denoting the players by A and B and their moves by $x_1, x_2, \cdots$ and $y_1, y_2, \cdots$ respectively, we can express the fact that A has a winning strategy symbolically as follows (we assume that A starts)

$$(\exists x_1)(y_1)(\exists x_2)(y_2)\cdots(\exists x_n)(y_n) \qquad A \text{ wins.}$$

The negation of this statement is obtained by the familiar rule of De Morgan, and it reads

$$(x_1)(\exists y_1)(x_2)(\exists y_2)\cdots(x_n)(\exists y_n) \quad A \quad \text{does not win.}$$

This however is clearly the statement that B has a winning strategy, and the proof is thus complete!

Steinhaus now proposed that this simple theorem be made into an axiom by removing the restriction that n is finite, i.e., that the game must terminate in a finite number of moves.

It is here that one runs afoul of the axiom of choice.

In the nineteen-thirties, Banach and Mazur (with much help from Ulam) had considered a class of infinite games of which the following is typical:

Let S be a subset of the interval $(0, 1)$ and let the players A and B choose binary digits x_1, y_1, x_2, $y_2, \cdots$ (actually in the original Banach-Mazur version the players were allowed to pick an arbitrary finite number of digits) defining the number ξ,

$$\xi = \frac{x_1}{2} + \frac{y_1}{2^2} + \frac{x_2}{2^3} + \frac{y_2}{2^4} + \cdots .$$

Player A wins if $\xi \in S$ and B if $\xi \notin S$. Using the axiom of choice one can "exhibit" (as was shown by Mycielski on the basis of an older result of Banach-Mazur-Ulam) a set S for which neither A nor B had a winning strategy, and therefore the Steinhaus axiom (which came to be known as the axiom of determinacy) contradicted the axiom of choice. I recall vaguely that the Banach-Mazur-Ulam result was used to "prove" that there could be at most one God, since if there were two, they could be made to play the game, with a Banach-Mazur-Ulam set giving rise to the inevitable difficulty that neither could be considered omniscient. Steinhaus felt that his axiom was closer to "reality" than the axiom of choice, for he insisted that intuition demands that for each set one of the players should have a winning strategy.

The brief note (written jointly with J. Mycielski, who also contributed much to the clarification of the role of the new axiom) "A mathematical axiom contradicting the axiom of choice" (*Bull. Ac. Pol. Sc. Série des sci. math., astr. et phys*,. 10 (1962) pp. 1–3) attracted considerable attention among contemporary logicians and is responsible for an ever-growing body of work.

I mention this unique excursion into set theory because it is so characteristic of Steinhaus' mathematical style—a style in which superb intelligence was combined with rare wit and an unerring instinct for what was essential and promising, with an eye for the unusual and striking.

His wit was not confined to mathematics and many of his repartees became legendary. When, about fifteen years ago, after failing to attend an important meeting of a committee of the Polish Academy of Sciences, he received a letter chiding him (along with several others) for not having "justified his absence," he wired the President of the Academy that, "as long as there are members who have not yet justified their *presence*, I do not have to justify my absence."

He was also a master of mathematical paraphrase. When he first heard the statement of the Borsuk-Ulam theorem, he immediately said, "It means that at any given time there is at least one pair of antipodal points on the surface of the earth at which the temperatures and pressures are the same."

It should be also recorded that it was Steinhaus who proved and invented the name "ham sandwich theorem" and that he liked to explain jokingly why π appears so often in probability theory by quoting a Polish proverb *Fortuna kołem się toczy* ("Fortune moves in circles" in a translation which unfortunately leaves much to be desired).

I cannot resist giving one more example of Steinhaus' quick mathematical intelligence. It has to do with his estimate of the casualties of the German army in 1944, and it should be borne in mind that he was then in hiding and completely cut off from any source of reliable news.

He noticed that some of the obituaries of German soldiers which were published in the rigidly controlled local newssheet mentioned that the dead was the second or even third member of his family to have fallen in the war, and this was information enough!

For by dividing the percentage of obituaries of second, third, etc. sons by the (conditional) probability that a family with at least one son will have more than one, an estimate of casualty percentage can be obtained. Disregarding the age factor (some sons may be too young to be drafted), all one needs is the average number of sons in a family (easily estimable) and the knowledge that the number of sons obeys the Poisson distribution.

5. I met Steinhaus for the first time in the Spring of 1932 at the end of my first year at the University of Lwów, when I drew him as the oral examiner in Analysis I. There were four Professors of mathematics, and to insure equitable distribution of examination fees, examiners were chosen by lot. He had a reputation of being very tough.

This reputation was not all that well deserved, as the following anecdote indicates. A girl student who was not terribly good drew Steinhaus as the examiner in Analysis II, by far the most difficult of all examination subjects. We were all surprised when she emerged with a B, and I asked him later on how it happened. "Well," he said, "I asked her to describe the Riemann surface of $\sqrt{z}$, and she said that she had one in her purse and after a brief search produced a rather nice model. Don't you think that any young lady who carries a Riemann surface in her purse deserves at least a B?" I couldn't argue the point.

He asked me, as I recall, two very simple questions and gave me an A. Before I left his office, I asked permission to attend his Seminar (which he conducted jointly with Mazur). Permission was needed, since I would be jumping into the second year of a two year cycle. He allowed me to register, and thus there began my mathematical life.

For a while my contacts with him were confined only to the Seminar, in which I learned more mathematics than in retrospect seems possible, but sometime in 1934, I believe, he presented me with a definition of independent functions (in the statistical sense) and suggested that I try to do something with it. We began to collaborate and from about 1935 until November 1938 when I left Poland, we were almost inseparable. For a time he employed me as a private assistant, and in this capacity I helped him with *Mathematical Snapshots* (2nd Ed., Oxford University Press, 1950) but only in mundane matters of routine. All the ideas that went into this remarkable book were his.

I shall not attempt to evaluate the role which our collaboration on independent functions played in some of the developments in probability theory of the last forty years. Suffice it to say that when I dedicated my Carus Monograph *Statistical*

Independence in Probability, Analysis and Number Theory to him, it was not merely the sentimental gesture of a grateful pupil. Nearly everything in that book is traceable to the years of our collaboration, to long walks through the streets and parks of Lwów, to interminable discussions and arguments, even to occasional minor battles. Were it not for the war and separation, the book might have been a joint undertaking. In a way it is, although only my name appears on the cover.

I should like to go back to the *Snapshots*, because to understand and appreciate Steinhaus' mathematical style, one must read (or rather look at) *Snapshots*.

Written in 1937 and designed to appeal to "the scientist in the child and to the child in the scientist," it has gone through uncountably many editions, has been translated into fourteen languages, and is still among the best selling "popular" books on mathematics. It is a book unlike any other, and it expresses, not always explicitly and at times even unconsciously, what Steinhaus thought mathematics is and should be.

To Steinhaus mathematics was a mirror of reality and life much in the same way as poetry is such a mirror, and he liked to "play" with numbers, sets, and curves, the way a poet plays with words, phrases, and sounds.

His approach to mathematics was largely visual and only seldom abstract. He liked objects and facts and was suspicious of most generalizations and extensions. "A statement about curves is not interesting unless it is already interesting in the case of a circle," he told me years ago, and this sums up well his fundamental belief that real insights are gained from contemplating the simplest and most elementary things.

Steinhaus deplored the growing professionalization of mathematics, the ever-increasing specialization, the flight from robust reality into the murky clouds of uncontrolled abstraction.

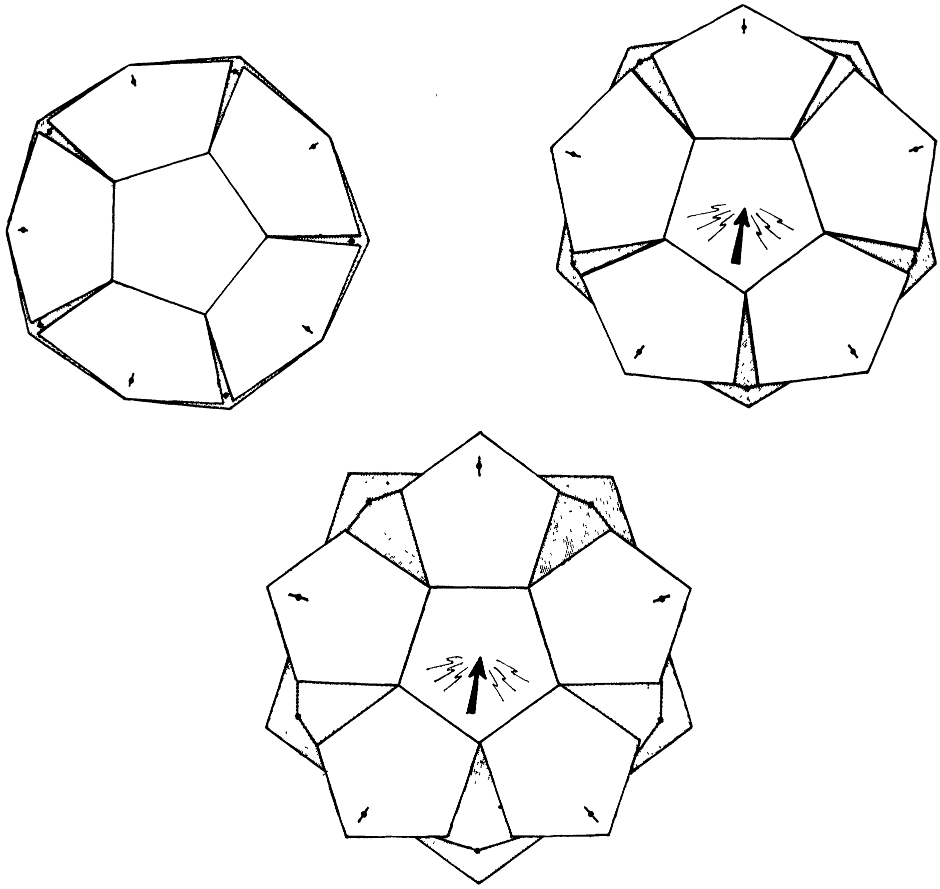
He spent a significant portion of his scientific life in collaborating with physicians (he was awarded an honorary degree of Doctor of Medicine in recognition of his contributions toward applying mathematics to a wide variety of biological and medical problems), engineers, oil prospectors, geologists, without a thought entering his mind that he might be engaging in a different sort of activity than that which led him and Banach to formulate the principle of condensation of singularities.

Mathematics to Steinhaus was mathematics, and he was scornful of labels such as "pure," "applied," "concrete," "useful," etc. He liked clear sharp points and was impatient with long-winded discourses. "Wo ist der Witz?" ("Where is the joke?"), he liked to ask in an attempt, not always successful, to cut through the fog.

He had a life-long love affair with elementary mathematics and could spot new wonders in the simplest and most familiar objects.

He was particularly proud (and justly so) of having invented the self-folding dodecahedron, and each copy of the first edition of *Snapshots* was provided with a handmade model (this had to be abandoned in later editions because of prohibitive cost).

The three sketches below are based on one of the few remaining models.



The barely visible line represents a rubber band which when unstretched is a geodesic of the regular dodecahedron, and as pressure is applied to the upper face, the band stretches. As the pressure is released, the tendency of the rubber band to snap back has the effect of restoring the "squashed" dodecahedron to its original Platonic form. Years ago in Lwów I remember Steinhaus telling me: "Everything I have done could have been done by someone else, but only I could have invented the self-folding dodecahedron."

He was wrong in the first premise but right in the second. Only he could have done it.

Acknowledgment: I wish to thank Professors D. A. Martin and J. Mycielski for setting me straight on a number of points concerning the axiom of determinacy, and Mr. Jonothan Logan for many suggestions and improvements.

DEPARTMENT OF MATHEMATICS, ROCKEFELLER UNIVERSITY, NEW YORK, NEW YORK 10021.