Chapter 2
A Personal History of the Scottish Book, *Mark Kac*

It is a special pleasure to be introduced by my old friend Erdős. The use of the adjective "old" is slightly depressing, and I would like to forget about it, but somehow Erdős will not let me do it.

I should like to begin my remarks by pointing out the remarkable thing that we celebrated the Scottish Book in Denton, Texas. It is remarkable not only because of the energy, dedication and interest of one man, namely Dan Mauldin, but it is also, for me at least, typically American. It represents the kind of combination of generosity and sentiment which runs through the whole history of this young civilization. I cannot think of any other country on the surface of the earth which would be interested in celebrating a somewhat obscure event which occurred in another country in what now seems like the dim past. And so on my own behalf, and I am sure also on behalf of all my former and present compatriots, I would like to express our thanks not only to Dan Mauldin but also to the spirit of America in Denton, Texas.

Before I come to Mathematics and to my connection (tenuous as it was) with the Scottish Book let me engage in a little of what Stan Ulam, quoting Disraeli, referred to as "anecdotage."

As you can see by perusing the Scottish Book, a significant number of problems were inscribed by distinguished foreign mathematicians who passed through Lwów. One of the most famous of these visitors and probably the most famous one, was Henri Lebesgue.

Lebesgue came to Lwów in May 1938 to receive an honorary doctorate from the University. At that time, since Stan Ulam, who was the Secretary of the Lwów Section of the Polish Mathematical Society, was away in the USA, I was substituting for him and was given the extremely pleasant job of showing Lebesgue around the city. I reminisced about this event in 1974 in Geneva, when the centennial of Lebesgue's birth was celebrated. My remarks were published in *L'Enseignment Mathématique* in French, and were later translated into Polish (not by me since my knowledge of my mother tongue is no longer sufficiently reliable). Today I give

R.D. Mauldin, *The Scottish Book*, DOI 10.1007/978-3-319-22897-6_2

you an abbreviated English version of these remarks. In fact I will tell you only two stories, one of which is directly connected with the Scottish Café, the birthplace and home of the Scottish Book.

At the time of his visit Lebesgue was no longer interested in anything but elementary mathematics; he refused to discuss measure, integrals, projection of Borel sets, or anything of that sort. He gave two lectures, both extremely beautiful, but entirely elementary: one on construction by ruler and compass, and the other on iterated radicals.[1]

As a footnote to the political atmosphere of those days it may be of interest to record the following. The Polish press, which was inept above and beyond the call of duty, confused Lebesgue with Hadamard. Hadamard was a known leftist. Lebesgue, on the other hand, was a man of rather conservative views, though by no means a reactionary. He was greeted upon arrival by a violent editorial against the leftist, communist French professor being honored by the Poles. The confusion was soon cleared up, but nobody bothered with a retraction. So you can see the press is the same the world over, and not much has changed in this respect over the years.

As I showed Lebesgue around the city he was extremely disappointed with me—he was very much interested in the churches, and wanted to know all about their history, and I was unable to provide him with much information on that subject. Lwów by the way was an extremely interesting city from the religious point of view, because it was, with the possible exception of Jerusalem, the See of all three lines of Catholicism. There were in fact three archbishops in Lwów, representing the Roman, Greek, and Armenian branches.

The Armenian Cathedral, one of the most beautiful churches in Europe, especially interested Lebesgue. To his chagrin I could not tell him anything about it, and I was equally disappointed by Lebesgue's refusal to discuss measure, integrals, and other mathematical topics. Still, we became reasonably friendly, and he merely pitied me as one doomed to some terrible fate for lack of interest in history.

That afternoon we had a 5 o'clock reception for Lebesgue in the Scottish Café. Fewer than 15 people attended, which goes to show how small the number of mathematicians was in those days. The waiter gave all of us menus, and not realizing that Lebesgue was not a Pole he gave him one too. Lebesgue looked at the menu for about 30 seconds with utmost seriousness and said, "Merci, je ne mange que des choses bien definies" (Thank you, I eat only well-defined things). At this moment I had an inspiration, and by changing a little a well-known phrase of Poincaré directed against Cantorism I said, "Ne mangez jamais que des objets susceptibles d'être définis par un nombre fini de mots" (Never eat things which cannot be defined in

[1]Professor Granas brought with him to the Texas conference a copy of *Summaries of the Proceedings* of the meetings of the Mathematical Society in Lwów. From these documents we can ascertain that one of the lectures took place on May 25, 1938, and was on iterated square roots. It also turned out that my recollection as reported in my Geneva talk was not entirely correct, but the errors are minor.

a finite number of words). “Ah,” said Lebesgue, “you are familiar a little bit with Poincaré’s philosophy,” and I think that he forgave me at that moment my ignorance of the history of the Armenian Cathedral.

My second remark of an anecdotal nature has to do with the beautiful talk by Professor Martin, my former colleague at Rockefeller University, which was presented at the conference in Texas. It should be of interest, because it characterizes the way Steinhaus felt about mathematics, and especially about the axiom of determinacy. I am sure of this because I attended lectures by Steinhaus at both Rockefeller and the Courant Institute in the early sixties. I also had many occasions to speak to him about it.

I will now give Steinhaus’ “proof” of the determinacy of the Ulam game.

We of course all remember the Ulam game, where Player One picks a zero or one and Player Two picks a zero or one, and one then constructs what Tony Martin called a decimal binary (which is an excellent name for what ordinary mortals call simply a binary). [Editor Note: The term binary decimal goes back to Turing’s classic 1937 paper on computable numbers and Hardy and Wright’s introduction to the theory of numbers.] If it falls into a set E Player One wins, and if it is not in E Player Two wins. The question is: Is there a winning strategy for either one of the players? Here is a “proof” that there is one:

Let me denote by $x_1, x_2, \ldots$ the moves of Player One, and by $y_1, y_2, \ldots$ the moves of Player Two. I will give in logical symbols, which I use very infrequently, the statement that Player One has a winning strategy:

$$\exists\, x_1(y_1)\, \exists\, x_2(y_2) \cdots \frac{x_1}{2} + \frac{y_1}{2^2} + \frac{x_2}{2^3} + \frac{y_2}{2^4} + \cdots \in E.$$

It says, “There is a first move of Player One such that for every first move of Player Two there is a second move of Player One, such that for every second move of Player Two, etc., the fraction $\frac{x_1}{2} + \frac{y_1}{2^2} + \cdots$ belongs to E.” This is merely a transcription in logical symbols of the statement that there is a strategy for Player One.

Now suppose there is no such strategy; then you put the symbol $\sim$ in front of the string of quantifiers in the formula above and use the DeMorgan rule, obtaining

$$(x_1)\, \exists\, y_1(x_2)\, \exists\, y_2 \cdots \frac{x_1}{2} + \frac{y_1}{2^2} + \frac{x_2}{2^3} + \frac{y_2}{2^4} + \cdots \notin E.$$

Now if you translate this into human language, it means that Player Two has a winning strategy. So if Player One doesn’t have a strategy, Player Two has a strategy, and, consequently, the axiom of determinacy in this case merely allows one to use DeMorgan’s law for an infinite number of quantifiers. Now of course you can see where the difficulty comes in. It is that difficulty which plagues the whole beastly subject, and it is, namely, where you ask, “How does one know whether something does or does not belong to set E?” It is here, of course, that we get into all the difficulties, and Steinhaus merely felt—and I have enormous sympathy for it—that his axiom had a chance to distinguish those sets E that are worthy to be called sets from those that are not.

Axioms like the axiom of choice allow us—give us a legal license—to create certain objects and then call them sets. Steinhaus thought that his axiom would be of the kind that would distinguish between constructible and nonconstructible sets.

This little argument reminds me—and now I am only almost serious; up to this point I was dead serious—of an imperfect analogy with what happens in quantum mechanics where certain statements, although they sound perfectly all right, are not allowable. For instance, when you say, "The amount of energy in a radiation field in a subvolume," then it sounds like a perfectly well-defined thing. But if you really follow the dicta of quantum mechanics, you have to express it in terms of a Hermitian operator—every physical quantity has to be represented by a Hermitian operator—and it turns out that it is not unique. In fact, how to interpret this may very well depend on the method of measurement. You have something of the sort here—nothing is really defined until you come to grips with saying, "How do you know whether a number constructed by an infinite number of operations does or does not belong to a set?"

Now, one final observation in connection with other people's involvement in the Scottish Book Conference, namely with Professor Zygmund's, who referred in his talk to one of the greatest Polish discoveries, the category method. As a matter of fact, this discovery is so well known that one does not even recognize what a remarkable discovery it was. It was remarkable because it showed that sometimes it is easier to prove that *most* objects have a certain property than to exhibit a particular example.

Professor Zygmund asked about the rearrangement of the Fourier series in connection with the question of convergence, and bemoaned the fact, which many of us bemoan, that there is no decent, sensible measure in the set of all permutations. However, if one goes back to the Polish invention of the method of category, then of course the set of all permutations can be easily metrized by the Frechet trick. Consequently, the concept of sets of first and second category is perfectly well defined. There is in fact a book by Professor Oxtoby, who attended the conference (and I even ascertained from him that it was published in 1971 by Springer-Verlag), called *Measure and Category*. The message of the book is that whenever both can be defined and whenever the measure is reasonable, then second category and measure one, other than in very exceptional situations, are the same. One can rephrase Professor Zygmund's question to ask whether the set of all permutations of Fourier series which lead to divergence is of second category. A very simple case—similar abut much simpler—was considered by my colleague at the time, and still a good friend, Professor Ralph Palmer Agnew of Cornell, in response to a question posed during a conversation we had many years ago. If you take a conditionally convergent series of real numbers, then of course we know that it can be rearranged so as to make it converge to any prescribed number, and it can also be rearranged into a divergent series. Now it is easy to prove, and in fact Agnew proved it (it was published around 1940 in the *Bulletin of the American Mathematical Society*) that the set of permutations which lead to divergent rearrangements is indeed of second category. You might say that everything bad which one might expect to happen is going to happen in a plentiful sort of way.

Now to some of the more personal things. I am not really, in a certain sense, a product, or at least not a typical product, of the Polish school. When I came to Lwów as a student in October 1931, I did not know any of the great masters; my first contact was with the late Marceli Stark, a remarkable man and a tremendously well-educated mathematician who died recently and to whose memory I would like to pay tribute. I was very concretely minded, and I still am—in fact even more so. Yet I felt a little bit that I also ought to do these abstract things, and Steinhaus, whom I met a little later, said, "You shouldn't; you must earn the right to generalize." I have not yet earned that right.

I became interested in probability theory in a way that I am not even going to tell you in detail, because I can't give you a full autobiography. Some day I am going to get even with Stan Ulam and write my own adventures, which, however, are not nearly as exciting as his.

It was through Steinhaus that I became interested in probability theory, and, with the exception of one problem (I put altogether four problems into the Scottish Book—numbers 126, 161, 177, and 178) and I really do not know why I put it in; it is not even properly stated—these problems deal directly or indirectly with probability theory. The first one is a minor technicality, which Hinčin proved in response to a letter.

The problem that I cannot for the life of me remember how and why I thought of it, is the problem of characterizing continuous functions, $\phi(x,y)$, such that if A and B are real symmetric matrices, then ϕ is positive definite (Problem no. 177). Now, because of noncommutativity, $\phi(A,B)$ is not properly defined. But that is easily remedied if ϕ is a polynomial in two variables—one simply replaces ϕ by a symmetrized polynomial, in which case it makes perfect sense, and the question can still be asked. Whether it is of any interest I have no idea. I do not have any recollection as to why it interested me at the time, and I probably should have appealed to Dan Mauldin to put this problem in a footnote because there is no particular reason to bother the next generation with this one—unless in the meantime I remember what it was I really wanted.

The first, as I have already told you, was a minor technical problem, but the fourth (Problem no. 178) has a certain degree of interest, and I may as well say what it is. It is unsolved not because it is necessarily difficult, but because nobody has tried. I am not going to give any prizes for it. It might, however, be of some interest to those of you who are analytically-minded.

There is a well-known theorem of Cramér that if a product of two characteristic functions $\phi_1(\xi)$, $\phi_2(\xi)$ is $\exp(-\xi^2/2)$ then both ϕ_1 and ϕ_2 must themselves be Gaussian, i.e.,

$$\phi_1 = \exp(-\alpha_1\xi^2 + \beta_1\xi)$$

$$\phi_2 = \exp(-\alpha_2\xi^2 + \beta_2\xi)$$

with $\alpha_1 + \alpha_2 = 1/2$ and $\beta_1 + \beta_2 = 0$. (In Problem 178 the theorem is slightly misstated.)

In probabilistic terms, if a sum of two *independent* random variable is Gaussian, then the random variables themselves must be Gaussian. Similar theorems hold for stable distributions. My Problem 178 raised the question of whether other distributions can be similarly characterized. One must, of course, get away from the product since the product is intimately tied to addition of random variables and therefore to stable distributions, and I hit upon

$$\left(\frac{1}{x}+\frac{1}{y}-1\right)^{-1}$$

as a candidate for the characterization of the class of characteristic functions

$$\frac{1}{1+\alpha\xi^2},\ \alpha > 0.$$

The problem is closely related to the following problem which is perhaps of greater general interest:

What are the functions $F(x,y)$ of two variables such that $F(\phi_1(\xi),\phi_2(\xi))$ is a characteristic function of a probability distribution whenever ϕ_1 and ϕ_2 are?

I strongly suspect that F must be a function of the product xy, i.e., $F(x,y) \equiv G(x,y)$ with G satisfying some additional conditions, but I have no idea how to go about proving it.

The only one of my four problems which was destined to have a future was Problem 161. There is not much point in going into details since an interested reader can consult my 1949 address, "Probability Methods in some problems of analysis and number theory" (*Bull. Am. Math. Soc. 55*, (1949), 390–408). Echoes of this problem are still reverberating, as witness a recent paper by I. Berkes, "A Central Limit Theorem for Trigonometric Series with Small Gaps," (*Z. für Wahrsch.*, 47 (1979), 157–161), but the original source will only become known with the publication of the Scottish Book. As it is, not even my 1949 address is cited, which is some kind of a price one must pay for pioneering.

Problem 161 bears the date June 10, 1937, which was five days after I repeated the ancient oath, "*Spondeo ac polliceor ...*" and was awarded the degree of Doctor of Philosophy of the John Casimir University in Lwów. Actually, not knowing Latin, I got into my head that in *spondeo* the accent is on the second syllable and not, as is correct, on the first. Steinhaus, who was my sponsor *(promotor)* and who was a stickler for proper usage of all languages, used to make me practice the correct pronunciation before the actual ceremony. When the moment arrived for me to reply to a Latin oath read with pomp (though not with pomposity) by the *Rector Magnificus* I forgot all the practice and put an emphatic accent on the wrong (second) syllable. Steinhaus cringed and so did my father, who knew Latin and who journeyed to Lwów to witness the occasion.

Returning to the Scottish Book, I would like to point out that although the problems in it range over most of the principal branches of Mathematics, one branch is conspicuously absent, and that is Number Theory. The reason is simple, and it is that Number Theory was not in vogue in Poland at the time. Sierpiński in his younger years (and also toward the end of his life) did important and interesting things in Number Theory, and the Warsaw school did produce two "mutants": A. Walfisz (who left Poland for the Soviet Union and was Professor at Tiblisi) and S. Lubelski. There was even a serious journal, *Acta Arithmetica* (which continues to this day), devoted to Number Theory, but this beautiful and important area was far from the forefront of mathematical preoccupation in Poland before World War II.

I cannot remember at all how I came to think about number theoretic problems in connection with Probability Theory, but I do remember making what appeared to me then to be a great discovery (it wasn't).

If $\phi(n)$ is the familiar Euler function, one has

$$\frac{\phi(n)}{n} = \prod_{p|n}\left(1-\frac{1}{p}\right)$$

which can be written in the form

$$\frac{\phi(n)}{n} = \prod_{p}\left(1-\frac{\rho_p(n)}{p}\right)$$

where

$$\rho_p(n) = \begin{cases} 1, & p \mid n, \\ 0, & p \nmid n. \end{cases}$$

This of course was a well-known elementary fact, but the method also yielded at once

$$\begin{aligned} M\left\{\left(\frac{\phi(n)}{n}\right)^{\ell}\right\} &= \prod_{p} M\left\{\left(1-\frac{\rho_p(n)}{p}\right)^{\ell}\right\} \\ &= \prod_{p}\left[1-\frac{1}{p}+\frac{1}{p}\left(1-\frac{1}{p}\right)^{\ell}\right], \end{aligned}$$

for all ℓ such that the infinite product converges, and hence one had a handle on the distribution of $\phi(n)/n$.

When late in November 1938 I left for the United States, the boat (M.S. *Pilsudski*, sunk in the early days of World War II) stopped for about six hours in Copenhagen, which gave me a chance to meet Professor Børge Jessen. I communicated my number theoretic discovery to him only to learn that the same result had been obtained and already published by I.J. Schoenberg. The probabilistic nature of the result was, however, somewhat hidden in Schoenberg's proof, and I had the advantage (because of my deep involvement with the normal distribution in unexpected contexts, as illustrated by Problem 161) of being—so to speak—on the ground floor. It was therefore a small step to suspect that the number of prime divisors $\nu(n)$ of n given by the formula

$$\nu(n) = \sum \rho_p(n)$$

should behave like a sum of independent random variables and hence be normally distributed after subtracting an appropriate mean ($\log\log n$) and scaling down by an appropriate standard deviation ($\sqrt{\log\log n}$). But here my ignorance of Number Theory proved an impediment. The number of terms in the sum $\sum \rho_p(n)$ depends on n, preventing a straightforward application of the Central Limit Theorem. I struggled unsuccessfully with the problem until I stated my difficulties during a lecture in March 1939 in Princeton. Fortunately Erdős was in the audience and he perked up at the mention of Number Theory. He made me repeat my problem, and before the lecture was over he had a proof. Thus did the Normal Distribution enter Number Theory and thus was born its probabilistic branch. While stretching a bit the historical truth I hereby assign the role of godmother of this branch to the Scottish Book.

http://www.springer.com/978-3-319-22896-9

The Scottish Book
Mathematics from The Scottish Café, with Selected Problems from The New Scottish Book
Mauldin, R.D. (Ed.)
2015, XVIII, 322 p. 23 illus., 4 illus. in color., Hardcover
ISBN: 978-3-319-22896-9
A product of Birkhäuser Basel