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Hilda Geiringer-von Mises, Charlier Series, Ideology, and the Human Side of the Emancipation of Applied Mathematics at the University of Berlin during the 1920s

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The controversy surrounding Hilda Geiringer's application for "Habilitation" (permission to teach) at the University of Berlin (1925–1927) sheds some light on the struggle of "applied mathematics" for cognitive and institutional independence. The controversy as well as Geiringer's unpublished reminiscences reveal the decisive influence of Richard von Mises, Geiringer's husband since 1943, on both her career and the course of applied mathematics at the University of Berlin. Some more speculative remarks reflect on the possible ideological background of this controversy in post-World War I Germany. The debate over Geiringer's theses for Habilitation ("Habilitationsschriften") opens up a chapter of the history of mathematical statistics, namely, expansions of a discrete distribution with an infinite number of values in a series in successive derivatives of the Poisson distribution with respect to the parameter. These expansions were first proposed by the Swedish astronomer C. L. W. Charlier (1862–1934) in 1905. © 1993 Academic Press, Inc.

Spor v 1925–1927 g. o prinyatii zhenshchiny-matematika, Khilda Geiringera, dotsentom v Berlinskom universitete (= "khabilitatsiya") otrazhaet borbu "prikladnoi matematiki" za samostoyatelnost. Spor i neopublikovannye do sikh por vospominaniya Geiringera pokazyvayut znachitelnoe vliyanie Rikharda Fon Misesa, muzha G. s 1943 g., na kareru G. i na razvitie prikladnoi matematiki. Neskolko legko spekulativnykh zamechanii kasayutsa ideologicheskogo zadnogo plana spora vskore posle pervoi mirovoi voiny. Diskussiya "tesisa za khabilitatsiyu" G. pozvolyaet ponimanie glavy istorii matematicheskoi statistiki. Rech idet o razlozhenii diskretnykh raspredelenii s beskonechnom mnozhestvom znachenii veroyatnosti v ryady po proizvodnym (otnositelno parametra) raspredeleniya Puassona. Takoe razlozhenie bylo predlozheno (no ne dokazano) pervyi raz shvedskim astronomom K. L. V. Sharle (1862–1934) v 1905 g. © 1993 Academic Press, Inc.

Die Kontroverse um Hilda Geiringers Habilitationsverfahren an der Berliner Universität (1925–1927) reflektiert Momente des Kampfes der "angewandten Mathematik" um kognitive und institutionelle Unabhängigkeit. Die Kontroverse selbst wie auch unveröffentlichte Erinnerungen von Geiringer zeigen den maßgeblichen Einfluß von Richard von Mises, Geiringers Ehemann seit 1943, auf ihre Laufbahn und auf die Entwicklung der angewandten Mathematik an der Berliner Universität. Einige spekulativere Bemerkungen sind dem möglichen ideologischen Hintergrund jener Auseinandersetzungen in den Jahren nach dem Ersten Weltkrieg gewidmet. Die Auseinandersetzung um Geiringers Habilitationsschriften ermöglicht Einblick in ein Kapitel der Geschichte der mathematischen Statistik. Es handelt sich dabei um die Entwicklung von diskreten Verteilungen mit unendlich vielen Wahrscheinlichkeitswerten in Reihen nach Ableitungen (hinsichtlich des Parameters) der Poisson-Verteilung, die erstmals 1905 von dem schwedischen Astronomen C. L. W. Charlier (1862–1934) vorgeschlagen wurde. © 1993 Academic Press, Inc.

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INTRODUCTION

In a talk delivered in 1965 Alexander Ostrowski (1893–1986) expressed the view that

> Only with the appointment of Richard von Mises [in 1920] to the University of Berlin did the first mathematically serious German school of applied mathematics with a broad sphere of influence come into existence. Von Mises was an incredibly dynamic person and at the same time amazingly versatile like Runge. He was especially well versed in the realm of technology. Because of his dynamic personality his occasionally major blunders were somehow tolerated. One has even forgiven him his theory of probability. At the same time the mathematical atmosphere in Berlin was much more open and less tense than in Göttingen. The sovereign Olympian, Erhard Schmidt, Issai Schur's evident sense of what was mathematically important, and Bieberbach's impulsive youthfulness created a mathematical climate that was very favorable to von Mises' activities. [Ostrowski 1966, 106]

Ostrowski's statement, however, may well have been influenced by his conception of what "applied mathematics" should be and does not seem to do justice to the Göttingen tradition [1]. Moreover, this article aims to show that Ostrowski painted the mathematical atmosphere in Berlin in somewhat too rosy colors. Applied mathematics in Berlin, notwithstanding the foundation of an "Institute for Applied Mathematics" (with only two permanent positions), was still in a situation of struggle for institutional and cognitive independence. Financial conditions were certainly tighter than in Göttingen. Perhaps the image Ostrowski painted of applied mathematics in the 1920s is also somewhat distorted, due to the comparison with the far worse situation in Nazi Germany, at least at the University of Berlin [Siegmund-Schultze 1989].

The controversy surrounding Hilda Geiringer's application for "Habilitation," i.e., the highest German academic degree, connected with the "venia legendi," the permission to teach at universities, reveals some of these problems and hints at additional ones.

First, the shortage of adequately trained personnel for the new, more sophisticated demands of applied mathematics is exemplified in the case of this young woman mathematician. Geiringer (1893–1973) came from a rather narrow field of pure mathematics and had to accommodate herself in a very short period of time to various fields of applied mathematics, which required versatility in methods. At the same time she had to cope with a considerable teaching load (the well-known "Praktikum" at the Institute of Applied Mathematics), with her duties as a single mother of a small child, and with a very exacting, sometimes even rude teacher and friend, Richard von Mises. Geiringer's extremely self-critical attitude until the end of her career [2]—not an uncommon trait among mathematicians in general—was partly a reflection of these considerable burdens. At the same time Geiringer's problems revealed the still unstable situation of applied mathematics within the German mathematical culture.

Second, it was the "pure mathematicians" at the University of Berlin who were anxious to maintain the standards of their profession, drawing a borderline around "applied mathematics" as a special, more restricted field. In the case of Ludwig

Bieberbach, his "impulsive youthfulness" may have been coupled with additional ideological motivations that seem to have sharpened his critical attitude toward Geiringer's work. This was an additional burden for the first (in the end successful) mathematical "Habilitation" of a woman in Berlin and certainly—after Emmy Noether's in Göttingen—the second in Germany [Boedeker 1974].

Finally, it is the outstanding personality of Richard von Mises (1883–1953) and his pioneering efforts for the promotion of applied mathematics as an independent discipline that provide the background for the following story. While these pioneering efforts have been described in [Bernhardt 1979, 1980], von Mises' influence on Geiringer's career in the 1920s has not been adequately discussed thus far. Although von Mises and Geiringer worked for the most part on different topics and coauthored only two papers [Binder 1992], there is no doubt that Geiringer remained essentially in the role of von Mises' student until the end of their mathematical and personal relationship. As late as 1953 Geiringer considered the following remark of von Mises as typical of their (one-sided) partnership:

> Du bist voreingenommen, mein Kind, meine treueste Bewunderin. [Binder 1992, 43]

It was her love for von Mises at least as much as her love for mathematics that enabled Hilda Geiringer to surmount most of the problems described above and to become one of the finest applied mathematicians of this century [3].

It is these three dimensions of Geiringer's career in the 1920s which are of considerable general historical importance, and the main goal of this paper is to provide an account of them. Although interesting in its own right, the mathematical content of Geiringer's two "Habilitationsschriften," especially her contribution to the theory of Charlier series in statistics, is discussed primarily with respect to these more general questions.

GEIRINGER'S HABILITATION

On April 22, 1940, the renowned Polish–British–American statistician J. Neyman (1894–1981) responded to an inquiry of Warren Weaver, director of the Rockefeller Foundation, concerning Hilda Geiringer, who had just sought refuge in the U.S.:

> Whether she is to be considered outstanding in ability or not, depends on the standard of comparison. Among the present day mathematicians there are few, whose names undoubtedly will remain in the history of mathematics . . . . As for the newcomers in this country, I have not the slightest doubt that von Mises is one of the men of such caliber . . . . There will perhaps be a dozen or perhaps a score of such persons all over the world . . . and Mrs. Geiringer does not belong in this category.
>
> But it may be reasonable to take another standard, that of an university professor of probability and statistics, perhaps an author of now numerous books on statistical methods. In comparison with many of those people Mrs. Geiringer *is* an outstanding person and I think it would be in the interest of American science and instruction to keep her in some university. [BUCB]

It should be noted that Neyman was referring to Geiringer's statistical work exclusively, whereas her main mathematical achievements seem to belong to the theory of plasticity. It was Leopold Schmetterer (born 1919) who, on the occasion of Geiringers doctorate-jubilee in Vienna 1967, mentioned the "fundamental Geiringer equations" in plasticity [Schmetterer 1967, 6].

In spite of Geiringer's indisputable importance and Neyman's support, she did not get an adequate position in the United States [Binder 1992]. And just as 15 years earlier it was her devotion to Richard von Mises that influenced the course of her career, this time, however—under the even more complicated conditions of emigration—it resulted in serious personal sacrifices. In order to live close to Cambridge, where von Mises, her husband since 1943, held a chair at Harvard University, Geiringer accepted an appointment at Wheaton College in Norton (Massachusetts) where, as she complained, "die Studentinnen definitely more socially als scientifically minded sind" [Binder 1992, 31]. After von Mises' death in 1953 she devoted her career almost exclusively to the edition of her husband's works, especially his "Mathematical Theory of Probability and Statistics" [von Mises 1964].

This tendency to sacrifice her own career in favor of von Mises' may be responsible for the fact that 20 years after Geiringer's death no comprehensive account of her merits has yet been given [4].

In order to understand Geiringer's extraordinary feelings of indebtedness to von Mises it is necessary to go back to the beginnings of Geiringer's relations with Richard von Mises in the early 1920s.

Hilda Geiringer was born in 1893 in Vienna, and took her doctorate from the University of Vienna in 1917 with a thesis on Fourier series in two variables. The reader ("Gutachter") was Wilhelm Wirtinger (1865–1945). Through her schoolmate Gerda Laski, who worked as a physicist in Heinrich Rubens' institute in Berlin [5], Geiringer obtained an assistantship at von Mises' new institute in 1921. In her "Mathematische Entwicklung" Geiringer writes:

> Mises teilte mir gleich mit, ich könne nicht damit rechnen, mich zu habilitieren. Er wünschte dringend, daß ich mich der 'angewandten' Mathematik zuwende, meinte, dann könne er mir wissenschaftliche Anregungen geben. Er begründete dort ein gutes Programm der angewandten Mathematik, und ich hörte alle Vorlesungen von Mises . . . . Meine Aufgabe war vor allem, ein 6-semestriges 'Praktikum' einzurichten und zu halten. [ME, 33/34]

Geiringer then refers to her short marriage to the statistician Felix Pollaczek (1892–1981); her papers betwen 1923 and 1934 appeared under the hyphenated name Pollaczek-Geiringer. (For the sake of simplicity this article refers to Geiringer under her maiden name, which she resumed after 1934.) The following quotation reveals Geiringer's active part in her personal relation with von Mises:

> Im Jahre 1921 heiratete ich Felix Pollaczek, ein ausgezeichneter Mathematiker. Ich will von unserer persönlichen Beziehung hier nicht sprechen, weil es zu kompliziert ist. Noch 1922, aber spätestens 1923, ließ ich mich von Felix scheiden, da ich Mises lieber hatte als ihn. Von 1923 an war ich Mises sehr nahe und sowohl Gerda als auch die Wiener Freundin traten in den Hintergrund. Doch liebte ich ihn sicher mehr als er mich. [ME, 35]

In 1922 Geiringer's daughter Magda was born in Berlin. Geiringer reports on her problems in dealing with all these personal and professional demands at the same time: "Ich war nachher einige Monate in Wien, da das Leben in Berlin unendlich schwer war. Mama war so gütig, Magda für eine Zeit (wohl nur einige Monate) zu behalten." [ME, 35]

Geiringer's scientific relations with von Mises were not unproblematic either. Referring to von Mises' comments on her paper [Pollaczek-Geiringer 1923] Geiringer wrote

> Ich hielt diese Arbeit eigentlich immer für ganz interessant. Aber sie war es vielleicht nicht. Irgendwann um diese Zeit sagte Mises, daß ihm scheine, als ob ich nicht imstande sei, mich in irgendetwas wirklich einzuarbeiten . . . . Es ist auch zu bemerken, daß Mi[ses] in Frank/Mises [i.e. [Frank & Mises 1925/1927]] diese Arbeit nicht zitiert, obgleich sie dem Sinn nach hineingehören würde. Entweder hat er—der mich ja wissenschaftlich am besten kannte,—wirklich wenig von mir gehalten und im speziellen von dieser Arbeit, oder sein Urteil war getrübt durch unsere persönliche Beziehung. [ME, 35]

Although von Mises, as seen above, did not initially encourage Geiringer's aspirations for Habilitation, his attitude seems to have changed sometime around 1925.

On July 18, 1925, Geiringer applied for Habilitation at the Philosophische Fakultät of the University of Berlin and submitted a paper on statics, "Über starre Gliederungen von Fachwerken." The faculty chose Richard von Mises and Ludwig Bieberbach (1886–1982) as readers of this "Habilitationsschrift." In his nine-page review ("Gutachten") of November 16, von Mises made some more general remarks concerning the critical situation of applied mathematics with respect to personnel in Germany:

> Es ist jedem, der die heutige mathematische Situation in Deutschland übersieht, bekannt, wie außerordentlich gering die Zahl der Gelehrten ist, die auf dem Gebiet der angewandten Mathematik produktiv tätig sind. Erledigte Lehrstühle können nicht wieder besetzt werden. Die an kleineren Hochschulen einige Jahrzehnte hindurch bestanden haben, gehen ein . . . . So wüßte ich unter allen jüngeren Leuten, die in den letzten fünf Jahren in Deutschland mit eigenen Arbeiten auf dem Gebiete der angewandten Mathematik hervorgetreten sind, keine zwei zu nennen, die ich hinsichtlich ihrer Eignung für eine Dozentur in diesem Fache Frau Dr. Pollaczek zur Seite stellen könnte. [BA1, fol.258]

Von Mises stressed the special features of the field of "applied mathematics" when he remarked that "es sich dem Wesen nach . . . um eine Habilitation für angewandte Mathematik handelt, auch wenn die Fakultät ihrer Übung gemäß die venia 'für Mathematik' schlechthin bezeichnen sollte." [BA1, fol.257]

Partly a sign of professional self-confidence, this quotation should be understood mainly as a preventive measure against possible objections on the part of the "pure" mathematicians at Berlin. As a matter of fact, the "venia legendi" for "mathematics" included "applied mathematics" but the latter title did not qualify a person to teach courses in pure mathematics [6]. Also, in her "Mathematische Entwicklung" Geiringer reinforced the commonly held view regarding pure and applied mathematics in the following self-assessment, in which she referred to her colleagues at Berlin university: "Die meisten dieser 'reinen' waren wesentlich begabter als ich; die 'angewandten' waren mehr von meinem Niveau, obwohl Collatz und Schulz gründlicher waren." [ME, 34]

Von Mises summarized the mathematical content of Geiringer's Habilitationsschrift in the following words: "Die vorliegende Arbeit gibt zum erstenmal, u[nd] zw[ar] sowohl für das ebene wie für das räumliche Fachwerk, die zugleich notwendige und hinreichende Bedingung der Brauchbarkeit an." [BA1, fol. 250]

Geiringer showed in the first part of her paper, which was published later as [Pollaczek-Geiringer 1927], that the absence of "accumulations" ("Anhäufungen," that is, superfluous connecting rods) in any partial system of *plane* frameworks is a necessary and sufficient condition for a framework with k nodes ("Knoten") and ($2k$-3) rods ("Stäbe") not to be "useless" ("unbrauchbar") solely due to its structure ("Gliederung"). "Uselessness" of the framework means that the tension-problem does not have a finite solution; i.e., there exists a finite or infinitesimal movability of the framework.

Geiringer tried to generalize the easily definable notion of "accumulation" in plane frameworks (i.e., existence of more than ($2k$-3) rods in a partial system of k nodes) to the case of *spatial* frameworks, introducing the notion of "Quasianhäufung."

At this point, some problems of historical judgment have to be mentioned. As a matter of fact the original version of Geiringer's Habilitationsschrift obviously has not been preserved in [Pollaczek-Geiringer 1927] [7].

There is no doubt, though, that Geiringer's theorem was wrong in the case of *spatial* frameworks, and the notion of "Quasianhäufung" was therefore irrelevant. Erhard Schmidt (1876–1959), although not appointed as a reader, was also interested in problems of applied mathematics [8] and read Geiringer's Habilitationsschrift. In a tactful manner he informed Geiringer of her mistake: "Daß das Resultat auch algebraisch nicht trivial ist, sieht man aus dem nicht-Gelten im Raum, auf das Erhard Schmidt mich an einem unvergeßlich schrecklichen Nachmittag (obgleich er sich so vornehm und ritterlich wie möglich benahm und mir . . . Thee servierte) aufmerksam machte." [ME, 47]

In an additional review of Geiringer's paper Issai Schur (1875–1941) also confirmed the incorrectness of Geiringer's result [BA1, fol.247].

This came as a reaction to Bieberbach's report of March 4, 1926, which was much more severe than Schmidt's and Schur's criticisms [BA1, fol.261–263]. Bieberbach wrote that he had gotten a "truly shattering impression" ("wahrhaft niederschmetternden Eindruck") of Geiringer's "purely mathematical abilities and achievements" ("rein mathematischen Fähigkeiten und Leistungen"). The second part of Geiringer's paper was—according to Bieberbach—a "collection of mistakes" ("Fehlersammlung"). He, therefore, would not approve of Geiringer's admission to any further stages of the Habilitation procedure as long as the problem of the exact specification of Geiringer's venia legendi was not yet resolved.

After this Geiringer withdrew the second part of her paper. In a later publication [Pollaczek-Geiringer 1932] she restricted the discussion to "a wide class of usable spatial frameworks," namely frameworks with triangles as boundary surfaces.

For her Habilitation Geiringer decided to submit a new paper on "The Poisson distribution and the development of arbitrary distributions" half a year later, on November 16, 1926. This is essentially [Pollaczek-Geiringer 1928a], although the title as quoted in the Berlin University files suggests [Pollaczek-Geiringer 1928b]. This conclusion follows from the subsequent reviews as well as from Geiringer's "Mathematische Entwicklung":

> Da mir die Lösung der Brauchbarkeitsaufgabe für räumliche Fachwerke nicht gelungen war, mußte ich, um mich zu habilitieren, noch eine Arbeit einreichen. Dies war die unter Druck gemachte Arbeit . . . "Die Charliersche Entwicklung willkürlicher Verteilungen," mit der ich mich wieder der Wahrscheinlichkeitsrechnung zuwandte. [ME, 51]

In order to make clear the goals and content of this paper the following remarks are necessary.

Richard von Mises considered Fechner's "Kollektivmaßlehre" as an important historical root of his own aspirations for an unification of the mathematical theories of probability and statistics with particular emphasis on the notion of relative frequency. In his 1931 book on probability and its applications in statistics and physics von Mises wrote:

> Stemming from the needs of practice, from problems of statistics and of the insurance business, a new theory emerged, apparently *alongside* the theory of probability, as its empirical counterpart, which Theodor Fechner called "Kollektivmaßlehre." Subsequently the astronomer Heinrich Bruns (1848–1919) tried to unite both theories at least as teaching subjects. [Von Mises 1931, 4]

Within the chapter "Beschreibende Statistik" ("Descriptive Statistics"), which constitutes, according to von Mises' book, "in some sense a preparatory chapter to 'theoretical statistics,' which is based on the theory of probability" [Von Mises 1931, 233], von Mises also discussed the so-called "Bruns' series". These are expansions introduced by Bruns in 1906 "of the 'Summenfunktion' [i.e., the distribution function] in an infinite series analogous to the Fourier expansion of an arbitrary function in certain fundamental functions" [Von Mises 1931, 250], the coefficients being certain linear functions of the moments of the given distribution.

Inspired by Bessel, Bruns chose as fundamental functions the integral (distribution function) of the Gaussian normal density function $\varphi(x) = (1/\sqrt{2\pi})e^{-x^2/2}$ and the integrals of its successive derivatives. Bruns' expansions applied to continuous distributions and to discrete distributions with a finite number of values (i.e., "frequency distributions"). Geiringer considered the use of $\varphi(x)$ as a comparative function for "arbitrary" distributions of this kind to be the "main idea of Kollektivmaßlehre" [Pollaczek-Geiringer 1928b, 301]. In his book von Mises recommended the so-called "Stetigkeitssatz des Momentenproblems" of G. Polya (1887–1985), dating from 1920 [Polya 1920], as a "deep mathematical theorem" for proving expansions of this kind since it allows for a "complete characterization of a distribution by its moments" [Von Mises 1931, 249].

Geiringer's paper on Charlier series [Pollaczek-Geiringer 1928a] is to be judged against this background. In a footnote she acknowledges her indebtedness to von

Mises, who called her attention to the papers of the Swedish astronomer Carl Ludwig Wilhelm Charlier (1862–1934) and to Polya's theorem as a method of proof [Pollaczek-Geiringer 1928a, 98]. The expansions proposed (but not proven) by Charlier in 1905, which were expansions of discrete distributions with an *infinite number* of values into series formed by successive derivatives with respect to the *parameter* of the Poisson-distribution, were a counterpart to Bruns' series. Charlier's expansions are sometimes called B-series and those of Bruns A-series. (The notion of an A-series sometimes also includes expansions on the level of the density functions, in the case of continuous distributions.) Von Mises saw the justification for Charlier's approach in the fact that both the Poisson distribution and the normal distribution are limits of the binomial distribution [Von Mises 1931, 265].

Geiringer's interest in Charlier series and in the question, still unsettled in 1926, of expandability conditions for arbitrary discrete distributions (on the domain of nonnegative integers 0, 1, 2, . . .) may have been stimulated by the topic of her Vienna dissertation of 1917 on "Trigonometrische Doppelreihen." As a matter of fact, the successive derivatives of the Poisson distribution,

$$\psi_0(x) = (a^x/x!)\, e^{-a} \qquad (x = 0, 1, 2 \ldots),$$

constitute an orthogonal system of functions; they are obtained by multiplication by the likewise orthogonal "Poisson–Charlier polynomials," which are still important in current probability theory [Pollaczek-Geiringer 1928b, 302]:

$$\psi_n(x) = p_n(x) \cdot \psi_0(x) = \frac{d^n}{da^n}\psi_0(x)$$

$$\sum_{x=0}^{\infty} \psi_m(x)\psi_n(x)\,(1/\psi_0(x)) = 0 \qquad (m \neq n)$$

$$= n!/a^n \qquad (m = n).$$

The Poisson–Charlier polynomials are closely related to the Laguerre polynomials. Gabor Szegö (1895–1985), who did fundamental work on orthogonal polynomials during his time as a Privatdozent in Berlin (1922–1926), found a condition in 1926 for the expandability of distributions in Charlier series, based on Hilbert's "Methode der unendlichvielen Variablen" [9]. Because Szegö's condition was less restrictive and more lucid than Geiringer's (cf. below [13]), her result became obsolete practically the moment it was published, and Geiringer referred to Szegö's condition with his permission [Pollaczek-Geiringer 1928a, 110]. Von Mises, in his book of 1931, did not even mention Geiringer's condition. Nevertheless, Geiringer's paper inspired Erhard Schmidt to undertake an investigation in 1928 of function-theoretic methods to determine necessary and sufficient conditions for the convergence of Charlier series [Schmidt 1928, 1933]. Geiringer referred to this fact in her "Mathematische Entwicklung" as follows:

> Dann kam noch ein interessantes Nachspiel. Der große Erhard Schmidt hatte seit Jahrzehnten

> (Tod seiner Frau) nicht veröffentlicht [10], obwohl er sich im Kolloquium etc. als unendlich scharfsinnig zeigte. Da er nun 2. Referent meiner Arbeit war [he was, in fact, an additional reader], begann das Problem ihn zu interessieren und er fand die *notwendigen und hinreichenden* Bedingungen für die Konvergenz, was natürlich großes Aufsehen machte. [ME, 52]

With Schmidt's result the *theoretical* problem—at least with respect to ordinary convergence [11]—of the convergence of Charlier series was settled. As to the *applicability* of Charlier's theorem within mathematical statistics, authors such as [Boas 1949a,b], [Cramér 1972], and [Kendall & Stuart 1963] stress the importance of the question of whether a *finite* number of terms in Charlier's expansion gives a sufficient approximation to the distribution. This is all the more important, since in general, "we cannot discuss the question of convergence or divergence without supposing that all moments have known finite values" [Cramér 1972, 206].

Geiringer's paper supplies no means for deciding this question of finite approximation. In general the B-series, sometimes also named after J. P. Gram (1850–1916) and Charlier, have proved to be of rather limited use in statistics [Kendall/Stuart 1963, 163].

While Geiringer's publication [1928a]—in contrast to some of her other contributions to probability theory and statistics [12]—had only a limited impact on mathematics, her application for Habilitation in 1926 was adversely affected by another circumstance: the incorrectness of her method of proof.

As reported above in her own words, Geiringer wrote the second part of her Habilitationsschrift "under pressure." Using Polya's theorem, "one of the few theorems I knew," she did not realize the restrictions of its applicability:

> Die Arbeit hat eine peinliche Geschichte. Ich wollte den "Stetigkeitssatz des Momentenproblems" benutzen, weil das einer der wenigen Sätze war, die ich kannte. Beim Beweis machte ich aber einen Fehler bezüglich Konvergenz einer Hilfsreihe—ich weiß nicht einmal mehr was es war—und es wurde von mir oder Mises oder E. Schmidt bemerkt, was schon wirklich ein Skandal war nach dem (immerhin noch ehrenvollen) débacle mit den Raumfachwerken. [ME, 52]

To give an idea of this mistake, some remarks are necessary concerning the decisive method of proof of Polya's theorem on moments [Polya 1920]. This theorem requires that the moments

$$M^{(t)} = \int_0^\infty x^t G(x)\, dx \qquad (t = 0, 1, 2, \ldots)$$

of a given *nonnegative* function $G(\mathrm{x})$, defined for all nonnegative real x, satisfy the condition

$$\left| \frac{\sqrt[t]{M^{(t)}}}{t} \right| < K, \qquad \text{if } t \text{ is large enough.} \qquad (*)$$

Then the convergence of the moments $M_n^{(t)}$, which are defined for a sequence of *nonnegative* functions G_n, to the moments $M^{(t)}$ of G, i.e., the condition that

$$\lim_{n\to\infty} M_n^{(t)} = \lim_{n\to\infty} \int_0^\infty x^{(t)} G_n(x)\, dx = M^{(t)}, \tag{**}$$

allows Polya to demonstrate the uniform convergence of the *indefinite* integrals of G_n to the *indefinite* integral of G, provided (an additional problem, which Geiringer did not discuss) the improper integrals $M_n^{(t)}$ *exist*, that is,

$$\lim_{n\to\infty} \int_{x_1}^{x_2} G_n(x)\, dx = \int_{x_1}^{x_2} G(x)\, dx.$$

In order to apply Polya's theorem to *discrete* distributions, Geiringer had to detour around the "summed-up" density functions, i.e., the distribution functions. Due to the special character of the "derivatives" of the Poisson distribution she obtained very simple expressions for the moments of the *differences*. $S_n(x) - \psi(x)$ and $V(x) - \psi(x)$, where S_n, V, and ψ denote the distribution functions of, respectively, the partial sum s_n of the Charlier series, the given discrete distribution V, and the Poisson distribution ψ_0 [Pollaczek-Geiringer 1928a, 106]. Geiringer also showed that the moments of these difference-functions fulfill Polya's conditions (*) and (**).

Geiringer missed the point, however, that Polya's theorem requires all involved functions to be *nonnegative,* which is generally not the case for these difference-functions.

While von Mises did not notice this mistake, as is clear from his report of November 12, 1926 [BA1, fol. 259/60], it did not escape the attention of the second reader, Ludwig Bieberbach. Again casting serious doubts on the mathematical abilities of the candidate, Bieberbach, in his undated review, revealed his rather condescending views toward the field of "applied mathematics":

> Soweit aber die Zulassung zu einem etwa ad hoc neu zu schaffenden Fach "Anwendungsgebiete der Mathematik" in Frage kommt, möchte ich dem verantwortlichen Urteil des Herrn ersten Referenten kein Gegenvotum entgegenstellen, zumal ja für die Beurteilung des Wertes einer Leistung im Rahmen eines Anwendungsgebietes noch andere als mathematische Fähigkeiten in Frage kommen, die sehr wohl Mängel in mathematischer Hinsicht ausgleichen können, Fähigkeiten, über deren Vorhandensein ich ein eigenes Urteil nicht besitze. [BA1, fol. 266]

The repeated mistakes of Geiringer resulted in the faculty's decision to solicit two additional reports from Issai Schur and Erhard Schmidt. Both readers basically supported Bieberbach's position, though without displaying a similar rudeness in their choice of words, and favored the creation of a special field for Habilitation called "Applied Mathematics." The final decision to grant Geiringer the Habilitation for this field became possible because of an addendum submitted by the candidate [13]. Geiringer, in her "Mathematische Entwicklung," refers to her indebtedness to Mises for finding this mathematical condition which saved the situation. At the same time she shows her awareness of the fact that her original mistake restricted the field of Habilitation:

> Der Fehler wurde durch fieberhafte Arbeit von Mises in Ordnung gebracht und die Arbeit

präsentiert und angenommen (in einer sehr angesehenen Zeitschrift, der Skandinavisk Aktuarietidskrift (1928). Damals wurden Arbeiten, die von einer guten Schule kamen, nicht vom Herausgeber der Zs. beurteilt), und meine Habilitation ging durch, aber nur für 'Angewandte Mathematik'. [ME, 52]

IDEOLOGICAL AND DISCIPLINARY ISSUES SURROUNDING GEIRINGER'S HABILITATION

The question should be raised why Bieberbach showed such strongly antagonistic feelings throughout Geiringer's Habilitation procedure [14].

A particularly striking feature in Geiringer's memoir "Mathematische Entwicklung" [ME] is the fact that she never mentions Bieberbach, not even in connection with her Habilitation, although he was certainly the one who was primarily responsible for her troubles. Her assumption, quoted above, that the mistake was found "by myself, or Mises or Schmidt," is clearly erroneous, and yet it is impossible that she was unaware of Bieberbach's role, even if she never read his scathing reports. Moreover, the fact that Szegö's condition [9] was first mentioned in Bieberbach's report leads to the conclusion that Geiringer suppressed in her "Mathematische Entwicklung" information about her relations with Bieberbach, perhaps because she simply found them too difficult to describe. A partial explanation for this may be that she, as a Jewish emigrée, wanted to exclude Bieberbach from her memoir because of his role as the leading Nazi among German mathematicians after 1933 [15]. Still, this explanation is not completely satisfactory in view of Geiringer's close mathematical collaboration with another former Nazi, Erhard Tornier (1894–1982), after World War II [16]. In any case, one cannot ignore the fact that precisely during the time of Geiringer's Habilitation there must have been contacts between Geiringer and Bieberbach on quite another level.

In 1926 the Teubner publishing house (Leipzig and Berlin) announced in a flyer that Hilda Pollaczek-Geiringer's translation of "L'idéal scientifique des mathématiciens" of Pierre Boutroux (1880–1922) was "in press" ("unter der Presse"). When this translation appeared the following year in the series "Wissenschaft und Hypothese," Geiringer remarked in the preface: "I undertook this translation of the 1920 original at the suggestion of Professor Dr. Bieberbach, who also kindly examined the manuscript." [Boutroux 1927]

Bieberbach's lively interest in Boutroux's book has been documented already in [Mehrtens 1987, 206]. On February 15, 1926, Bieberbach addressed the Verein zur Förderung des mathematischen und naturwissenschaftlichen Unterrichts (Förderverein) with a lecture, entitled "Vom Wissenschaftsideal der Mathematiker." In this talk Bieberbach emphasized "intuition" as a decisive source of mathematical thinking. While inspired by Boutroux in this respect, Bieberbach went considerably further, alluding directly to Brouwer's philosophy of mathematics [17].

How should this scarcely documented [18] "collaboration" between Geiringer and Bieberbach at the very time of her Habilitation be understood? I shall try to give a tentative explanation. Certain facts seem to indicate that the origin of this "collaboration" was an ideological discussion between Bieberbach and Geiringer which was in some sense connected with Geiringer's application for Habilitation. Against this background Geiringer's translation and her grateful acknowledgment of Bieberbach's role in the preface to the book could be interpreted as an attempt to reconcile the conflicts that arose during her Habilitation procedure.

As a matter of fact, in his review of Geiringer's first Habilitationsschrift, Richard von Mises alluded to some "pedagogical and popular writings" of the candidate, which, in von Mises' opinion, "included some immature judgments which can only be understood as a result of the intellectual situation immediately after the war." [BA1, fol.257]

That von Mises alluded to these papers of Geiringer's at all suggests that he was preparing a defence strategy for his candidate, against the possibility that some colleagues, especially Bieberbach, might react adversely to this kind of political and philosophical writing [19]. That there were real reasons to fear such troubles can be seen clearly from negotiations surrounding the appointment of Hans Reichenbach (1891–1953) at the University of Berlin, which were taking place at the same time as Geiringer's Habilitation. In the course of these negotiations Bieberbach remarked on January 15, 1926—that is, two months before his first report on Geiringer's Habilitationsschrift and one month before his talk at the Förderverein—with respect to Reichenbach's booklet "Student und Sozialismus" of 1920 that "it lacks the objectivity and the appropriate tone to be expected of a scholar." [Hecht/Hoffmann 1982, 654]

What was the "tone" of Geiringer's writing immediately after World War I and the November revolution?

In 1922 Geiringer's booklet "The World of Mathematical Ideas" [Geiringer 1922b] appeared in a series which was largely inspired by the developing movement of adult evening classes ("Volkshochschulwesen") after the war. In her book Geiringer, who once attended Freud's lectures in Vienna [Geiringer 1967, III], strongly emphasized the standpoints of psychoanalysis and of Mach's empiricism. Von Mises, a noted Mach specialist, reviewed Geiringer's book very favorably in his journal *Zeitschrift für Angewandte Mathematik und Mechanik* (*ZAMM*), recommending it as "worthy of being distributed, and above all, being read" [Von Mises 1922]. As a matter of fact, this booklet of Geiringer's seems to have been a kind of icebreaker in their personal relations: "Am Bemerkenswertesten finde ich, daß Mises, den ich damals noch nicht gut kannte (Aber ich war 1922 schon bei ihm in Berlin) und der ein sehr strenger Kritiker war, darüber eine sehr gute Kritik schrieb." [ME, 27]

Geiringer's book includes passages such as the following, which may have provoked Bieberbach's interest, in the event—indubitable to this author—that he read the book:

> Without any doubt the beginnings as well as the further development of mathematics were always influenced by both driving forces, a biological—economical ("external") and a psychological ("internal"). It is still a question whether psychoanalytical investigations can shed light on those internal forces and may thus explain the almost mystic enchantment which lies in just those mathematical problems most remote from reality. [Geiringer 1922b, 160]

Curiously enough, the very fact that Geiringer and Bieberbach shared a common interest in disclosing the psychological–internal dimension of the history of mathematics may well have been a source of tension between these two Berlin mathematicians. In fact, Mach's philosophy had but limited influence on German scientists [Holton 1992, 49], and Bieberbach, unlike von Mises, had no connections with the Berlin circle of empiricist philosophy. Also Freud, Geiringer's other main source, probably did not appeal to Bieberbach, who turned toward a different kind of psychological theory, the typology of E. R. Jaensch, toward the end of the 1920s [Mehrtens 1987]. Finally, Bieberbach was very touchy and at times self-righteous [Biermann 1988, 220]. It may have been in the context of a discussion of Geiringer's booklet that Bieberbach drew her attention to Boutroux's book, which she did not cite in her publication of 1922. Instead, Geiringer quoted A. Bogdanov's "Science and the Working Class," commenting on it in the following words: "The author develops some interesting ideas concerning the origins and goals of science, basing his discussion on Marx' theory and on the ideology of class struggle." [Geiringer 1922b, 196]

In spite of a considerable revision of previous positions, Geiringer maintained some strong views from a 1920 speech on "Reflections on the Teaching Method at Adult Evening Classes." In this talk Geiringer called "our entire science alien to the people . . . the class-bound product of a tiny minority . . . full of pseudo-knowledge" [Geiringer 1920, 100], and she called for a "real socialization of the mind."

Passages like this no doubt had the potential to antagonize scholars such as Bieberbach, who, for all his sometimes unconventional behaviour, remained at that time in the spirit of the German "Bildungsbürgertum." It is also clear that at least Geiringer's earlier paper of 1920 expressed a mood of departure from the present conditions and a longing for another world, a sentiment very similar to that expressed in Reichenbach's "Student und Sozialismus" of the same year [20].

In the above mentioned negotiations on Reichenbach's appointment, Bieberbach—partly supported by a review by Hermann Weyl—tried to dismiss Reichenbach's epistemological papers as superficial ("Arbeiten eines Popularphilosophen" [BA2, fol.327]) as well.

Therefore the conjecture may be allowed that in Geiringer's case it was Bieberbach who saw a connection between the political and scientific sides of her personality, and this may partly explain his extreme reactions throughout Geiringer's Habilitation procedure. For lack of documentary evidence it remains an open question to what extent additional ideological factors, anti-Semitism or sexism, were possibly involved.

Before Geiringer obtained the Habilitation, some additional quarrels between von Mises and "pure" mathematicians in Berlin seem to have taken place [Biermann 1988]. Von Mises, of course, must have felt humiliated by Bieberbach's reports, which at the very least showed that von Mises had not read Geiringer's papers carefully enough. Occasional "blunders" in his own writings, to which

Ostrowski referred in his talk, may have been another reason for the fact that von Mises was never proposed by any of his three prominent colleagues as a member of the Prussian Academy of Sciences [Biermann 1988, 201]. Although there seems to have been no open confrontation [19], the somewhat different interests and values of the fields of pure and applied mathematics at least remained a source of dispute.

Von Mises' statement on November 25, 1926, concerning the Habilitation of Georg Feigl (1890–1945), who had been substituting for Erhard Schmidt by giving introductory mathematical lectures for several years, cannot be understood except against the background of the Geiringer controversy at the same time. Von Mises wrote:

> Neither the reviews of the Habilitationsschrift nor my personal acquaintance with the candidate allow the conclusion that he has achieved "outstanding" contributions according to our stipulations. Opinions of other mathematicians whom I have asked privately for information conform that the candidate's papers hardly reach the average of current achievements in this field. Nevertheless I do not want to oppose the unanimous vote of the three representatives of pure mathematics, because in my opinion it is the specialists who are the most responsible for a Habilitation in their particular field. [Biermann 1988, 206]

It cannot be denied that von Mises' judgment with respect to Feigl was correct [21]. Moreover, von Mises' statement points to a real conflict of interests between two fields of teaching and research. Both Feigl and Geiringer were important figures in the teaching of pure and applied mathematics, respectively. Due in part to an overburden of teaching duties, their research had some defects. It is therefore understandable that the representatives of pure mathematics in their reviews of both Geiringer's and Feigl's Habilitationsschriften stressed the strength of the methods they employed and did not go into the importance of the results. Thus, for instance, Erhard Schmidt emphasized Feigl's systematic proofs of certain "famous fundamental theorems of topology," which "generally are considered true although their proofs have not been checked so far" [BA1, fol.66]. Von Mises, on the other hand, rightly stressed the competence of the "specialists" ("engste Fachvertreter").

It was only on November 11, 1927, that the "venia legendi" was officially bestowed on Hilda Geiringer. After the Berlin mathematicians had settled the question of the proper delimitation of the fields in which she was to be granted the permission to teach, the remaining parts of the Habilitation procedure were merely formal:

> Nun jedenfalls wurde ich zum mündlichen Kolloquium zugelassen (ein Parterre von Königen, außer den Mathematikern die weltberühmten Physiker Planck, Laue, . . .) was eine Formalität war, und so wurde ich 1927 Privatdozentin für A[ngewandte] M[athematik]. Dies hat mir noch nach Jahrzehnten entscheidend genützt bei der Zuerkennung meiner Professoren-Pension. [ME, 52]

Richard von Mises, on the other hand, may well have viewed this recognition of "applied mathematics" as a separate field of instruction with mixed feelings.

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This paper is a revised and considerably extended version of a talk given at the Oberwolfach meeting of historians of mathematics in 1988. I postponed publication because I hoped to find new archival evidence for the main theses of that talk, in particular drafts of Geiringer's "Habilitationsschriften." Although these hopes were not fulfilled, I did find a handwritten "Mathematische Entwicklung" (German, 71 pp., around 1970) in the possession of Geiringer's daughter Magda Tisza in Chestnut Hill, near Boston. This is a remarkable document, which although concerned primarily with Geiringer's own mathematical career, sheds considerable light on the development of several fields of applied mathematics in this century and particularly on the contribution of Richard von Mises. The information in this document concerning Geiringer's personal and mathematical relations in 1920s Berlin with her future husband, von Mises, gave the original paper an additional dimension. In order to preserve the original form of the documents, I quote all unpublished sources without translation. All translations of quotations, which had been published before, are mine.

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NOTES

1. Ostrowski's judgment of von Mises' theory of probability can only be understood as strong, one-sided support for Kolmogorov's measure-theoretic approach. The allusion to the "tense" ("verkrampft") atmosphere in Göttingen seems to refer to the competitiveness there and the fact that Göttingen was ahead of Berlin with respect to some aspects of the modernization of mathematics.

2. A typical remark by her in this respect alludes to [Geiringer 1922a], which was published under the influence of L. Lichtenstein (1878–1933) during her time as his assistant at the "Jahrbuch über die Fortschritte der Mathematik": Von der Arbeit gilt m.E. wie von vielen meiner Arbeiten, daß sie besser ist als ich, d.h. als mein Verständnis. Da ich aber andererseits immer viel weniger "bekam," als was mir gebührte, so ist auch wieder eine Art Gerechtigkeit hergestellt." [ME, 32] As late as 1967, on the occasion of her doctoral jubilee in Vienna, Geiringer commented on her achievements: "Es waren vielleicht zu viele Gebiete und in keinem tief genug." [Geiringer 1967, III]

3. The study of her private utterances, especially her "Mathematische Entwicklung" [ME], led me inevitably to this conclusion; cf. the acknowledgments above.

4. [Binder 1992] and [Richards 1989].

5. Geiringer alludes in ME to her contacts in Berlin with some other Austrians, among them Lise Meitner: "Dort waren schon viele Wiener, vor allem am KWI in Dahlem. Die bedeutendste was Lise Meitner, die Gerda und mir sehr nahe stand." [ME, 33]

6. In 1928 von Mises, perhaps still partly under the influence of the quarrels surrounding Geiringer's Habilitation, insisted on restricting the venia of Robert Remak (1888–about 1944) to *pure* mathematics. Von Mises' objection, however, was not upheld, and Remak went on, in fact, to teach courses on actuarial mathematics [Biermann 1988, 210].

7. Neither in the Berlin University Archives, nor in Geiringer's partial estates at Harvard and in the possession of her daughter Magda Tisza, could either of the two parts of her Habilitationsschrift be found. As for the latter two locations, this is hardly surprising since academic theses at that time were generally still submitted as handwritten manuscripts leaving the applicant without any copies.

8. Schmidt's crucial role in the process of founding von Mises' institute, beginning in 1918, is reported in [Biermann 1988, 186].

9. Szegö's condition is

$$\sum_{x=0}^{\infty} v^2(x)/\psi_0(x) = e^a \sum_{x=0}^{\infty} v^2(x)\, x!/a^x,$$

$\psi_0(x)$ being the Poisson distribution, a its parameter, and $v(x)$ the given distribution.

10. This is a slight exaggeration, although, in fact, Schmidt's most important papers on integral equations dated back to 1905–1908. Between the death of his wife in 1918 and [Schmidt 1933], he published only six short mathematical papers.

11. Quadratic convergence was investigated by [Boas 1949a,b].

12. In [Pollaczek-Geiringer 1928b] she introduced the notion of a "zusammengesetzte Poisson-Verteilung." [Schmetterer 1967, 4] emphasizes Geiringer's contribution to mathematical genetics.

13. This addendum was a rather cumbersome additional restriction for the moments of the given distribution,

$$|\sqrt[t]{m^{(t)}}| < \frac{a}{e} \ln t',$$

with $t' \ln t' = t$, if t is large enough. This enables one to construct a nonnegative bound function for the difference functions, making the application of Polya's theorem possible. Mises and Geiringer's condition is stronger than Szegö's [9], as conceded by Geiringer in her publication.

14. It is the following somewhat speculative remarks of my 1988 talk concerning the political and ideological background that I could not confirm by additional documentary evidence.

15. Mrs. Magda Tisza, Hilda Geiringer's daughter, informed me that Bieberbach was always considered a friend before 1933. His sudden turn to national socialism in 1933 came to Hilda Geiringer and Richard von Mises, as well as to most of their colleagues, as a total surprise. Some irony but also bureaucratic thoughtlessness can be seen in the fact that Geiringer had to rely on Bieberbach's testimony after the war to obtain a German pension to which her Habilitation entitled her (1957). In response to previous testimony of Bieberbach's, which led to a pension for Geiringer as von Mises' widow, she wrote him a letter, dated July 13, 1955, in which she stated: "Ich weiß, daß Sie meinem Manne freundschaftlich zugetan waren, und daß Sie es darum gerne taten." I thank Mr. Ulrich Bieberbach (Oberaudorf) for providing this information.

16. Geiringer acknowledged Tornier's crucial part in preparing the edition of [Von Mises 1964] on page vii.

17. Also, it would do injustice to the fine book of Boutroux to hold it responsible for Bieberbach's later racist aberrations.

18. In her "Mathematische Entwicklung" Geiringer, again, does not mention Bieberbach when she refers to her translation of Boutroux's book.

19. Von Mises' political position in the 1920s is in itself far from evident, and probably changed during the course of the decade. Von Mises sided with Bieberbach, Brouwer, and Erhard Schmidt in a nationalist campaign against the participation of German mathematicians in the 1928 Bologna congress, although participation was endorsed by Hilbert and the Göttingen mathematicians (cf. [Dalen 1990]

and [Mehrtens 1987]). On the other hand, in his philosophical positions von Mises seems to have differed considerably from the mainstream of German scientists.

20. Substantial personal contacts between Reichenbach and Geiringer at this time are unlikely, however, according to Geiringer's daughter Magda Tisza. There is no correspondence between the two, either in Geiringer's papers (Harvard and private) or in Reichenbach's (at the University of Pittsburgh).

21. In a letter to the author, Feigl's friend and colleague in the 1920s, Hans Freudenthal, wrote (September 30, 1984): "Ich betrachte es als ein Ehrenblatt für das Gremium der Berliner Mathematiker, daß man ihn ausschließlich auf Grund seiner didaktischen Qualitäten zur Habilitation zuließ. In Göttingen wäre das undenkbar gewesen."

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BA2: BA, Philosophische Fakultät 1241.

BUCB: Bancroft-Library, University of California at Berkeley, J. Neyman-Papers 84/30c, carton 47, folder 33.

ME: "Mathematische Entwicklung" (Manuscript of Geiringer's around 1970, 71 pages, handwritten German, in the possession of Mrs. Magda Tisza, Chestnut Hill, near Boston, USA).

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