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Pure mathematics applied in early twentieth-century America: The case of T.H. Gronwall, consulting mathematician

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Abstract

Thomas Hakon Gronwall (1877–1932) was a Swedish-American mathematician with a broad range of interests in mathematical analysis, physics, and engineering. Though he was primarily known for his results in pure mathematics, his career as a "consulting mathematician" in America from 1912 to his death in 1932 provides a backdrop against which one can discuss contemporary issues involved in the increasing application of mathematics to engineering, industrial, and scientific problems. This paper attempts a summary of his major mathematical contributions to industrial, governmental, and academic institutions while relating his often difficult life during these years.

Zusammenfassung

Thomas Hakon Gronwall (1877–1932) war ein schwedisch-amerikanischer Mathematiker mit briet-gefächertem Interesse an Analysis, Physik, und Ingenieur-Wissenschaften. Während er am besten bekannt ist für seine Ergebnisse in der reinen Mathematik, seine Karriere als "beratender Mathematiker" in Amerika von 1912 bis zu seinem Tod in 1932 bietet einen guten Hintergrund für eine Diskussion von Fragen der Angewandten Mathematik seiner Zeit. Diese Arbeit versucht Gronwalls Anteil an Fortschritten in industriellen, akademischen und Regierungs Bereichen zusammenzufassen, und gleichzeitig sein oft schwieriges Leben darzustellen.

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1. Introduction

On page 21 of the List of Officers and Members [1925–1926], of the American Mathematical Society of October 1926 [List of Officers and Members, 1925–1926, 21] one finds the entry "Gronwall, Dr. T.H. Consulting Mathematician. Care of Chemistry Dept., Columbia Univ., New York, N.Y." Sitting among the nearly 1700 entries for professors and instructors at colleges, universities, and high schools, and for employees of insurance companies, industries, and power companies are these two lines, a gateway to the life and work of an extraordinary mathematical talent which found expression in the years 1912 through 1932 in the United States.

The contributions of Thomas Hakon Gronwall [1877–1932] to pure mathematics are well known among specialists in the many fields in which he worked. He is responsible for the Area Theorem in univalent function theory, a classical upper bound on the growth of the divisor function in number theory, a summability method, several papers on the Gibbs phenomena, and an inequality known as Gronwall's Lemma in differential equations, to cite some of his best-known results. His talent for pure analysis extended over many fields, an achievement that was possible in, though by no means typical of, the era in which he worked.[1]

What is less well known, but of greater interest for the insights it gives into the American mathematical community in the first third of the 20th century, is his work in what might be called either applied or industrial mathematics.

The circumstances of his life gave rise to a nomadic existence in which Gronwall moved from one post to another in America, never staying anywhere for longer than two or three years, except for his final stop at Columbia University, with which he was associated for the last nine years of his life. If his achievements in pure mathematics, exemplified in his nearly 90 publications, are evidence of the relative independence of this work from the American mathematical community, the most illuminating work from the historical point of view is the handful of papers which relate to the stations of his life in his adopted country. These papers reflect the nature of the mathematical community and related institutions during what Parshall and Rowe have termed the third of four distinct periods shaping mathematics in America, the era (1900–1933) "during which the institutions and research traditions largely established in the previous era consolidated and grew" [Parshall and Rowe, 1994, 428]. In following the path of Gronwall's life one encounters such educational institutions as Princeton University, the University of Chicago, and Columbia University, and three of the six "towering figures" of American mathematics sketched by David Zitarelli [Zitarelli, 2001]. One comes across such firms as the Pennsylvania Railroad and U.S. Steel as well as those firms developing science-based research groups such as A.T.&T. and E.I. Du Pont de Nemours, Inc. One sees the members of the mathematical community making practical contributions to ballistics at Aberdeen Proving Grounds, as well as collaborating on pure science research at universities. Associations such as the National Academy of Sciences, the American Mathematical Society, and the Mathematical Association of America play a role in his life. The status of mathematics in such user groups as university engineering faculties, engineers themselves, and the National Bureau of Standards comes into view.

[1] For a summary of his life and work [Hille, 1932] is the most comprehensive treatment to date. Hille met Gronwall in 1921. This document lists Gronwall's published bibliography as well as the dates of his addresses to the American Mathematical Society and gives a detailed analysis of the significance of his pure mathematical work and a short biographical sketch. We will refer to this article as "Hille." Another good summary, in Swedish, is to be found in [Björk, 1946].

The goals of this paper are:

(1) to provide a more comprehensive biography of Gronwall than is available to date,
(2) to show how the posts of his life displayed the growing use of mathematics in industry, government, and science in America, and
(3) to show how the mathematical community had at this time become sufficiently consolidated to make efforts to take care of errant members.

These goals are approached by interspersing biography with papers of Gronwall whose contents reflect his current circumstances as well as they showcase his analytic abilities. These papers share one feature: they all display a concern for the processes involved in rendering mathematical expressions or equations computable. Numbers were needed by those for whom he consulted, numbers for ballistic trajectories, electrical measurements, stress ratios, and descriptions of electrolytes in solution. It is a remarkable feature of the papers discussed that they display a pure analyst's ability to manipulate integrals or differential equations at the same time that they deliver numerical results which could be entered into a machinist's handbook, a Bureau of Standards circular, or a firing range table. Analytical techniques which might under other circumstances be used to bound linear operators or provide growth bounds for functions are mustered in behalf of calculation.

The first and last of the six papers under consideration deal with the tools of computation themselves, the first discussing the analog tool of nomography and the last mentioning the digital calculating devices of the day. The four papers in between deal with applications of mathematics to the problems of interest to his current station in life. By considering them one is led to the conclusion that Gronwall lived in an era in which it was possible to pick up classical or recently constructed mathematical models, such as systems of differential equations or integrals, provide solutions, and have the results be considered of value to interested parties. It is to be understood that each of these papers could itself be the subject of a full length treatment; our discussions are necessarily incomplete. In particular, we do not go into depth on the reception and subsequent history of these contributions and how they fit in with current practices. It is sufficient for our purposes to note that they had perceived value at the time. But an attempt has been made to supply a context for each to give an idea of how Gronwall's contributions fit into the literature and problems of the day.

2. The rise of mathematics and the sciences in industrial America, 1900–1940

In order to understand Gronwall's contributions we shall attempt a brief overview of the increasing role played by mathematics and the sciences in industrial and governmental settings during this period. Such an attempt can make no claim to be a comprehensive study, but by choosing contemporary documents relevant to his efforts, we can put his work in context of the historical developments of the time.

In his study of invention in America in the period 1870–1970 [Hughes, 1998, 48], Thomas P. Hughes notes that "Before the rise, about 1900, of the industrial research laboratory ... the nation's technical inventiveness was concentrated in the independent inventors" such as Alexander Graham Bell and Thomas Edison. Such inventors "needed men trained in science and chemistry," though the relationship between the parties was often complex, sometimes strained. He continues, "... most of the independent inventors were mathematically unsophisticated." On the part of one independent, Edwin H. Armstrong, the pioneer of frequency modulation (FM), there was an active dislike of abstract mathematics [Armstrong, 1944].[2] Hughes's work details the arrival of a newer type practitioner of engineering exemplified by Charles Steinmetz, who proposed General Electric's research laboratory in 1900. Steinmetz was well versed in mathematics, physics, and engineering, and Hughes shows how this type of background began to become of importance in the development of the industrial laboratory.[3]

The increasing role of scientific research in engineering and industry at this time is illustrated on a small scale by the establishment of the Engineering Experiment Station in Urbana, Illinois in 1903. Its stated purpose [Moore, 1909, Introduction][4] was to "carry on investigations along various lines of engineering, and to study problems of importance to professional engineers and to the manufacturing, railway, mining, constructional and industrial interests of the state." Under the auspices of the University of Illinois "There will also be issued from time to time in the form of circulars, compilations giving the results of engineers, industrial works, technical institutions, and governmental testing departments." On a larger scale the *Bulletin of the Bureau of Standards*, which began publication in 1904, stated similar goals in its "Announcement" in the first issue: "The Bulletin of the Bureau of Standards, of which this is the initial number, will embody the results of its investigations, researches, and other work which may be of importance to the scientific, technical, and manufacturing interests of the country" [Bulletin of the National Bureau of Standards, 1904, 4].

Occasionally overtures from the mechanical and civil engineering communities were made to mathematicians. The role of mathematics in aiding these communities was the subject of "An Appeal to Producing Mathematicians" [Paaswell, 1914] in 1914, by one George Paaswell, C.E., who complained that "Hardly any [mathematical] treatise has attempted to discuss or analyze the serious problems of the applied science professions. The profession of civil engineering is teeming with problems awaiting the solution of a St. Venant, a Laplace, a Newton" [Paaswell, 1914, 128], he lamented. Citing the 1907 collapse of the Quebec Bridge, Paaswell states "The failure of the largest bridge in the world—the Quebec Bridge—was due to lack of knowledge of the action of large compression members (columns) and the only path open to engineers was that of experimentation on larger size test pieces: mathematicians had failed them" [Paaswell, 1914, 128].[5] He levels a criticism of engineering schools for their inadequate attention to higher mathematics—a criticism often stated in this period—and notes that many a branch of mathematical analysis could "add its quota to applied science" [Paaswell, 1914, 129]. Regarding computation, "... general analysis itself [could lend] to the reduction of the resulting expressions of stress analysis" [Paaswell, 1914, 129], presumably to aid in the design process.

The activities of scientists and mathematicians in World War I provided an opportunity for more general reflections on the place of science and mathematics education in preparedness for war and promotion of peacetime economic growth. In an address before the American Mathematical Society by the University of Chicago number theorist L.E. Dickson in 1918, a summary of the exterior ballistics work done at Aberdeen Proving Grounds by the group which included Gronwall was given, and Dickson declared

[2] This paper on the history of FM broadcasting criticizes A.T.&T. engineer John R. Carson's mathematical treatment of FM in 1922, which had found Armstrong's researches wanting.

[3] Steinmetz studied at the University of Breslau, and in 1887 obtained a scholarship which allowed him to continue working toward a doctorate. It appears that he nearly completed a dissertation in synthetic geometry under Heinrich Schröter [Kline, 1992].

[4] This paper is our example of a publication in the series of bulletins issued by the station; it will be considered below.

[5] The Quebec Bridge collapsed one more time after this appeal was written, in 1916.

that "While science has played an important role in this war, it would undoubtedly play a dominant role in a future war and no scheme of national preparedness will prove adequate which does not insure an ample supply of highly trained scientists and furnish to all men effective training in the fundamentals of exact sciences. Owing to its recognized value as a fundamental part of military education, I expressly include mathematics..." [Dickson, 1919, 289]. Dickson tied this summary in with an appeal for broader scientific training for future wars, claiming that such "scientific training here advocated as an essential part of national preparedness for war furnishes at the same time the surest means to retain and increase our material prosperity, to add to our health, comfort and conveniences..." [Dickson, 1919, 289].

By the end of World War I and into the 1920s many American corporations set up industrial laboratories in which basic research was done; the list of such corporations includes General Electric, Du Pont, General Motors, and American Telephone and Telegraph. The shift in industrial work to such facilities can be noted by quoting the Foreward to the first issue of the *Bell System Technical Journal* [Bell System Technical Journal, 1922, 1] in which the anonymous author states "Modern industry is characterized by the extent to which scientific research and technique based on precise study have contributed to its progress. So complete has been the adaptation of and reliance on scientific research in many industries that it is difficult at this time [1922] to visualize the state of affairs of two or three decades ago, when substantially all industry on its technical side was dependent for advancement on cut-and-dry, rule of thumb, methods of development" [Bell System Technical Journal, 1922, 1].

In 1924 one of the most prominent of A.T.&T.'s mathematical staff, George A. Campbell, read a paper entitled "Mathematics in Industrial Research" at the International Mathematical Congress in Toronto [Campbell, 1926]. Although subtitled "Selling Mathematics to the Industries," it in fact dealt more with selling the idea of industrial mathematics to pure mathematicians. Beginning with a quote from Francis Bacon on the indispensability of mathematics to the investigation of natural phenomena, he continued by listing the heat engine, the telephone, the radio, the airplane, and electric power transmission as "useful devices" whose development would have been impossible without mathematical support. He claimed that "Electricity is now preeminently a field for mathematics, and all advances in it are primarily through mathematics" [Campbell, 1926, 551], and went on to develop a case for the choice of problem-solving in industry as a career for budding mathematicians, citing specific examples of work at A.T.&T. He voiced ideas on the education of industrial mathematicians and noted that "Above all, industry needs mathematicians of an especially broad type—men whose interests naturally extend beyond their special field, and who are flexible enough to co-operate with non-mathematicians" [Campbell, 1926, 557].

Campbell emphasized a fact which is worth noting, for it serves to indicate a problem encountered in the movement from invention-based research to science-based industrial research. "It is characteristic of many problems encountered in industry that a great number of independent variables are involved, far too great a number for the best solution to be reached simply by trained judgement" [Campbell, 1926, 553]. He delivered a brief history of long-distance telephone cable development to illustrate this point. This insight was echoed by Gilbert A. Bliss, a University of Chicago mathematician, in a 1927 address to the American Association for the Advancement of Science on mathematics in industry [Bliss, 1927]. "I find that practising engineers, and members of engineering faculties, frequently show great reluctance in admitting that mathematics plays an essential role in engineering problems, though the books on engineering seem to tell a different story," he writes, and continues, "I was much interested recently to find in print an exposition of the engineer's mistrust of mathematics in the last chapter of a well-known book on the strength of materials... The keynote of the chapter is the adjuration to use common sense in avoiding mathematics where mathematics is inapplicable... But in deference to mathematics I should like to add one further principle. It is that common sense should also be scrupulously avoided in places where common sense does not apply. No amount of common sense unaided can predict the motions of the heavenly bodies, or construct a range table..." [Bliss, 1927, 316].[6]

By 1940 industrial mathematics was well enough established so that a 38-page report on its status, written by Thornton Fry of Bell Telephone Laboratories, could be written and published in the *American Mathematical Monthly* [Fry, 1941]. In this comprehensive work are listed characteristics of the point of view of the pure mathematicians, and the remark that "the typical mathematician described above is not the sort of man to carry on an industrial project. He is a dreamer..." [Fry, 1941, 4]. Fry goes on to specify how this frame of mind must be compromised, concluding that "...the mathematician in industry, to the extent to which he functions as a mathematician, is a consultant, not a project man" [Fry, 1941, 4]. At another point he notes that "...the type of mathematician who could not do a good engineering job if he turned his hand to it will not get on very well in an industrial career" [Fry, 1941, 5]. Among other thoughts expressed are the ideas that "...throughout the whole of industry, research is becoming more complex and theoretical, and hence the value of consultants in general, and mathematical consultants in particular, must increase" [Fry, 1941, 10], and that "mathematics frequently aids in promoting economy either by reducing the amount of experimentation required, or by replacing it entirely" [Fry, 1941, 21]. With regards to calculation: "...mathematics frequently plays an important part in reducing complicated theoretical results and complicated methods of calculation to readily available working form" [Fry, 1941, 26], and that devices aiding in this effort by saving labor "...are, in fact, examples of the use of mathematics to avoid the use of mathematics" [Fry, 1941, 26]. The long study concludes with the remark, "There was a day when, in engineering circles, mathematicians were rather contemptuously characterized as queer and incompetent. That day is about over" [Fry, 1941, 38]. Fry estimated that there were between 100 and 150 workers who fit this characterization of the industrial mathematician.

The role of mathematics in the development of physics and chemistry in America during this period is also relevant to Gronwall's story, but we shall limit ourselves to a few remarks. In a 1921 survey of "A Decade of Mathematics," Harvard analyst O.D. Kellogg remarks on the relative neglect of mathematical physics on the part of American mathematicians, saying that "...it does seem clear that a greater cultivation of this field in this country is most desirable" [Kellogg, 1921, 543]. He lists Gronwall as one of only four such Americans who have contributed to the field. Possible reasons for this neglect are discussed. A recent treatment of this issue [Servos, 1986][7] traces some of the problem to the lack of proper instruction in physics at the undergraduate level. This paper also contains a discussion of physical chemistry and cites a 1929 note on the teaching of chemistry by Farrington Daniels which argues towards the strong conclusion that "Inadequate experience in mathematics is the greatest single handicap in the progress of chemistry in America" [Servos, 1986, 628].

[6] Bliss also points out the relative paucity of applied mathematicians in America at this time, as well as the lack of an adequate school of applied mathematics. He also reiterates Paaswell's request for more usable forms for mathematical solutions to applied problems: "The further development of methods of computation which will make theoretical results of immediate service in the applications is highly to be desired. Until such methods are known we can not hope to convince fully the practitioner of the importance of these theories from his standpoint" [Bliss, 1927, 318].

[7] See also [Feffer, 1997].

3. From Sweden to mathematical debut in America

Gronwall was born Hakon Tomi Grönwall on January 16, 1877, son of “a gentleman farmer–engineer and a well-educated mother from Varmland” [Shohat, 1933, p. 125] at Dylta Bruk,[8] a parish of Axberg in Central Sweden. He attended Venersborg Highschool from 1887 until 1893, and then entered the University of Uppsala the same year. In 1894 he transferred to Stockholms Högskola, which was at this time a privately funded institution partially supported by the city of Stockholm. This establishment, which later became the University of Stockholm, was at this time a research center for mathematics and the natural sciences with a select student body and work directed primarily by Gösta Mittag-Leffler.[9] The Högskola had no degree-granting powers until a few years after Gronwall left; one needed to travel to Uppsala in order to be examined. Gronwall received a Bachelor of Arts from Uppsala in 1896 with a thesis in mathematics, and then a doctorate in 1898 on systems of linear total differential equations. He had published 10 papers, including the thesis, by age 21.[10]

There is in the Archives of the Mittag-Leffler Institute a collection of correspondence [Gronwall, T.H., letters to Gösta Mittag-Leffler] dating from the years 1897–1898 which indicates a period of tribulation for Gronwall. From the contents of these letters it appears that he was suffering from exhaustion and spent several months in the country at a farm in an attempt to recover. One remarkable feature of this arrangement was that it was financed by Mittag-Leffler himself, in monthly payments made through a third party. For this Mittag-Leffler received profuse thanks from both Gronwall and his father. Words such as “melancholy” and “nervousness” were used to describe Gronwall’s frame of mind at this time, and at one point Gronwall complained that he could work for no more than an hour without experiencing debilitating dizziness. Apparently he had also gotten in debt, and experienced bouts of intense doubt about his future. From this period he eventually recovered, and he abruptly left his place of convalesence without notice to his hosts. This incident is noteworthy for its revelation of unfortunate emotional difficulties, a condition which would later assert itself and perhaps, together with a certain degree of impulsiveness, was responsible for an itinerancy which was characteristic of his later life in America.

Upon receiving his doctorate Gronwall was faced with the fact that there were only four professorships in mathematics available in Sweden at that time[11] and there was much competition for them. Consequently he enrolled in the Royal Institute of Technology to broaden his career possibilities. However, in May of 1899 he and a friend had a party at which some damage to university property was done, as a result of which both were banned from the university for six months.[12] A strong element of pride, also an important aspect of Gronwall’s character, asserted itself in response to this, and he decided to leave Sweden and enroll in the corresponding school in Germany, the Charlottenburg Technische Hochschule in Berlin, where he received a degree in civil engineering in 1902. He practiced this vocation in Berlin until deciding to move to America in 1904.

8 The Swedish word “bruk” denotes a factory located in the countryside but surrounded by a village, many of whose inhabitants are employees of the factory. Gronwall’s father was the “bruksförvaltaren,” or manager, of the factory, though not an owner.

9 For the history of this institution and the research conducted there see [Domar, undated].

10 A discussion of these papers can be found in [Gaarding, 1998].

11 Communication to the author from Lars Gaarding, May 9, 2000.

12 More on this incident can be found in Gaarding’s book mentioned previously.

From 1904 to 1910 Gronwall worked at various steel and bridge-building concerns including Carnegie Steel, the Pennsylvania Railroad, and the American Bridge Company, the last a conglomerate of steel manufacturers formed by the American financier J.P. Morgan in 1900; in 1901 it became a subsidiary of United States Steel. His places of employment followed a westward trend[13] from Pittsburgh through the midwest to Chicago. About this period in his life Hille says "very little is known" [Hille, 1932, 775], and we have nothing to add, other than the obvious observation that such work clearly allowed Gronwall to experience certain American industries from the inside and acquaint himself with the needs and practices of engineers in that country.

It appears that in Chicago in 1910, or perhaps the year before, Gronwall rekindled his interest in pure mathematics. This coincided with a change in status from employee of the American Bridge Company to consulting engineer in the Chicago area, a title which he kept until 1913. The first expression of this interest the author has found is a letter to Leopold Féjèr, the Hungarian analyst, dated October 2, 1910, in which Gronwall discusses the trigonometric series $\sin(x) + (1/2)\sin(2x) + \cdots + (1/n)\sin(nx)$, a recent object of interest to Féjèr.[14] It would seem that Gronwall followed in the footsteps of G.D. Birkhoff, who several years earlier "made a first journey across the city of Chicago to the university, and found [his] way into the excellent mathematical library ... I remember the thrill which the sight of the well-filled shelves gave me," Birkhoff recalled [Birkhoff, 1938, 461]. A set of six mathematical papers appeared in 1912 issued under the name Thomas Hakon Gronwall with address given as Chicago, Illinois. He addressed the American Mathematical Society for the first time at its Chicago Section meeting on April 5 and 6, 1912, delivering two talks: "On a Theorem of Féjèr's and an Analogon to Gibbs' Phenomena" and "Some Asymptotic Expressions in the Theory of Numbers." The next month he became a member of the Society. Among his first six publications is a memoir on nomography, which we now discuss.

4. A contribution to nomography

Nomography[15] is the study and construction of graphical representations of mathematical relations for use in quick and repeated calculation. The subject has a history which extends as far back as the early part of the 19th century, and arose as the need for such devices was evident in engineering projects

[13] "The Princeton University Annual Reports of the President and the Treasurer for the Year Ending December, 1914" [Princeton University Annual Reports, 1914, 13] lists Gronwall's employment history as follows:

1904–1906 Carnegie Steel Company, Pittsburgh,
1906–1907 American Bridge Company,
1907–1909 Pennsylvania Lines West of Pittsburgh,
1909–1910 American Bridge Company, Chicago,
1910–1913 Consulting Engineer in Chicago.

One can note the itinerant pattern displayed here. Hille: "He was [at this time] apparently a rolling stone" [Hille, 1932, 775].

[14] Féjèr also reports that Gronwall communicated to him a proof of the nonnegativity of this series on the interval $0 < x < \pi$ for each n, and that Dunham Jackson, then working with Landau at Gottingen, reported the same result to him at a later date that year [Féjèr, 1952, 808]. The resulting positivity inequality is occasionally known as the Féjèr–Jackson–Gronwall inequality, though Gronwall's name is often omitted. Gronwall's 1912 paper [Gronwall, 1912a], in which this and other results were published was praised highly and explicated by Edwin Hewitt and Robert Hewitt in [Hewitt and Hewitt, 1979] A priority dispute involving Gronwall's paper is also discussed by the authors.

[15] For general discussions of nomography see [Evesham, 1986; Hankins, 1999].

such as the construction of the French railroad system in the 1840s. The devices produced featured most prominently the intersection and alignment nomograms. Restricting ourselves to mathematical relations involving three variables only, we may describe an intersection nomogram as follows: let $F(x, y, z) = 0$ be the mathematical relation, arising from a scientific or engineering problem, to be charted. The x and y values appear on scales on the horizontal and vertical axes, respectively, and the level curves of F are plotted. Thus if x and y values are fixed on their axes, and the intersection of the corresponding vertical and horizontal lines found, the level curve passing through this point is labeled with the corresponding value of z to be found. By allowing nonuniformity in the x and y scales, the level curves may be replaced by straight lines.

An alignment nomogram assigns a scale, perhaps curved, in the Cartesian plane to each of the three variables x, y, and z, and is constructed in such a manner that if the values of any two variables satisfying the relation $F(x, y, z) = 0$ are given, the third may be found by placing a straightedge at the appropriate locations on the scales of the two known variables; the intersection of the straightedge with the remaining scale occurs at the location of the value of the remaining variable satisfying the relation.

The alignment nomogram was an innovation of Maurice D'Ocagne dating from 1884. D'Ocagne apparently created the word nomography and was primarily responsible for organizing the various aspects of nomogram construction into a coherent body of knowledge, detailed in several books, the first dating 1891 [Ocagne, 1891, 1921]. The subject was broad, encompassing various aspects of geometry, analysis, and applied sciences, and the nomograms found uses in many branches of engineering, including those with which Gronwall was familiar: ballistics, railway and bridge construction (especially with regards to calculations of cut and fill), determination of self-inductance of circuits, and traction of locomotives. Their construction could be described as falling under Fry's category of the use of mathematics to avoid further use of mathematics.

The American mathematical community would have been familiar with nomography as early as 1893, when D'Ocagne presented a paper at the World's Columbian Exposition that year, and David Roberts [Roberts, 2001] has called attention to a 1906 pedagogical paper of the University of Chicago mathematician E.H. Moore, in which Moore refers to the potential for the use of nomography in a classroom setting as one of many interdisciplinary efforts to reinvigorate the teaching of school mathematics.[16] In [Evesham, 1986] the author notes that a series of articles in *The American Machinist* in 1908 brought D'Ocagne's ideas to the engineering community in the United States. Gronwall's familiarity with the subject could easily have resulted from his European education in civil engineering and his resulting practice; in fact, he constructed at least one alignment nomogram himself, while working at the Aberdeen Proving Grounds [Gronwall, 1919a].[17]

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16 "The nomographic methods are rapidly becoming of central importance [to graphical computations of functional expresions]," states Moore [1906, 324].

17 Gronwall's nomogram allowed the easy computation of range and deflection of a trajectory from its initial and final coordinates. The technical report in which he describes it contains explicit instructions on the physical construction of the nomogram, including advice on engineering tools and the procurement of appropriate celluloid logarithmic scales from slide rules. The document entitled "Memorandum On Range Computation," dated November 28, 1919, from the Aberdeen Proving Ground [Memorandum on Range Computing, November 28, 1919], details the use of 10 "general schemes" [Memorandum on Range Computing, November 28, 1919, 1] for range computation of which alignment nomograms are considered best. "... the construction of such a chart requires an absolute minimum amount of computation. It is estimated that 50 such nomograms will be required to represent the entire water range [a portion of the firing range in which projectiles landed in the Chesapeake Bay]" [Memorandum on Range Computing, November 28, 1919, 5]. Details on their physical construction follow.

Among the theoretical aspects of nomography is the following question: Under what conditions can the relation to be graphed, $F(x, y, z) = 0$, be represented by an alignment nomogram? It is easy to see that this question is equivalent to the existence of three pairs of real-valued functions of a real variable (f_i, g_i), $i = 1, 2, 3$, satisfying the following determinant relation, where x, y, and z range over intervals of values:

$$\begin{vmatrix} f_1(x) & g_1(x) & 1 \\ f_2(y) & g_2(y) & 1 \\ f_3(z) & g_3(z) & 1 \end{vmatrix} = 0.$$

The question was raised by D'Ocagne in 1891, and attempts to solve it yielded partial results, but Gronwall was the first to find a necessary and sufficient condition for a relation to be so representable. This he communicated in [Gronwall, 1912b], a paper written in French presumably as an acknowledgment of the French origins to the subject. In fact, in his introduction he mentioned D'Ocagne, "qui en a développé une théorie également remarkable par son élégance analytique et par son importance pratique, surtout pour l'art de l'ingénieur" [Gronwall, 1912b, 60].[18] We will summarize the contents of this paper, and then reflect on its reception and significance.

In the first section Gronwall expands the determinant condition along the third row of the determinant. Through a succession of partial differentiations and the creation of several auxiliary functions stemming from the relation $F(x, y, z) = 0$ and relations among them he arrives after seven pages of calculation at a pair of partial differential equations involving his auxiliary functions. These equations are quite complicated and will not be reproduced here. He has at this point proved that a necessary condition for nomographizabilty is the existence of a common solution to this pair, a function given the label "C." He then proves this condition sufficient by transforming an intermediate system of partial differential equations into linear form and drawing on an existence theorem for a fundamental set of solutions to this set. Thus he obtains the necessary and sufficient criteria for which the paper is usually cited.

In the second section Gronwall uses the variables he created to state necessary and sufficient conditions that respectively one, two, or all of the scales used in the nomograms are straight lines, a condition of interest in practical use.[19] In the third section he assumes that two of the three scales are not straight lines, that the function pairs are unknown but the defining relation $F(x, y, z) = 0$ is given, and that the common solution C can be found, and shows how one can obtain the function pairs by successive differentiation and elimination of his auxiliary functions. This derivation includes an explicit expression for the solution of the intermediate linear system mentioned above. Gronwall evidently considered this production of the functions of more than just theoretical interest; in his introduction he states "Dans un travail ulterieur, je formerai explicitement l'intégrale commune $[C]$ des équations aux dérivées partielles du paragraphe 1..." [Gronwall, 1912b, 61]. The promised work never appeared, but had the determination of C been possible a much closer link to the actual construction of a nomogram would have resulted from the material in this section. He also specifically remarks that in the case in which one has two straight line scales and one nonline scale, his method produces the function pairs "sans quadrature," a computation of difficulty to be avoided.

18 Gronwall may be making a reference here to the title of an 1846 paper [Lalanne, 1846] by Leon Lalanne, a pioneer in the use of nomograms in railroad construction.

19 "Le tracé graphiques d'un nomogramme se simplifie considérablement lorsqu'une ou plusiers des échelles deviennent rectilignes, circonstance qu'on rencontre dans beaucoup d'équations fourniers par la practique"[Gronwall, 1912b, 70].

In the fourth section Gronwall considered a special condition on one of his auxiliary functions which results in the classification of many nomograms, well known at that time, due to D'Ocagne and J. Clark. These sections show a familiarity with the large literature on nomograms. At one point he draws on the Weierstrass elliptic function in his analysis. In the fifth section he assumes that two of the three scales are straight lines, the third a curve, and shows again how to obtain the function pairs by differentiation and elimination only ("sans aucune quadrature"), a method which he compares with that of an earlier writer, M. Massau, in which four quadratures are required. In the final section he analyzes another class of nomograms due to Clark. One should note that this summary cannot do justice to the complications arising in the 40 or so pages of material which comprose the work. Stylistically the paper is typical of Gronwall's exposition, which features carefully defined functions and variables, a patient listing of equations, and references to each relevant equation in a derivation to help the reader through the bewildering arguments.

Taken as a whole, it is evident that Gronwall's work here comprises more than the necessary and sufficient condition for nomographizabilty with which he is usually credited. That result is contained in the first section. The paper appears to be an attempt to put the entire theory and construction of alignment nomograms into a mathematical framework and to create with his auxiliary functions a scheme in which they can be studied and classified. The nonappearance of the promised follow-up paper obviously limited the effectiveness of this effort.

In the mathematical community acknowledgment of Gronwall's work came in the form of a 1913 paper by O.D. Kellogg [1913], then at the University of Missouri, Columbia, in which a quite different necessary and sufficient condition was derived; Kellogg noted Gronwall's pioneering effort.[20] In the second edition (1921) of his *Traité de Nomographie*, D'Ocagne himself noted "La question, d'un intérèt purement théoretique, qui à reconnaître si une équation $F_{123} = 0$ est réductible à cette forme [an alignment nomogram], constitue un difficile problème d'Analyse résolu de la façon la plus remarkable, en 1912, par M. Gronwall..." [Ocagne, 1921, 156] and in his massive two-volume treatise of the same year *Nomographie, ou Traité des Abaques* Rodolphe Soreau [1921] credited Gronwall with the resolution of the problem, albeit with a qualification similar to D'Ocagne's. Soreau included in an appendix to his work an attempt to simplify Gronwall's larger classification efforts.

In later textbooks on nomography similar comments can be found in the sections devoted to the mathematics behind the subject. By the late 1950s the work of Gronwall and Kellogg came to be seen as much too complicated to be of use to practical nomography. Eventually nomography itself, of course, became outdated with the coming of digital electronic computers in the 1940s. But Gronwall's work can be seen as a mathematician's view of the analysis of an important analog computational tool, an attempt to comprehend the process of rendering computable science's important mathematical relations. We conclude with the comment: "Excursions into its [nomography's] theoretical aspects have had the motive that a better understanding would lead to a more satisfactory application" [Evesham, 1986, 331].

5. Princeton years

In a letter to Oswald Veblen dated March 16, 1913, George D. Birkhoff, then at Harvard University, described his plans for the summer, which included a trip to Chicago. "In the first place we shall be in

[20] Kellogg's paper involves criteria for the determination of the ranks of matrices in which auxiliary functions and their partial derivatives appear.

Chicago for a week or two, at which time I hope to see the Chicago men [members of the Mathematics Department] and also to meet Gronwall. If Princeton[21] is still out for an analyst, by the way, it seems to me that here is the man. He will cut a very substantial figure in American mathematics. You know of course all about him. Everything is in his favor" [G.D. Birkhoff to Oswald Veblen, March 16, 1913]. The vacancy at Princeton was in fact occasioned by Birkhoff's departure from Princeton for Harvard in 1912.[22] By May of 1913 Gronwall had been appointed instructor of mathematics at Princeton, the institutional base for Veblen. Surely his publications in pure mathematics played a role in his selection, but his engineering background was of importance as well, as evidenced by the Faculty Minutes from September 24, 1914, regarding his promotion that year to assistant professor: "... his work to be primarily in the department of Civil Engineering. Professor Gronwall comes to us with a record for fine scholarship, both on the Continent and in our own country. He is, in fact, one of the ablest of the Pure Mathematicians in America, and will be of a great assistance in the problems of Mathematics with the Engineering students, a point in our instruction where we have been unfortunately lacking" [Faculty Minutes, 1914, 27].

During the years 1913–1915 Gronwall taught a variety of undergraduate courses at Princeton, including a coordinate geometry course and sophomore and junior level analytical mechanics courses required for all civil engineers, as well as the standard courses in algebra and trigonometry. He offered graduate courses in integral equations (two semesters)[23] and a number theory course. His graduate work also included the advising of the doctoral dissertation of J.W. Alexander II on conformal mapping.[24] His output of pure mathematics during the years 1912–1916, described by Hille as a "volcanic eruption" [Hille, 1932, 776], totaled 34 papers, including those most highly regarded by Hille and others. He also was made an editor for the Princeton-based *Annals of Mathematics*.

But these ideal circumstances did not last. The precise story is at the present time still not clear, but at the very least it appears that Gronwall had been called to task for "irregular attendance upon undergraduate classes" [G.D. Birkhoff to Oswald Veblen, April 25, 1923], to quote a 1923 letter from Birkhoff to Veblen. In 1915 Birkhoff communicated his sadness regarding this dismissal to Veblen, "I was very sorry to learn of the very difficult situation with regard to Gronwall. What can anyone do for him, despite his great abilities, after you at Princeton have given him so many opportunities? Moreover he is precisely the man you want as far as abilities are concerned and it is going to be extremely difficult to replace him.... My feelings toward Gronwall are of the very friendliest..." [G.D. Birkhoff to Oswald Veblen, November 10, 1915]. In a letter from later that year he details his efforts to find academic positions for Gronwall [G.D. Birkhoff to Oswald Veblen, November 25, 1915].[25]

It also appears that charges of alcohol abuse were leveled at Gronwall at this time.[26] The charges dating from this period as well as later times were discussed by Birkhoff in his account to Veblen of the debate

21 For the role played by Princeton in American mathematics, see [Aspray, 1988–1989].

22 For portraits of George D. Birkhoff, Oswald Veblen, and E.H. Moore, each of whom played roles in Gronwall's career, see [Zitarelli, 2001].

23 Gronwall's contribution to the mathematical treatment of the Debye–Hückel theory, to be detailed in a later section, involved the solution of an explicit integral equation, although the course mentioned here was likely more theoretical in nature.

24 For a detailed analysis of the contents of this thesis and its ramifications see [Gluchoff and Hartmann, 2000].

25 One of the institutions mentioned by Birkhoff was Columbia University, with which Gronwall was later associated: "It seems to me that Columbia has infinitely greater need for him than we [Harvard] have. I wonder, however, if they understand their own particular needs!" [G.D. Birkhoff to Oswald Veblen, November 25, 1915].

26 Hille appears to make a reference to this difficulty: "Though he must have been occasionally somewhat of a trial to the puritanical brethren, I have never heard of his having any enemies" [Hille, 1932, 780].

on Gronwall's 1923 nomination to the National Academy of Sciences. Birkhoff acted as Gronwall's advocate in the proceedings and wrote Veblen in detail about his efforts. One such letter to included the account "I felt that some inkling of the situation [alleged alcohol abuse] had to come up, but I made it VERY PLAIN that the rumors were not substantiated, as you will see," and "... finally I said that you [Veblen], Eisenhart, Bliss, Blichtfeld, Kasner, Trowbridge had all lived intimately with Gronwall and had said definitely that the rumors were without any substantial foundation so far as you know" [G.D. Birkhoff to Oswald Veblen, April 25, 1923]. Regardless of the truth of these allegations, alcoholism has ever since been tied to his dismissal from Princeton.[27] Gronwall was on his own again.

Details of his activities during the period immediately following his dismissal from Princeton have not become available. The January 1916 *Bulletin of the American Mathematical Society* still gives a Princeton address for Gronwall, and he spent some time doing a series of reviews of recent mathematical books for the Bulletin of the American Mathematical Society. One of these reviews [Gronwall, 1916] expressed his recent experiences in teaching and engineering practice, for it ended with the remark: "The book under review brings forth one sad reflection: when will our writers of calculus texts for engineering students see fit to give something really modern and practical on graphical integration and solution of differential equations?"

On February 26 of that year he addressed the Society with a paper demonstrating his work on the mathematical theory of elasticity, tempered by his engineering experiences.

6. The stress distributions in a keyed cylindrical shaft

In 1843, Jean Claude de Saint-Venant, the French civil engineer and elastician mentioned in the "Plea to Producing Mathematicians" in the second section, began a series of papers considering the stress distributions in a homogeneous right prism subject to a twisting at both ends. The so-called Saint-Venant torsion problem is to determine the expressions for the components of displacement and stress due to shear at each point of a cross section of the prism.[28] He wrote several lengthy papers on this subject and began a literature which has become quite extensive; a summary of the results as of 1942 may be found in [Higgins, 1942]. From the outset this was more than just an exercise in applied mathematics: Saint-Venant himself was a civil engineer for 27 years prior to turning to these studies. In the late 1850s his theory was applied to a prism with a cross-section which was an approximation to a train rail. Saint-Venant's own papers contained graphical material representing stress distributions on cross-sections, numerical calculations, and comparisons of the calculations with experiments, material which was clearly of use to engineers. Higgins points out in his 1942 survey that "in the past decade more has been written on it [the Saint-Venant problem] than in any preceding like period" [Higgins, 1942, 248] and that "known torsion solutions often are of considerable aid in the study of various problems of technical importance: again,

[27] Thus Albert Tucker, in the oral history "The Princeton Mathematics Community in the 1930s," states "Earlier on there had been another [than Hille] Swedish analyst at Princeton by the name of T.H. Gronwall. ... But Gronwall was an alcoholic and finally had to be eased out of his position at Princeton. I think a job was found for him for a few years working in industry, but he died sometime in the 1920s [sic]. ... Except for the scuttlebutt, my knowledge of Gronwall is from that obituary notice written by Hille" [Tucker, 1984].

[28] A treatment of this theory may be found in [Love, 1944]. Gronwall cited the second edition (1906) of this book in the paper under consideration in this section.

as certain structural problems encountered in modern high speed airplane, engine and tool design have currently centered considerable attention on the complex stress analysis associated with the torsion of solid and tubular prisms of irregular cross-section, knowledge of all methods available for solving these problems is obviously desirable" [Higgins, 1942, 248].[29]

The connection between this problem and industrial concerns of the early 20th century were evidenced in the publication of "The Effect of Keyways on the Strength of Shafts" by Herbert F. Moore in 1909, a bulletin of the University of Illinois Engineering Experiment Station mentioned in our Section 2 [Moore, 1909]. A keyed shaft is a cylindrical shaft to which it is desired to attach a gear, pulley, or housing in order that the power of the rotating shaft may be transmitted to another member of a mechanism; the method of securing the housing is by means of a slot cut into both the shaft and the element to be attached and the insertion of a "key" into the slots of both simultaneously. The slot is called a keyway and the shaft thus slotted is called a keyed cylindrical shaft. The key mechanism may also be viewed as a mechanical fuse; if the shaft is stressed to its breaking point the key mechanism will break first. The shaft is obviously weakened by the keyway, and the stresses to which the keyed shaft are subjected by torsion clearly form a study of interest. Moore writes "The strength and the proper proportioning of keys have been the subject of considerable study and of some experimentation, but the effect of the keyway on the torsional strength of the shaft has apparently been studied but little... The mathematical analysis of the strength of a shaft with a keyway cut in it is a problem of great complexity... Mathematical researches by Saint-Venant and others have developed the theory of square, rectangular, triangular, and elliptical shafts, but, so far as the writer knows, there has been no successful attempt to develop the mathematical theory of the stresses in a shaft with a keyway cut in it" [Moore, 1909, 3].

The bulletin describes a series of experiments conducted by the station in 1908 and 1909, which subjected keyed shafts of various key dimensions to torsional stresses by means of a machine designed for the purpose. The keyways considered were rectangular in cross section. The strength of a shaft with or without the key was defined in terms of its "elastic limit," a quantity which could be precisely measured as the machine twisting of the shaft progressed. A quantity called "efficiency" of a keyed shaft was introduced, defined as the ratio of the strength of the keyed shaft to the strength of a similar shaft without the keyway; the efficiency was related by experiment to the width and depth of the rectangular keyway. There resulted an empirical formula, a linear relation among efficiency, and length and width of the keyway. An intersection nomogram of this relation was also provided.[30]

Whether Gronwall was aware of this document is not known, but his own purely mathematical work on this problem as contained in his 1916 address and published in 1919 as "On the Influence of Keyways on the Stress Distribution In Cylindrical Shafts" [Gronwall, 1919c] addresses much the same problem using Saint-Venant's methods. It is not clear when this investigation was made; Hille says only that it "reflect[s] the interest he held for such questions since his engineering days" [Hille, 1932, 779]. It may have had its origins from that period, or have been the result of post-Princeton engineering employment. In any case the paper shows an interesting combination of an applied mathematician's ability to solve a

[29] On the other hand, regarding the contributors to the mathematical theory of elasticity, Love notes in his introduction, "To get insight into what goes on in impact, to bring the theory of the behaviour of thin bars and plates into accord with the general equations—these and such-like aims have been more attractive to most of the men to whom we owe the theory than endeavors to devise means for effecting economies in engineering construction or to ascertain the conditions in which structures become safe" [Love, 1944, 31].

[30] A similar report from 1925 is [Gough, 1925].

torsion problem, an engineer's desire to present the answer in a computable form, and a steel industry man's knowledge of the nature of the shafts in question.

To begin with the last point, the keyway considered by Gronwall has for a cross-section a small circular arc intersecting the original cross-sectional circle orthogonally; the resulting cross-section is sometimes called the "orthogonal lune." In older texts on machine design this keyway is called simply a round key, and it has the advantage that it is easier to machine than the rectangular cut. Gronwall's treatment incorporates the restriction that $0 < b/a < 1/4$, where a is the radius of the original shaft and b is the radius of the circular arc forming the keyway. He notes that "this range of the ratio b/a [is] sufficient for all cases occurring in common practice" [Gronwall, 1919c, 234]. This comment refers to the fact that, with adjustments made for the circular rather than a rectangular key, "For transmitting power, it is common American practice to use a square key whose width and depth are each equal to about one-fourth the diameter of the shaft" [Moore, 1909, 6].[31]

As for the solution to the torsion problem itself, the Saint-Venant theory required the solution of a classical boundary value problem: given the cross section of the shaft, find a function, usually denoted ψ in the literature, which is harmonic in the cross section (viewed as a region in the Cartesian plane) and assumes the boundary values $(1/2)(x^2 + y^2)$ at a point (x, y) on the boundary of the region. The stress components at a point of the shaft can then be expressed in terms of ψ and its harmonic conjugate; other constants and moments can similarly be expressed using these functions. In Section 1 Gronwall sets the orthogonal lune region in the plane with the origin at the center of the circular shaft and sitting symmetrically with respect to the x-axis; then in Section 2 he finds ψ by mapping the cross section conformally onto the first quadrant of the complex plane by a linear fractional transformation[32] and producing the Green's function for the first quadrant. Then ψ is given by the usual integral of its boundary values against the Green's function as in classical potential theory. He then calculates the conjugate function for ψ, and using both finds the stress components X and Y according to the expressions given in the Saint-Venant theory.

One goal of the paper is to find, for given a and b, the points at which the stress is the maximum, the so-called fail points, an object of much attention in applications of the theory. The stress value at these points is also needed. These he finds in Section 3 of the paper; the calculation reduces quickly to a single-variable optimization problem which yields local maxima at the point on the boundary of the orthogonal lune closest to the origin and at the point of the boundary of the shaft intersecting the negative x-axis. The former point is shown by a calculation to be the global maximum, the stress there expressed in terms of trigonometric functions of $\alpha = \arctan(b/a)$.

This part of the work is in a sense straightforward, though difficult, but what strikes the modern reader is the effort to prove that the single-variable function mentioned in the preceding paragraph has no other critical points than those found: two full pages of transformations, bounding of mathematical expressions,

[31] This restriction reflects the transformation of steel working from milling to scientific treatment: an older text refers to the ratio as "an old rule, used by millwrights" [Hyland and Kommers, 1937, 381], Moore cites *Kent's Pocket-Book*, a machinists' handbook listing diameters and depths of keyways [Moore, 1909, 6], and other machine design books refer to "the U.S. Navy Standard," which gives a precise linear relation for width and depth of the keyway in terms of the diameter of the shaft, e.g., width $= (3/16)D + 1/8$ inches [Bradford and Eaton, 1940, 121].

[32] It is interesting to see this rather straightforward use of conformal mapping by Gronwall at a time when his pure mathematical researches had lead him to some work on the frontiers of the theory of conformal mapping, including the Area Theorem and bounds on the growth of schlicht mappings as a class.

and proofs of nonnegativity of algebraic and trigonometric polynomials are devoted to this task. The work culminates in the demonstration of the nonnegativity of two different polynomials in α and $\sin(\alpha)$ on the interval $[0, \pi/2]$, each of which requires a subdivision of this interval into four subintervals on which the polynomial is tested using different bounding strategies. Gronwall's skill in hard analysis is clearly demonstrated here, and one senses a certain reveling in the details of the demonstration.

The fourth section of the paper is devoted to calculating the moment of external forces around the axis of the keyed shaft, a calculation which involves integration of $\psi - (1/2)(x^2 + y^2)$ over the cross section and evaluation of other constants according to the general theory. This expression is ultimately used to construct the ratio of the maximum stress in the keyed shaft to the maximum stress in the unkeyed shaft of the same dimension, a quantity which may be compared to the "efficiency" in Moore's study. This ratio is approximated by Gronwall in the last section: "We shall now derive approximate formulas, adapted to numerical calculation, from the purely theoretical results of the preceding paragraphs..." [Gronwall, 1919c, 241]. There follow three more pages of boundings, integrations by parts, Taylor series approximations, integral estimations, and clever algebra which lead to a quintic polynomial in $\tan(\alpha)$. This polynomial is then bounded by using the restriction $0 < \tan(\alpha) < 1/4$, resulting in a ratio of two quadratics in $\tan(\alpha)$, which is the desired approximation, whose "values... may be tabulated, or we may replace [it] by a linear expression" [Gronwall, 1919c, 244], which he then gives. One immediate result is noted: as $\alpha \to 0$, the ratio approaches the value 2, which means that "a flaw or crack in the surface of a circular shaft has the effect of doubling the maximum stress" [Gronwall, 1919c, 234].

From our point of view it is perhaps this last section which is of greatest interest, for it shows Gronwall's concern with producing readily computable answers, a concern voiced by some of the commentators quoted in our Section 2. Gronwall himself cited in his first section the results of L.N.G. Filon in a 1900 paper [Filon, 1900] which were similar to the scenario he considered, but "His [Filon's] results are expressed in infinite series of trigonometric and hyperbolic functions, and their numerical computation is necessarily somewhat laborious" [Gronwall, 1919c, 234].[33] In Gronwall's paper the approximation of an infinite series, a common method of expressing a solution in an applied mathematics paper, is replaced by analytical work producing the rational expression, a form clearly suitable for "tabulation."[34] Perhaps his familiarity with pocket-book engineering tables brought this consideration to mind.

The interest in Gronwall's work on the part of the engineering community in America at this time is hard to gauge, but a similar study met with a good reception. In 1921 a 70-page paper entitled "Die Lehre der Drehungsfestigkeit" by Constantin Weber[35] was published in Berlin as part of a series for the German Society of Engineers [Weber, 1921]. The paper included some treatment of the cross section dealt with by Gronwall among many others and contained "formulas and numerical data, approximate and exact, pertinent to cross sections in common structural use" [Higgins, 1942, 255], to quote Higgins's

[33] Section 8 of Filon's paper describes his computation methods, which involve the evaluation from tables of terms of a series of hyberbolic tangents until the function argument was so great that it could "sensibly be taken equal to unity" [Filon, 1900, 326]; the remainder was then estimated using values of various p-series and related series from Chrystal's *Algebra*. Other series required various techniques depending on the rapidity of their convergence. "Even with the help of all these devices the labour of calculating the moment and stress for the sixteen sections was considerable" [Filon, 1900, 328].

[34] This aspect of Gronwall's work was also noted by Hille: "His long and frequent contacts with the applications had given him a strong feeling for what constituted a useful solution in such fields; he was himself an experienced and skilled computer [person whose job was the numerical computation of mathematical expressions]" [Hille, 1932, 780].

[35] Weber had the degree of Diploma-Engineer, the first degree in engineering obtained by students in Germany, roughly equivalent to a master's degree in America. Gronwall had this degree from Charlottenburg.

summary of it. Weber reached the same conclusion as Gronwall regarding the doubling of maximum stress on a shaft by the introduction of a crack. The analysis was based on Saint-Venant's theory, but the techniques used were different. In 1922 Weber published an abstract of this work which was translated into English and published in the American journal *Mechanical Engineering* in 1922. According to a follow-up article in the same journal "...so many inquiries as to details [of the abstract] have been received that it has been decided to publish a more complete abstract" [Weber, 1921, English version, 45]. This later article notes that "In practice bars are encountered of cross-sections vastly different from those treated by Saint-Venant" and "...there are still a number of sections of considerable interest from a practical point of view which remain unresolved ..." [Weber, 1921, English version, 45]. It is Weber's work in connection with keyed shafts which is usually cited in the literature on theory of elasticity.

Gronwall's contribution won him a citation in Love's treatise on the mathematical theory of elasticity, as well as a mention in Higgins' survey ("This cross section is of considerable interest as it is that of a circular shaft with a frequently used, standard keyway" [Higgins, 1942, 255]), and was referred to by later applied mathematicians in their generalizations of it. It might with some license be referred to as an unremunerated consultation for the mechanical engineering community in America at this time.[36]

7. A difficult period

In the years following his dismissal from Princeton Gronwall found himself in a period of difficulties which in some ways was similar to that immediately preceding the awarding of his Ph.D. in Sweden. This time found him trying to work on pure mathematics while finding employment at various engineering jobs, and struggling to maintain emotional balance. During these years he was aided by Oswald Veblen, whose role as institution and community builder within the American mathematical community has been well documented [Feffer, 1998; Zitarelli, 2001]; to these we may add the roles of friend and benefactor. Veblen's concern for Gronwall shows his attempts to use some of the scientific institutions of the day on behalf of his troubled friend. The American mathematical community had evolved to the point where it was possible for a leader such as Veblen to attempt to look out for one of its members by appealing to its institutions as well as colleagues in established university departments for help.

It appears that sometime in 1916 Gronwall relocated to New York City, for he became part of the Mathematical Association of America's Library Committee, whose charge was to report on suggested contents of mathematical libraries for colleges. In this capacity he delivered a report at a December meeting of the MAA at Columbia University, where he was listed as "Dr. T.H. Gronwall of New York City."[37] Many of his pure mathematics papers which had been produced at Princeton began to appear in print that year.

The year 1917 was one of much itinerancy; Veblen's datebooks [Veblen, 1917, Datebook] for this period include seven different entries mentioning Gronwall from April to November, spreading over five different addresses. But this was also the year in which Veblen began to take action: he collaborated

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[36] Gronwall presented a second brief note on elasticity at the same time as the paper discussed here [Gronwall, 1918a] in which he takes issue with an earlier writer on the subject who had argued a conclusion based on general "physical arguments." Gronwall wrote "this conclusion in respect to the tensile or compressive stresses is not borne out by the mathematical theory of elasticity" [Gronwall, 1918a, 295].

[37] Gronwall did not become a member of the MAA until 1920.

with his thesis advisor E.H. Moore of the University of Chicago to obtain funding for Gronwall. This action first resulted in the awarding of a grant of $300 from the Bache Fund, one of several general funds administered by the National Academy of Sciences for research purposes. The application for an appropriation was made by Moore on Gronwall's behalf: its stated purpose, "To complete and to extend mathematical researches on conformal representation" and "To free in a measure Dr. Gronwall from engineering duties so that he may devote himself to mathematical researches" [Moore, 1917]. The money was to be administered by Moore. The application includes a strong recommendation from a Dr. McCurdy, who remains unidentified. The request was granted as Appropriation #207.

In correspondence with Moore it is clear that Veblen was trying other possibilities: in a letter to Moore he writes:

> "It was pointed out to me by Professor Thompson that institutions like the Rockefeller & Carnegie are apt to be more responsive to other institutions than to individuals. Would you think it sensible for the Annals [*Annals of Mathematics*, the Princeton-based mathematics journal] to make a plea along the following lines? "The Annals has learned of certain mathematical investigations of high importance which are in danger of not being completed because the author is forced by material necessities to devote his time to work of a different character. [The words "Engineering & other fields" immediately preceding "of a different character" are crossed out.] The studies in question refer (a) to the theory of conformal representation and analytic functions and (b) to the theory of the Gamma Function. The author in question is T.H.G. who is well known for his work in both pure and applied mathematics and has a very high reputation as an Analyst, particularly in Europe. The importance of these investigations has been recognized by a committee of the National Academy of Sciences which has voted a grant of $300 from the Bache Fund in support of investigation (a). The Annals requests a grant of $1000 to be paid to Dr. Gronwall in installments as his work progresses." If you think that this attempt would be worth while, (4) [the last of four questions put to Moore] would you be willing to support it with a letter?" [Oswald Veblen to E.H. Moore, undated]

Whether Moore endorsed this plan is not known, nor is it clear that other funds were obtained, although in a letter to Moore in 1918 Gronwall writes "I am writing to Veblen regarding the arrangements to be made concerning that part of the money which is being contributed by others [than the Bache Fund]" [T.H. Gronwall to E.H. Moore, June 28, 1918].

Matters took a turn for the worse in the latter part of 1917: In a letter to Moore dating July 14, Veblen reported to Moore that Gronwall "reappeared a couple of weeks ago—was first seen in the library at Columbia." Veblen went immediately to visit Gronwall, who said that he had been in the country recuperating from eyestrain induced by his latest job. The upshot was that "Following the best advice I could get," Veblen took his friend to Bellevue Hospital where a period of several weeks recuperation for the ailing mathematician was discussed. "He [Gronwall] said that he wants to get back into regular standing and admits that obtaining a clean bill of health from medical experts will be an essential part of the process" [Oswald Veblen to E.H. Moore, 1917].

At the beginning of 1918 Veblen reported to Aberdeen Proving Grounds[38] as part of his efforts to upgrade U.S. ballistics, and his work there took up most of his time. Gronwall apparently recovered his energies and began writing. The work on conformal mapping was to take the form of a multivolume book on the subject. This competed for his attention with the gamma function treatise, which appeared in 1918

[38] According to [Schwartz, 1920, 4] Veblen's involvement with the Proving Grounds began in the winter of 1917.

under the name "The Gamma Function in the Integral Calculus" [Gronwall, 1918b][39] in the September and December 1918 issues of the *Annals*, and was reprinted in book form. The former work had a more complicated history. On June 28, 1918 Gronwall wrote to Moore enclosing the first two chapters of the intended book, delayed due to his work on the gamma function treatise, and claimed to have done most of the "preliminary work" on the first volume. He had also produced some "new things" [T.H. Gronwall to E.H. Moore, June 28, 1918] in conformal mapping. Ultimately it was these "new things" which appeared in print in the form of four notes in the *Proceedings of the National Academy of Sciences*; the fate of the book manuscript is not known.[40]

An excerpt from this letter speaks much about Gronwall's state at this time:

> According to the latest information I have received from Veblen, I understand that you will find it practicable to let me have an installment of the money granted by the National Academy of Sciences for the purpose [of continuing the book]. As to the size of this installment, I beg to submit that the two chapters I sent you today will occupy about 40 pages in print, or 1/10 of the first volume. Since I have at present no other source of income, it is of course necessary for the progress of the book that installments of the money become available immediately upon delivery of the corresponding installments of the manuscript, the other alternative being to abandon the book temporarily and take up engineering work again [T.H. Gronwall to E.H. Moore, June 28, 1918].

By November 29, 1918 Gronwall submitted the first note, on a new development in conformal mapping, to Moore, who acknowledged receipt: "I hope ... that you will exercise every effort to make the work on the book progress as rapidly and effectively as possible, and that you will send to me for the Proceedings short snappy notes like this one covering the important new features as they may develop" [E.H. Moore to T.H. Gronwall, November 29, 1918]. The saga of this work continued until 1920, when Gronwall submitted his final notes. By this time he had been working for U.S. Army Ordnance for nearly two years, having been called to the Aberdeen Proving Grounds in the fall of 1918.

8. United States Army Ordnance at Aberdeen Proving Grounds and Washington, DC

The role of the Aberdeen Proving Grounds in the development of the American mathematical community has been recounted many times: the raising of public awareness of the roles that mathematicians had played in the war, the resulting increase in status of the profession, and the devotion to the profession engendered by living for a period with fellow mathematicians have all been discussed.[41] For our purposes we wish to note that the Aberdeen Proving Grounds fits the mold of the new scientifically based

[39] Gronwall stated in the Introduction to his gamma function treatise that [Gronwall, 1918b, 35] "The object of this paper is to give an exposition, as elementary as possible, of some of those aspects of the theory of the Gamma function which are not dealt with in Jensen's 'An elementary exposition of the theory of the Gamma function,"' which he had translated and annotated for publication by the *Annals* in 1916. It is possible that these activities filled a relative void in his creative output at this time.

[40] It is likely that some of this material found its way into a memoir which, according to a recollection of Hille [1932, 778], had been submitted to a competition sponsored by *Acta Mathematica*.

[41] For an overview of the role of mathematicians at Aberdeen Proving Grounds see [Grier, 2001]. For recollections of Norbert Wiener of this time see [Wiener, 1953, 254–263]; for a general history of exterior ballistics see [McShane et al., 1953] which contains a "Historical Appendix."

industrial laboratory similar to those arising at A.T.&T. and Du Pont; for example, it included a branch devoted to the mathematical theory of ballistics and methods of computation. It can be regarded as a scientific testing ground for the design and firing of guns and shells; in the words of P. Schwartz[42] in his 1920 history of the ballistics work done there, "As far as possible all of the customary refinements of the physical laboratory are being introduced so as to let no avoidable error creep into the work [the conduction of range firings]" [Schwartz, 1920, 10], and, prior to the work at Aberdeen, "Range observing, computing, range table computation and all computing in general were not conducted on a truly scientific basis" [Schwartz, 1920, 3]. As Norbert Wiener described it, "It was a period in which all the armies of the world were making the transition between the rough old formal ballistics to the point-by-point solution of differential equations..." [Wiener, 1953, 256]. Since the contributions of Gronwall are inseparable from those of his fellow mathematicians and lie several layers within the theory they developed, some background is needed.

The division in which Gronwall found himself was the Range Firing Section, whose goals, as stated in the postarmistice document "The Range Firing Section of the Proof Department, Aberdeen Proving Ground: Its Objects, Its Development, and Its Accomplishments," were the conduction of range firing, the improvement of projectile design, the preparation of range tables and the advancement of ballistic theory. It was the last goal to which Gronwall made several contributions. The role of mathematics in ballistics theory had several justifications in the eyes of the participants; the author of the preceding document stated that "Another important function is that of making improvements in ballistic theory in order that no incident of firing may be unexplained. Nowhere in America other than at the Proving Ground is there such a wealth of original data which may be made use of in the verification of original theories" [Range Firing Section, 1918, 1], i.e., the firing data could be used to test the theories proposed by the ballisticians and, conversely, firings both routine and unusual could be accounted for by theory.

An important goal for the mathematicians called to the Proving Grounds in the Fall of 1918 was the construction of range firing tables for the many different types of arriving field guns, including new antiaircraft weapons. Among the variables to be considered in developing these tables were accurate measurement of cross wind, rear and head winds, density of air, angle of elevation, rotation of the earth, and type of shell and gun.[43] Several of these variables had only recently needed to be considered because the newer guns had higher angles of elevation, thus creating trajectories which penetrated into higher, less dense atmosphere. The existing ballistic theory proved inadequate for these new conditions and thus required modification. "In order to take accurate account of these data and in order to apply to the many problems uncovered in experimental range firing the best mathematical talent, Major Veblen procured for the Proving Grounds the services of a number of prominent mathematicians" [Range Firing Section, 1918, 3]. Each new weapon needed a firing range table, and, in the words of Gilbert Ames Bliss, one of the prominent mathematicians at Aberdeen, "The business of the mathematical ballistician is to compute the data required for range tables and to assist in the arrangement of the data in a form as convenient as possible for use in the field" [Bliss, 1944, 13]. Interest in computing the entire trajectory of a shell, as opposed to merely knowing its fall point or maximum ordinate, was linked to the antiaircraft problem; "The first important problem [in the ballistics of the new war] was the computation of antiaircraft range

42 P. Schwartz is in all likelihood the "Mr. Philip Schwartz" listed on p. 4 of [Moulton, 1919] under "Civilians of the Ballistics Branch" with the information "(now 2nd Lt.), B.S. (Columbia University), detailed from the Aberdeen Proving Ground, Computer, May 24, 1918, to July 1, 1918."

43 This list of variables recalls the comments in our second section about the increasing complexity of industrial problems.

tables so as to give the characteristics of the trajectory at any point, for use with antiaircraft guns which shot at high velocities and at all angles up to 90 degrees" [Schwartz, 1920, 6].

As previously noted, the existing theory was inadequate for the new weapons. It had however reached the stage that its variables had been tabulated (the so-called Siacci tables, after the Italian theorist), and with these tables, four equations which linked them, and basic trigonometric functions, most practical ballistics problems were assumed to be solvable [Grier, 2001, 924–926]. The tables were the result of certain approximations in the solutions of the differential equations of the motion of the projectile which were inappropriate for the higher elevation weapons. The first of the major mathematical developments was made by Forrest Ray Moulton, a professor of astronomy at University of Chicago, who along with Veblen was attached to Ordnance in the winter of 1917. This development was a new numerical method of solution for the basic system of differential equations governing the flight of a projectile: if the origin of the Cartesian coordinate system is taken as the muzzle of the gun, positive x-axis horizontal and directed at the target, positive y-axis vertical and directed upward, positive z-axis directed to the right of the line of fire, the equations were

$$x'' = -Ex', \qquad y'' = -Ey' - g, \qquad z'' = -Ez', \tag{1}$$

where g is the acceleration of gravity,

$$E = \frac{G(v)H(y)}{C}, \tag{2}$$

and

$$v = \sqrt{x'^2 + y'^2 + z'^2}. \tag{3}$$

The initial conditions for $t = 0$,

$$\begin{aligned} x_0 &= 0, \quad x_0' = v_0 \cos\alpha, \\ y_0 &= 0, \quad y_0' = v_0 \sin\alpha, \\ z_0 &= 0, \quad z_0' = 0, \end{aligned} \tag{4}$$

where v_0 is the initial velocity and α the angle of departure and all derivatives are with respect to time t. The function G was called the Gâvre function, an experimentally determined piecewise continuous function which attempted to account for the drag on the projectile as a function of its velocity, H a specific negative exponential function to account for atmospheric density, and C, the "ballistic coefficient," a constant which essentially was assumed to contain all the information about the projectile. The projectile was assumed to act as a particle. Moulton developed what came to be known as the "method of small arcs," a numerical method for "wrenching" a solution out of the above equations, to cite a contemporary phrase much in use. Details of this method may be found in [Jackson, 1921].

Two issues are relevant to the absorption of mathematics into this setting. First, although it is commonly assumed that Moulton's experience with astronomy and its computations of the orbits of planets made him particularly well suited for this work, by his own admission this qualification was irrelevant: "The introduction of the method of solving numerically differential equations is so simple and obvious that any one familiar with the general field of differential equations would hardly fail to do substantially what I did" [Moulton, 1928, 246] he wrote in a reply to a review of his 1926 summary volume *New Methods In Exterior Ballistics* [Moulton, 1926]. Second, he regarded another contribution as being "very important," namely "I laid down for the first time explicit conditions under which the process is valid in a strict mathematical sense. One having any considerable degree of mathematical sophistication would not feel at liberty to ignore the question of the validity of the process on which he bases all his conclusions" [Moulton, 1928, 247]. Thus the mathematician in Moulton needed proof of the convergence of the process he developed as much as the process itself, which was regarded as an "instant success, both with respect to increase in accuracy of computation, and also with respect to gain in time in comparison with other short arc methods" [Schwartz, 1920, 5].[44]

The next contribution also came from Moulton. If a trajectory for a projectile has been computed assuming "normal conditions," it is of interest to know how that trajectory would change if one of the variables on which the calculation depended were changed slightly; what would happen, for example, if a wind arose, or a different amount of firing powder were used, imparting a different initial velocity. One could of course recompute the trajectory with the new conditions, but an alternative, instituted by Moulton, was to "regard the differences between the coordinates of the projectile on the two trajectories as new unknown functions of t, to write down differential equations which have these functions for solutions, and to treat the new equations, or a system of equations derived from them, by a method of numerical integration similar to that used in the original trajectory computation" [Jackson, 1921, 22]. The new variables were called the differential corrections. The computation of these corrections, though, in their many numerous combinations, was "still a tedious process even by the method Major Moulton derived for use with his trajectory computations" [Schwartz, 1920, 5].

This problem was solved by Gilbert Ames Bliss of the University of Chicago, a former student of Moulton.[45] His contribution was written up in a series of internal memos, called "blueprints" due to the kind of paper on which they were written, beginning in November of 1918. His solution invoked the idea of the adjoint to a linear system of differential equations. Suppose one has a system of, say, four linear differential equations

$$
\begin{aligned}
\xi' &= a_1\xi + b_1\eta + c_1\sigma + d_1\tau + e_1,\\
\eta' &= a_2\xi + b_2\eta + c_2\sigma + d_2\tau + e_2,\\
\sigma' &= a_3\xi + b_3\eta + c_3\sigma + d_3\tau + e_3,\\
\tau' &= a_4\xi + b_4\eta + c_4\sigma + d_4\tau + e_4.
\end{aligned}
\tag{5}
$$

Then one can define the adjoint system as

$$
\begin{aligned}
-\lambda' &= a_1\lambda + a_2\mu + a_3\upsilon + a_4\rho,\\
-\mu' &= b_1\lambda + b_2\mu + b_3\upsilon + b_4\rho,
\end{aligned}
$$

[44] One interesting observation made by Schwartz was that the calculating machines used for the computational work, vividly referred to by Wiener as "crashers," actually made the method of short arcs feasible: "Calculating machines have been introduced, thus causing a great saving in time and labor. One of the aids in making the short arc method by numerical integration, practicable was the Monroe calculating machine [comma in the original]" [Schwartz, 1920, 5].

[45] A full treatment of this approach may be found in Bliss's 1944 book [Bliss, 1944].

$$-\upsilon' = c_1\lambda + c_2\mu + c_3\upsilon + c_4\rho,$$
$$-\rho' = d_1\lambda + d_2\mu + d_3\upsilon + d_4\rho. \tag{6}$$

All derivatives are taken with respect to time. The adjoint system is a topic in the classical theory of differential equations; a summary of its use there may be found in [Goursat, 1917]. The variables in the two systems are related by an easily derived fundamental relation

$$[\lambda\xi + \mu\eta + \upsilon\sigma + \rho\tau]_{t=0}^{t=T} = \int_0^T (\lambda e_1 + \mu e_2 + \eta e_3 + \rho e_4)\,\mathrm{d}t. \tag{7}$$

Bliss's insight consisted of noting that the original Moulton system for the differential corrections could be written as a system like the first just given (using only x and y coordinates for the sake of simplicity) with all the expressions involving actual changes in the variables in question, like drag function, air density, etc., contained in the expressions e_1, e_2, e_3, e_4; the other coefficients involved known quantities or those already computed along the unperturbed trajectory. The adjoint system, whose variables have no actual physical meaning, does not involve the e_k, and thus once the original trajectory has been computed, the adjoint may be solved without inputting any of the changes in the new trajectory, since all its coefficients are obtained from the original system. Now, for example, a change in range can be computed by noting that it can be expressed in terms of the new variables by setting $\lambda = 1$, $\mu = \cot(\text{angle of hit of projectile})$, $\nu = 0$, and $\rho = 0$ at $t = T$ on the left-hand side of the equation. Then integrating the adjoint system backwards from $t = T$ to $t = 0$ one can compute the values of its variables at all values of t down to $t = 0$. Then, using the fundamental relation above with the computed values of the adjoint variables and the changes given by e_1 through e_4 in the right-hand side of the equation, the desired change in range can be found with one integration of a single function only. Thus the work involved in finding the range correction involves one numerical integration of a linear system and a single definite integration of a single function for any changes in the original variables, not a new numerical integration for each change.[46]

This discovery was unanimously praised by all accounts of the history of the range table problem at Aberdeen, due to its substantial savings of computational effort. Again, the mathematical aspect of this contribution is our main concern. For Bliss the method was more than just a clever rearrangement of variables to expedite computation. Like Moulton, Bliss addressed the issue of the convergence of his method in two purely mathematical papers in the *Transactions of the American Mathematical Society* [Bliss 1920a, 1920b]. The theory involved notions of functional analysis, then commonly referred to as functions of lines, which at that time was at the boundary of pure mathematical research. Bliss himself recorded his surprise that a subject so theoretical should have practical application in ballistics [Bliss, 1927], and Norbert Wiener spoke of Bliss's "brilliant use of the new theory of functionals" [Wiener, 1953, 260]. As late as 1944, when Bliss wrote his book on exterior ballistics for use in the Second World War, W.E. Milne wrote in *Mathematical Reviews* that "It is interesting to observe how the subject of adjoint differential equations, once an esoteric theory of 'pure' mathematicians, has now become a commonplace tool of practical engineers" [Milne, 1944].

[46] Gronwall made a small addition to these reports involving another transformation of the variables to ease computation [Gronwall, 1919b].

Gronwall's time with Ordnance began in the Fall of 1918; he is listed in the 1918 "Objects" report under "Surveying and Theoretical Research." He blueprinted several reports on various computational aspects of range firing tables over the next several months, and by February 20, 1919 had a preliminary report, "Qualitative Properties of the Ballistic Trajectory," which he later enlarged for publication [Gronwall, 1920]. In it he proceeded as in earlier work, accepting as given the differential equations for a trajectory for which Moulton developed his numerical method of integration, and deriving from them by means of analysis properties that such a trajectory would have, given various hypotheses on the functions E, G, and H. This was an exercise in pure analysis, and familiar tools such as Chebyshev's integral inequality for monotone functions, Schwartz's inequality, and the generalized mean value theorem appear. He demonstrated his liking for absorbing existing literature on a subject by comparing some of his results and derivations to those of older ballisticians.

Gronwall's stay at Aberdeen was longer than that of most of his other colleagues, ending roughly in mid-July 1919 with an appointment as technical expert with the Office of Ordnance in Washington, DC. Once again Veblen played the role of benefactor by helping Gronwall secure this position.[47] His immediate supervisor was Dunham Jackson, his old competitor for priority in the nonnegative trigonometric sum result of 1912. It was in this situation that he made his major contribution: in a letter to Veblen dated August 14, 1919 [T.H. Gronwall to Oswald Veblen, August 14, 1919], he wrote:

> As soon as I arrived here some problems in differential corrections were put up to me, and the result was a rather lengthy investigation of various aspects of the theory. A couple of days ago, I made what seems a rather important discovery, namely that the Bliss adjoint system has a first integral, which is $x'\lambda + y'\mu + x''\lambda_1 + y''\mu_1 =$ const in the notation of Bliss' first paper. This reduces the adjoint system from the third order to the second, and I expect that the numerical work in computing the differential corrections will reduce, by the new method the details of which I am now developing, by something like 40% for the range corrections, and 60% for the anti-aircraft trajectories.

Some words of explanation are in order. Bliss's adjoint system in the form given in this section has four linear differential equations, but one of these is the trivial relation $\lambda =$ constant; another virtue of the Bliss method is thus that it immediately reduces the amount computational work by presenting a system with only three nontrivial equations. Gronwall discovered a "first integral" for this system, which for our purposes may be described as a relation among the variables in the system which can be used to reduce the number of differential equations, in this case from three to two.[48] Clearly this was an important discovery in the days when computations were done by hand or adding machine. The discovery can be seen to depend on two essentially equivalent observations: (1) that the velocity components of the original trajectory satisfy the differential equations for the corrections, and thus when substituted into the "fundamental relation" between original and adjoint systems give the relation discovered by Gronwall [Jackson, 1921, 29] or (2) that the original system of differential equations for a trajectory do not involve the independent variable "t" directly. It was the latter that Gronwall pointed out in his *Transactions* paper on differential corrections [Gronwall, 1921].

[47] "Now that the victory [in the fight for this new position] is won, I wish to thank you for your efforts in getting this appointment" [T.H. Gronwall to Oswald Veblen, July 11, 1919]

[48] The notion of a first integral of a system of differential equations has a precise mathematical definition which can be found, for example, in [Goursat, 1917, 74–76].

In this paper Gronwall indeed noted that "...the adjoint system is thereby reduced to the second order...and in consequence, the numerical computation of the variations is materially shortened" [Gronwall, 1921, 505], but the bulk of the paper is again concerned with the properties of the differential corrections which can be made to follow from this shortened form of the adjoint system. "Of greater theoretical interest is the fact that, the system of linear differential equations involved being of the second order, the general behavior of their solutions may be determined in a fairly complete manner" [Gronwall, 1921, 505]. Thus we have the typical mix of computational concern with theoretical development so characteristic of his work. Formulas for the variation in range and maximum ordinate due to changes in initial velocity, angle of departure, and following wind are found, and theorems such as "For low trajectories...the range increases less rapidly than the square of the initial velocity" [Gronwall, 1921, 519] are proved. Comparisons to results of classical ballistics are again made. The paper proceeds through 20 pages of hypnotically evolving equations, each thoroughly related to the others. These results were communicated at an October 1919 meeting of the American Mathematical Society.

This result of Gronwall's, though not as deep as Bliss' original ideas, was accepted as of great use in reducing computational time for the range firing tables, and his name was usually mentioned in conjunction with Bliss' in post-war accounts of the activities of these mathematicians. As an example, Gordon F. Hull, in his 1919 article "Some Applications of Physics to War and Peace," that stated "Professor Bliss gave an inclusive method of computing variations in range, altitude and time due to changes in air density, winds, muzzle velocity. Dr. Gronwall greatly simplified and extended the work of Bliss, and made other important contributions" [Hull, 1919, 225].

It is not clear how many range tables were actually constructed using these methods, as the end of the war made these objectives less pressing, but the one sign of their perceived importance is their inclusion in two ordnance textbooks. Dunham Jackson's ordnance text [Jackson, 1921], published in October 1919, contained a last-minute Supplementary Note which related that "At the time when the [preface was] written, Dr. Gronwall was engaged in working out the practical details of a modification of Professor Bliss's method, by which the labor of computing differential variations is materially reduced. This method has been found so effective that a section describing it has been inserted in Chapter 11" [Jackson, 1921, 6]. Gronwall's reduction was also included in *A Course In Exterior Ballistics* by Roger Sherman Hoar [Hoar, 1921, 90], a textbook for the first course of instruction in the new ballistic methods given in the United States, at the Ordnance School of Application in the winter of 1919–1920. Hoar went so far as to declare that "Gronwall discovered a new first integral which revolutionized the computations" [Hoar, 1927, 325].[49]

It is worth noting that the new mathematical methods of range firing computations had their detractors. In P. Schwartz's critical study mentioned above one finds reservations expressed, among which is the

[49] This statement was contained in Hoar's review of Moulton's *New Methods in Exterior Ballistics* (University of Chicago, 1926), a piece which occasioned a nasty reply from Moulton [1928]. The reply took some swipes at Gronwall. Regarding the statement about the newly discovered first integral: "Perhaps he [Hoar] was misled by the great detail with which Dr. Gronwall set forth his results ... He [Gronwall] devotes many pages to writing out the elementary transformations to derive my equations for the differential variations and those of Professor Bliss for the adjoint system. As for the 'discovered new first integral,' it is a direct consequence in this particular problem of general principles which have been well known for at least fifty years" [Moulton, 1928, 249]. This presumably refers either to the fact that the fundamental relation linking a system with its adjoint is the basis for the new first integral discovered by Gronwall or that a system in which a variable is suppressed can be reduced to a system of lower order. This reply as a whole holds additional interest for its contrast of different ideas of what constituted mathematical sophistication at the time.

difficulty of the mathematics itself: “Mathematicians may say that the Siacci method is based on poor and complicated methods and that army officers have been kept away from the study of ballistics on this account, but a casual glance at the published papers on the short arc [Moulton] method and the method of computing differential corrections will make one think that the Siacci mathematics is much simpler. A complete understanding of the mathematics of the new methods including the differential corrections involves an understanding of a great deal of higher mathematics” [Schwartz, 1920, 6]. This sentiment is repeated by J.E. Rowe in a review of Moulton’s book: “Thus the problem, from the standpoint of the practical ballistician at least, is one of engineering mathematics. What is to be gained by making the mathematical method of approximation more difficult than is necessary?... Let us aim to get the physical data or tabulated data as accurately as possible, and the method of approximation as simple as possible. Surely this is the practical point of view, and it must be admitted to be preeminently the case when one takes into account the kind of educational training usually possessed by army officers of any nation. Their training must be broad and practical in the extreme, and they want mathematics presented in as simple and usable [a] form as possible” [Rowe, 1928, 231]. Both authors point out that the short-arc method is only as accurate as the assumptions built into it, such as the drag and air density laws, a point with which the mathematicians would surely agree. The limitations of the use of the tables by artillerymen in the field is also discussed by Schwartz.

As for Gronwall, he stayed on at Ordnance in Washington, computing tables and doing other pure mathematical research. As with J.E. Littlewood’s tenure in England in a similar situation, his own research occupied much of his time. His career in America thus far earned him a starred entry in the 1921 edition of *American Men of Science* [Cattell and Brimhall, 1921, 274].[50] On the other hand, Hille met Gronwall at some time during this period; in his memoriam Hille states without elaboration that “... he [Gronwall] was already a disillusioned man, modest, quiet, and retiring” [Hille, 1932, 780]. But apparently his restless nature asserted itself again, for suddenly in 1922 he departed for New York City, a move of some surprise to his colleagues.

9. The Neumann integral and its evaluation

The year 1922 was significant for the American Telephone and Telegraph Company in New York City. The organization’s technical journal, *The Bell System Technical Journal*, began publication of research articles. John Renshaw Carson, who had been working with A.T.&T. since 1914, published an important mathematical analysis of frequency modulation theory which made a connection between bandwidth and highest modulating frequency. (Recall that Carson was the target of Edwin Armstrong, whose *FM* article was quoted in our second section.) Thornton Fry, whose discussion of the role of mathematics in industry was previously discussed, convinced the Western Electric Company to form a separate mathematics consulting department which was reproduced when he joined Bell Laboratories in 1925; in 1922 Fry had collaborated with physicists and engineers on binaural location of sound and on phenomena in photocells. In the following years such prominent mathematical talents as Harry Nyquist and George Campbell (author of the paper “Selling Mathematics to the Industries”) made research contributions at A.T.&T.[51]

50 “A star is prefixed to the subject of research in the case of about a thousand of the biographical notes. These are the thousand students of the natural and exact sciences in the United States whose work is supposed to be most important.”

51 For details of these contributions see [Bell Telephone Laboratories, 1975, ch. 10].

Somehow T.H. Gronwall came into contact with A.T.&T. in the years 1922–1923, and made the acquaintance of Carson[52] and his collaborator Otto J. Zobel, a pair to whom we owe the first reference to Gronwall as a "consulting mathematician." In a July 1923 publication [Carson and Zobel, 1923, 17 (footnote)] the authors needed to study integral convolutions of the Bessel function of order n with sine and cosine functions of arbitrary frequency. The values of these integrals had been tabulated for n up to 60, but for larger values they needed new information; this was provided by Gronwall in the form of asymptotic expansions, though they were not reproduced in the paper. For this service Carson and Zobel stated in a footnote: "The writers take pleasure in acknowledging their indebtedness to T.H. Gronwall, consulting mathematician, who furnished asymptotic formulas for the computation of these integrals."[53] Significant in this regard is a comment by Hille: "... [Gronwall] used to complain that their [A.T.&T.'s] problems required his knowing Watson's *Theory of Bessels Functions* by heart" [Hille, 1932, 776]. The book in question had appeared in 1922 and was an outstanding work of scholarship and pure mathematics (see [Askey, 1995] for a summary of this book), and Gronwall's familiarity with it shows his voracious mathematical appetite as well as his tendency to use the most current literature to solve problems (recall his use of Love's theory of elasticity for the keyed shaft problem).

Among other problems tackled by Gronwall on behalf of A.T.&T. at this time was that of evaluating the Neumann integral of a certain pair of loops. We will need some background on this object. In 1845 and 1847 F.E. Neumann published studies [Neumann, 1845, 1847] of the laws of electrical induction which gave rise to an integral expression as follows: let C_1 and C_2 be two curves, and then define $M = \int_{C_1}\int_{C_2} (\cos(\theta))/r \, \mathrm{d}s_2 \, \mathrm{d}s_1$. Thus one has an iterated line integral over both C_1 and C_2, θ is the angle between the arc length elements $\mathrm{d}s_1$ and $\mathrm{d}s_2$, and r is the distance between $\mathrm{d}s_1$ and $\mathrm{d}s_2$. This expression is now called the Neumann integral, and it is one way of calculating what came to be called mutual inductance. The physical situation in which this arises is that of two circuits C_1 and C_2 in one of which, say C_1, a current flows. If that current changes, there is a magnetic field set up around C_1, which in turn induces an electric current in C_2. The electromotive force thus induced in C_2 is equal to $M(\mathrm{d}i/\mathrm{d}t)$, where i is the current in C_1 and M is by definition the coefficient of mutual induction, or mutual inductance. The Neumann integral, it can be shown, is one way of evaluating this quantity. If the roles of C_1 and C_2 are reversed, the constant M has the same value; thus the term "mutual." The Neumann integral is dependent only on the geometry of the curves, though it relates various electrical and magnetic quantities.[54] Another concept in this class is self-inductance: each turn of a wound coil, for example, links with the magnetic field produced by its own current and currents in the other turns of the coil inducing a counter

[52] Gronwall may have met Carson at Princeton, where the latter was an instructor in electrical engineering during the academic year 1913–1914. Carson attended Princeton as an undergraduate and received an electrical engineering degree in 1909 and a Masters of Science in 1912.

[53] Gronwall kept in contact with Carson as late as 1929, as indicated by a reference Carson made in his paper of that year [Carson, 1929, 785] to "an unpublished memorandum [by Gronwall] communicated to the writer" on the subject of the paper. Carson made several other significant contributions to communications theory, most prominently single sideband, an efficient means of signal transmission. His 1926 book [Carson, 1926], a rigorous treatment of the Heaviside operational calculus to solve the differential equations of circuits, is classic in the field.

[54] A typical derivation of this quantity in physics textbooks is to relate it to the magnetic flux F set up by the current in C_1,

$$F = \int_{S_2} \vec{B} \cdot \mathrm{d}S = \mu \int_{S_2} \nabla \times (\vec{A}) \cdot \mathrm{d}S = \mu \int_{C_2} \vec{A} \cdot \mathrm{d}s = \mu \int_{C_2}\int_{C_1} i \frac{\mathrm{d}v}{r} = \mu i \int_{C_2}\int_{C_1} \frac{\cos(\theta)}{r} \, \mathrm{d}s_1 \, \mathrm{d}s_2, \tag{8}$$

electromotive force. When a wound coil becomes a component of a circuit it is called an inductor. Inductance in either case is measured in henries. A coil has 1 H of self-inductance if a current change of 1 A s^{-1} induces 1 V of counter electromotive force; it has 1 H of mutual inductance if a current change of 1 A s^{-1} induces an electromotive force of 1 V in the other coil. Self-inductance can also be thought of as the sum of all mutual inductances of all pairs of filaments which compose the coil.

Neumann himself had evaluated the double integral for the case of two congruent parallel coaxial square loops and expressed the answer in closed-form rational functions involving logarithms and square roots; the work is an exercise in techniques of integration. The task of evaluating these integrals for various configurations soon got caught up in developments in electrical technology, specifically with devices used to measure electrical quantities, like resistance and amperage, and later with communications technology. James Clerk Maxwell, in his 1873 work *A Treatise On Electricity And Magnetism*, considered evaluating the Neumann integral, "...a quantity of great importance in the theory of electric currents" [Maxwell, 1954, vol. 2, 46], for the case of two parallel coaxial circles and was led to an expression involving elliptic integrals. He also took the trouble to tabulate values of his expressions "on account of the importance of the quantity M in electromagnetic calculations" [Maxwell, 1954, 339].

As an example of the use of the Neumann integral in electrical metrology, we briefly discuss a scheme devised by J. Viriamu Jones of University College, Cardiff, in the late 1880s and 1890s for determining resistance. Jones had adopted a method of Conrad Lorenz for measuring this quantity by suspending by conducting wires a metallic disc in the "mean plane" of a coaxial coil with one layer of wire. The conducting wires were connected to a resistance to be measured with the same current passed through the coil and the resistance. When the disc is rotated there is set up due to the electrical and mechanical forces generated an equation $Mn\gamma = R\gamma$, where $M =$ mutual inductance of the coil and the circumference of the disc, $R =$ resistance, $n =$ the rate of rotation of the disc, and $\gamma =$ current through the coil and the resistance. Thus if n is known and M can be calculated, the resistance can be found [Jones, 1891]. In an earlier paper of 1888 [Jones, 1889], Jones had done the calculation of M, which he expressed as an infinite series of elliptic functions and truncated after five terms. In a follow-up paper in 1898 [Jones, 1898], Jones pointed out a more general "theorem" inherent in this device, as well as a revised method of calculating M. The theorem relates the force between a cylindrical current sheet and any fixed second curve and culminates in the equation $F = \gamma_2\gamma(M_2 - M_1)$, where F is the force between the current sheet and the second curve in the direction in which the current sheet was generated, γ_2 the current in the second curve, γ the current per unit length in the sheet, and M_1 and M_2 the mutual inductances of the second curve with the initial and terminal curves of the current sheet. Thus if the current is identical in both objects and the mutual inductances can be calculated, the current can be measured. Jones concludes with a note that this investigation was undertaken "...in consequence of the 'Report of the Electrical Standards Committee of the British Association' made at Toronto, in which mention is made of the importance of redetermining the ampere" [Jones, 1898, 205].[55]

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where μ is the constant permeability, i is the current in C_1, B is the magnetic field produced by the change in the current, A is its vector potential, and θ and r are as before, S_1 and S_2 are surfaces bounded by C_1 and C_2, respectively. Note that the Neumann integral is thus a purely geometric quantity. See, for example, [Panofsky and Phillips, 1962, 174].

[55] Maxwell's treatise contains a chapter on various instruments for the measurement of electrical quantities, including an 1849 method of Kirchoff for measuring resistance which requires a mutual inductance calculation. It is also possible to measure inductance itself by a similar scheme once resistance and current are known; in fact, the first paper by Rosa and Grover in the *Bulletin of the Bureau of Standards* in 1904 suggests such a method [Rosa and Grover, 1904]. (This paper was presented at the

As technology developed, the need for more accurate measurements of electrical quantities became apparent, and international conferences devoted to developing standards for these units became widespread; the development of radio further increased this need.[56] In 1901 the Bureau of Standards in the United States was created largely in response to the need to establish electrical standards. There existed a need in U.S. industry for a single, consistent basis for measurements of power, current, impedance, voltage, and other electrical units. The role played by inductance calculations in this work is evident in the publication in 1911 of "Formulas and Tables for the Calculation of Mutual and Self-Induction" [Rosa and Grover, 1911],[57] a 237-page compilation of theory and techniques of evaluation of these quantities, including the use of the Neumann integral, by Rosa and F.W. Grover. Much of this material draws from the numerous individual papers which appeared in the *Bulletin of the Bureau of Standards* from 1904 on; this work was itself a revision of a 1907 collection by the authors.[58]

A perusal of the pages of this document, as well as later Bureau of Standards publications on mutual inductance, shows some of the computational difficulties in evaluating this expression, which was the problem put to Gronwall by A.T.&T. The introduction points out that a great many formulas for calculation of mutual and self-inductance exist, and consequently there is a choice to be made by one applying them, "because of the greater accuracy or convenience of one as compared with the others" [Rosa and Grover, 1911, 5]. The first section alone, for example, treats the case of two coaxial circles considered by Maxwell, but by this time there were six subsections each devoted to a family of techniques for evaluating the Neumann integral in this case alone. Difficulties in dependence on tables of elliptic integrals were pointed out in the case of Maxwell's treatment. Many series expansions of M are presented, some considered "very convergent" within certain ranges of variables. Some series work better when the circles are close together. In some of these series terms are dropped when one variable is small compared with another, resulting in simplification of the expression used. The size of the error committed is mentioned in many cases by relating the magnitude of the negligible quantity to the magnitude of the corresponding term in the series. Statements such as the following appear: "... a converging series which is often more convenient to use than the elliptical integral formula, and when the circles are nearly of the same radii and relatively near each other the value given is generally sufficiently exact" [Rosa and Grover, 1911,

International Electrical Congress in St. Louis in 1904.) Clearly issues involving the accuracy of measurements are also involved in these methods.

56 An interesting and amusing indication of the influence of radio on the increased attention paid to measurement and calculation of electrical quantities can be found in the preface to the 1921 revised edition of *Absolute Measurements in Electricity and Magnetism*, by Andrew Gray [1921]. The first edition had appeared as two volumes, the first in 1888 and the second in 1893. In the preface of the 1921 edition Gray, a professor at the University of Glasgow, expresses exasperation with the neglect of this subject by contemporary physicists more interested in modern theoretical developments: "In the interval since the publication of the First Edition of this book the subjects of physical study have changed enormously, and if it were not for the needs of Wireless Telegraphy, I question whether the theory and practice of absolute measurements would at the present time command serious attention. It has even been said that radioactivity and the phenomena of X rays are the only things worthy of the attention of physicists ... As it is, we have now an army of students and others talking glibly of Einstein and of quantum theory, whose attention to the fundamentals of dynamics and physics has been wofully [sic] slight" [Gray, 1921, preface, v].

57 In a section in a related work [Grover, 1922] there occurs the statement, "At the Bureau of Standards a set of single layer coils wound on bakelite forms of such a shape that each turn has the shape of a 12-sided polygon has been used as standards of inductance in radio measuring circuits. It is, accordingly, a matter of importance to be able to calculate accurately the inductance of coils of this type from their dimensions" [Grover, 1922, 738].

58 The volume [Hak, 1938] is an even more comprehensive collection containing 687 individual references to papers on the Neumann integral dating from 1845 through 1937.

13].[59] On the other hand, some series are shown to converge, leaving en error of one part in a million.[60] The section culminates with a table suggesting which formula to use for the best results as a function of the values of r_2/r_1, where r_1 and r_2 are respectively the longest and shortest distances between the circles. Tables of elliptic functions are given in the volume, and many examples are worked.[61]

The problem for which Gronwall was consulted by A.T.&T. was the evaluation of the Neumann integral for two squares in a configuration to be described. The use of this value was given by Gronwall as follows: "In the design of apparatus for the absolute measurement of radio field intensity, it is necessary to compute the mutual inductance of two square coils in the following position: In a vertical plane, place two squares of sides l and L, where $l < L$, so that their centers coincide and a pair of sides in each square are vertical. Rotate the square of side length l through an angle α about a vertical axis passing through the midpoints of the horizontal sides, and move the square of side length L through a distance h perpendicularly to the original plane. L and l being given, the design of the apparatus requires the numerical values of I [the Neumann integral] for several values of h and a large number of values of α ..." [Gronwall, 1925, 516–517]. Unfortunately, the exact nature of the device has not become available to the author, but it seems likely that it is in the class of instruments including that of Jones discussed previously, in which a loop, coil or disc is suspended within another coil and currents applied. "Radio field intensity" is an archaic phrase which can be used for magnetic or electric field intensity, apparently the ultimate quantities of interest, and the mutual inductance appears to be a stepping stone in its calculation as it was in the determination of resistance or current by Jones. The apparatus also requires the restriction that $l/L < 0.35$, for reasons not explained.

In 1909 F.F. Martens [Martens, 1909] developed an expression for the Neumann integral of any two skew lines; his general expression involved a finite summation of complicated expressions involving trigonometric functions of auxiliary variables as well as the inverse sine function. He, did, however, specialize this to the case $h = 0$ of Gronwall's problem, a fact not noted by Gronwall. These expressions were rejected by George Campbell in a 1915 paper [Campbell, 1915, 42] on the same subject as being "involved and unsatisfactory for actual use," though in fact they were used quite extensively by Grover and Rosa in determining the inductance of polygonal coils and by others in calculating self-inductance of antennae of polygonal shapes (see, for example, [Bashenoff, 1928]). Campbell provided alternative formulae in special cases, and an ingenious analog device for estimating the integral, but these were in

[59] This is not meant as a denigration of the work under consideration, which takes great pains to illustrate the many techniques and their appropriate use, but rather as a contrast to the single work of Gronwall in which the error term in analyzed in more mathematical detail.

[60] It is interesting to note that in the 1948 edition of the book an accuracy of a part in a thousand is the stated general goal. Also, graphs drawn from the tabulated data are subject to the criticism that "... interpolation from the tables is simpler and more accurate than that obtainable from the curves" [Rosa and Grover, 1948, Introduction, xiv]. The volume [Hak, 1938] has many nomograms, including one alignment nomogram.

[61] Later editions of this work, which became a "bible" for such computations, included more specific comments aimed at engineers and the design process, going into much greater detail on the difficulties involved in making choices among the formulae. These include the difficulty of evaluating special functions in exact solutions, the near cancellation of terms read from tables of these functions in the course of a calculation, the determination of the rate of convergence of various series expressions, and the need to combine solutions by integration or summation to cover more complicated inductors. The issues of determining the inductance or mutual inductance in an existing circuit by calculation and designing an inductor having a given inductance by use of these formula are also discussed. The issues are further complicated by the possibility of direct physical measurement, as mentioned earlier, with the attendant measurement errors.

turn rejected by Gronwall, who stated: “While these formulas are very interesting from a theoretical point of view, they do not work well in the present problem... the formulas referred to would consequently have to be evaluated separately and independently for each combination of values of h and α. Even with the aid of the graphical method proposed by Campbell, this numerical work becomes rather formidable” [Gronwall, 1925, 516].

Gronwall’s solution to the problem occupies 20 pages, and his treatment again shows his expertise as a pure mathematician dealing with an applied problem. He obtains the value of the integral as an infinite series of even powers of $\lambda = l/L$ with nth coefficient equal to

$$\sum_{m=1}^{n} f_{mn}(\chi)\cos(2m-1)\alpha, \quad \chi = \frac{2h}{L}. \tag{9}$$

The series converges under the restriction $2\lambda^2 < 1 + \chi^2$, and the f_{mn} are polynomial functions of $a = 1/(1+\chi^2)$ and $b = 1/\sqrt{2+\chi^2}$. Lest this all sound hopelessly complicated, he includes the comment that if only terms up to $n = 3$ inclusive in the series are retained, then an error of less than 0.002 is made. The functions f_{nm} for $n = 1, \ldots, 3$ are explicitly given. Thus the actual computation of the approximation involves only the four basic arithmetic functions, square roots, and the cosine function. The error has also been provided, uniformly over all choices of the variable satisfying the two restrictions $l/L < 0.35$ and $2\lambda^2 < 1 + \chi^2$. We will summarize in one paragraph the outline and special features of this paper.

In the first section the Neumann integral is found by routine substitutions to be equal to the sum of two double integrals over the $[-1, 1] \times [-1, 1]$ of the kernel $(1/r)$ expressed in terms of variables set up for the apparatus. In the second section these kernels are reexpressed in terms of infinite series involving Legendre polynomials. The convergence of these series is guaranteed by the restriction $2\lambda^2 < 1 + \chi^2$. Noting carefully that these series converge uniformly, he reverses the order of integration and summation to obtain, after some involved algebra, the single infinite series having the required even powers of λ. The coefficients of this series are then double integrals of a combination of various Legendre polynomials. In the next section he bounds the error involved in truncating the series at an arbitrary $n = p$; this section invokes the analyst’s tools of triangle inequalities for sums and integrals, reduction formulas, dominating terms in a series by other terms to achieve an inequality, and increasing the domain of an integral of a positive function to make use of a known integral value in bounding. The result is a bound on the remainder of the truncated infinite series; the bound value is numerically compared with the exact remainder in the case of truncation after three terms. In the fourth section he replaces the coefficients in their double integral form with the finite trigonometric series expressions given previously. This involves use of relations between the Legendre polynomials and so-called associated Legendre polynomials as well as the Laplace form of Legendre polynomials to separate the double integrals into the product of two single integrals of auxiliary variables. These are then pulverized into integrals involving only trigonometric functions to make them tractable for evaluation. In the fifth and final section his bound from the third section shows that if only the first three terms of the original infinite series are retained, the error of less than 0.002 is committed under the condition $\lambda < 0.35$, and then the coefficients of the first three terms are evaluated explicitly. It is clear from the method that greater accuracy could be obtained if desired, at the cost of having to evaluate more functions to use in his approximating expressions. The restriction $l < 0.35L$ is only used here.

The use of special functions such as the Legendre polynomials was not unique to Gronwall: other authors had used Bessel functions, for example, to expand the kernel of the Neumann integral. One of the aspects of this paper which is striking, however, is the care with which the estimate of the infinite series is done. A typical paper in which Bessel functions are used may leave the answer with those functions unevaluated or provide an infinite series for them to be truncated by the user with general comments as to the rapidity of convergence under various qualitative relations among the variables [Havelock, 1908]. Other stylistic features present here are similar to those of his earlier papers: the carefully related sequence of equations and variable definitions, the art of transforming extremely complicated expressions, the tabulating of lists of specific functions. One gets a sense of the great amount of pure mathematical effort expended in rendering the integral computable in elementary terms.

This work did not find its way into either of the large collections of such formulas mentioned previously. Perhaps this was due to the complicated nature of the coefficient functions, which take nearly a page to list. It was, however, presented to the American Mathematical Society on April 28, 1923; the published version carries the footnote "This investigation was undertaken at the request of the American Telephone and Telegraph Company, and is published with their permission."[62]

10. Columbia University and the Debye–Hückel theory

In a note to "The Secretary, Columbia University" dated December 22, 1923, Victor K. La Mer, then an instructor of chemistry at Columbia University, made the following request: "Dear Mr. Hayden: Will you please have the paymaster draw a check for $100.00 in favor of T.H. Gronwall for the next date of payment (before January 1st) and charge the same to my research fund" [V.K. La Mer to Secretary Hayden, December 22, 1923]. This is the first evidence of Gronwall's permanent association with Columbia, an association which lasted the rest of his life, and of his collaboration with La Mer, which lasted through the 1920s.[63] Similar requests appear later in the 1920s, at which point La Mer was an assistant professor. These requests, together with two joint publications and two related papers, are some of the fruits of the collaboration between the two men.

La Mer had received his Ph.D at Columbia in 1921 and was at this point at the beginning of his research activities. In the academic year 1922–1923 he had a fellowship which allowed study in Copenhagen and Cambridge, and by 1928 he was chairman of the division of inorganic and physical chemistry of the American Chemical Society. In 1933 he was named chairman of the New York section of the Society. He went on to enjoy a long and distinguished career.

[62] During the spring of 1923 Gronwall was proposed as a candidate for the National Academy of Sciences. Much of the correspondence describing the debates on this issue within the Academy and among other mathematicians of the day is to be found in the Oswald Veblen Papers, National Archives, Washington, DC. The correspondence shows the efforts which Veblen exerted on Gronwall's behalf in trying to influence various members to vote in Gronwall's favor. The case for membership was argued at length in the Academy by George Birkhoff, whom Veblen had delegated for the job, in a meeting which must have been contentious. The membership was not obtained. The correspondence between Birkhoff and Veblen on this matter may be found in the Oswald Veblen Papers, Box 2, Library of Congress.

[63] For the role of Columbia University as a "Times Square" for mathematics, a meeting place for the entire Northeast corridor, see [Lorch, 1988–1989]. Gronwall had of course been to Columbia many times prior to this decade, for example during his low period of 1917. His obituary in the *New York Times* says "Hee [sic] went to Columbia in 1922" [Dr. T.H. Gronwall, obituary, May 12, 1932]. It is likely that he spent much time there after his arrival in New York in 1922, but Hille dates his collaboration with La Mer only from 1925.

Among his interests at this time was the newly developed Debye–Hückel theory of electrolyte solutions, published in 1923 [Debye and Hückel, 1923]. This theory addresses the behavior of salts dissolved in a suitable solvent such as water; under these circumstances the salt dissociates into the ions of which it is constituted. The behavior of such solutions is different from that of neutral dissolved substances like sugar, due to the presence of the charged ions. The theory was intended to explain measurable properties of these solutions from certain basic assumptions, specifically accounting for the "excess electric potential arising from the unequal distributions of the ions," to quote La Mer's description in [Hille, 1932, 779]. The theory can be seen as a historical milepost in the understanding of electrochemistry, an ongoing process dating back to the earliest investigations into electricity, but having a distinct modern era beginning in the 1880s with the dissociation theory of Svante August Arrehenius.[64]

La Mer's interest in the theory appears to have begun quite soon after its publication, and one aspect, in particular, caught his attention: the limitations inherent in the authors' mathematical treatment. The fundamental equation of the Debye–Hückel theory is obtained by combining the Poisson equation for electrostatic potential with Boltzmann's statistical density law to obtain the differential equation

$$\frac{1}{r^2}\frac{\mathrm{d}}{\mathrm{d}r}\left(r^2\frac{\mathrm{d}\psi_i}{\mathrm{d}r}\right) = -\frac{4\pi\varepsilon}{D}\sum_{j=1}^{s}\frac{Nn_jz_j}{V}\mathrm{e}^{-\frac{z_j\varepsilon\psi_i}{kT}}, \quad \text{where } \psi_i \to 0 \text{ for } r \to \infty,$$
$$\frac{\mathrm{d}\psi_i}{\mathrm{d}r} = -\frac{\varepsilon z_i}{D}\cdot\frac{1}{a_i^2} \quad \text{for } r = a_i. \tag{10}$$

In this equation we have $\psi_i =$ electrostatic potential due to an ion of the ith kind at a distance r from that ion, $\varepsilon =$ unit electrical charge, $D =$ dielectric constant of the solution, $N =$ Avagadro's number, $k =$ Boltzmann's constant, $V =$ volume of solution which contains n_0 moles of solvent and n_i moles of ion of the ith kind, $T =$ absolute temperature, and $a_i =$ closest distance of approach of two ions, assumed to be the same for all ions in the solution. This differential equation came to be known as the Poisson–Boltzmann equation. In their solution Debye and Hückel expanded the exponential functions on the right-hand side but retained only the linear term in each summand. It was on the basis of the solution of this linearized equation that the original theory was developed, and experimental verification or contradiction of it referred to this version of the equation.

A great deal of work was done in response to the Debye–Hückel theory both on theoretical extensions and experimental verifications. Some of these experiments yielded results which contradicted the original theory, and applications of the theory to certain types of salts produced ion diameters which seemed unreasonably small or in some cases even negative, an obvious absurdity. In an annotated translation of a German physical chemistry text [Eucken et al., 1925, footnote, 326] published in 1925 La Mer suggested that some of these inconsistencies might disappear if all of the terms in the original differential equation, not just the linear terms, were kept. It was for the solution of this purely mathematical problem of the expanded differential equation and the comparison of its solution to experimental results that Gronwall was consulted.

64 For a summary of this history see [Laidler, 1993]. Arrhenius, incidently, took his Ph.D. at Uppsala in 1884. This account makes it clear that even the idea of dissociation of a salt into its constituent ions was at one time controversial, and many theories were put forth to account for the phenomena of electrolysis.

Gronwall's first response was typical of a pure mathematician's point of view: in an address to the American Mathematical Society on January 2, 1926 he presented a paper [Gronwall, 1927a] which considered the existence of the solutions to differential equations which were generalizations of the Poisson–Boltzmann type, citing the Debye–Hückel paper as motivation, but noting that the authors "assume without proof the existence of a unique solution" [Gronwall, 1927a, 355]. Specific smoothness hypotheses on the coefficient functions were given. The techniques consist of modifications of the well-known Picard method of iteration, and references to earlier treatments of the subject are given. In some cases Gronwall was able to remove certain superfluous assumptions from earlier results. Properties of these solutions were also investigated, as in his ballistics work. Following work done in 1926,[65] Gronwall reported in March 1927 in a paper to the National Academy of Sciences [Gronwall, 1927b] on his solution of the complete Poisson–Boltzmann equation for the case of symmetrical salts of valence type $(z, -z)$, without providing details of this solution. This special case reduces the number of summands on the right-hand side to two, thus creating a single hyperbolic sine function of the input. In this paper Gronwall produced a correction factor based on his solution which spoke to the issue of the ionic diameters: with this factor " …negative diameters can no longer occur, and very small positive ones are likewise excluded, as appears from the following table: …" [Gronwall, 1927b, 201]. The table compared experimental results with the newly calculated diameters.

It was also in 1927, according to Hille, that Gronwall was appointed Associate in Physics at Columbia, a position he retained until his death in 1932.[66] These papers thus form part of what Hille termed the last "period of intense activity start[ing] in 1925 and last[ing] with undiminished strength until his final illness and death" [Hille, 1932, 776].

The bulk of Gronwall's contribution to the Debye–Hückel theory is contained in a 35-page paper which appeared in 1928 [Gronwall et al., 1928]. This paper was co-authored by La Mer and Karl Sandved, who was on a fellowship at Columbia in 1927. It is, of the four applied papers we are considering, the most involved from both the pure and applied points of view. We will again attempt a summary of it, but in this particular case much will have to be left out. The first section is a summary of the problem and the contents of the paper, which is organized so that the reader "der die viele Mathematik nicht liebt" [Gronwall et al., 1928, 358] may skip to the last of the eight sections to obtain the needed comparison with the variables as predicted by the theory and the experimental data. The first section merely states the Poisson–Boltzmann equation and rewrites it by expanding and rearranging the exponentials. The second treats a theoretical issue which we will omit. The third section converts the differential equation into an equivalent integral equation by forming the Green's function for the related homogeneous differential

65 Several requests by La Mer for support for Gronwall from his research fund date from the year 1926. These and the other documents quoted in this paper relating to Gronwall's association with Columbia University are to be found in the Columbia University Archives and Columbiana Collection.

66 Hille points out that "This connection seems to have suited him fully. There were no teaching obligations; he had complete control of his own time and an abundance of new intriguing problems to solve …" [Hille, 1932, 776]. According to the pay card issued for Gronwall for this time (the first page of which is missing in the file at Columbia University Archives), however, there was no salary [T.H. Gronwall, University Record, undated].

equation.[67] To this equation is proposed a series solution of the form

$$\psi_i(\rho) = \sum_{m=1}^{\infty} \frac{\varepsilon^{2m-1} z_i^m}{(Da_i)^m (-kT)^{m-1}} \psi_m(\rho, x_i). \tag{11}$$

In this development the functions $\psi_m(\rho, \xi)$ remain to be determined. This is done by substituting the series into the integral equation and developing a recurrence relation for the $\psi_m(\rho, \xi)$. The quantities which are to be calculated from these functions and compared with experiment are then determined in terms of the $\psi_m(x, x)$; these are the total excess free energy and the activity coefficients of the solvent and solute. All these manipulations are formal only; considerations of the convergence of the infinite series representations are postponed until Section 4, where uniform and absolute convergence are proved by a majorant method which requires much pure analysis; at one point a theorem on the radius of convergence of a power series with positive coefficients is cited (from the contemporary 1927 two-volume *Lehrbuch der Funktionentheorie by Bieberbach*). The fifth section has as its goal the proof that a fundamental law of the Debye–Hückel theory, the Debye limit formula, is still valid with the retention of all the terms in the original differential equation. This section is impossible to summarize in a sentence, but one interesting calculation has the dielectric coefficient assuming complex values in a use of the Cauchy integral formula.

These sections highlight Gronwall's pure analysis skills, the remaining sections show his applied mathematics abilities. Considering the symmetric case (ions with pairwise equal valence), he notes that the terms with even powers in his series expressions vanish, then proceeds to calculate the first three odd power expressions in these series for approximation purposes. This involves the creation of auxiliary variables called X_1, X_3, X_5, and Y_1, Y_3, and Y_5 whose algebraic and integral combinations compose the terms he desires. These variables are all found and expressed in terms of integrals of rational functions, yet more auxiliary series, and the exponential integral function $E(x) = \int_{u=x}^{\infty} \frac{e^{-u}}{u}\, du$.[68] In Section 7 were tabulated values of the six variables just mentioned as well as algebraic combinations of them necessary to compute the truncated versions of the activity coefficients needed for comparison with experimental values. Finally, in Section 8, these truncated versions are computed and compared with experimental data for several specific salts. It is, in fact, the electromotive force which is one of the precise quantities compared; this can be related easily to the activity coefficients. (In a followup paper [Greiff et al., 1931, 2252] one finds the observation that "...and, in general, electrical measurements are the most precise and trustworthy..." as opposed to, say, thermal measurements.)

It should be apparent that this paper involved an enormous amount of work, yet it was not the end of Gronwall's involvement with the Debye–Hückel theory. He generalized his procedures in the 1930

[67] Although this paper cites no reference for this technique, a followup paper [Greiff et al., 1931, 2288] mentions the recently (1924) issued Courant–Hilbert classic *Methoden der Mathematischen Physik* [Courant and Hilbert, 1924, 273–275] as a reference for the method. The conversion of a differential equation to an integral equation can most easily be understood by a special case, namely the undergraduate topic variation of parameters; see [Wiley and Barrett, 1982, 127–137] for a nice treatment.

[68] Elementary properties of this function were developed within [Gronwall et al., 1928, Section 6]. The Debye–Hückel theory was listed as one of the applications for which values of the exponential integral function needed tabulation in *History of the Computation Laboratory of the National Bureau of Standards* by Arnold Lowan [1948, 9], a student of Gronwall at the time of Gronwall's death. Lowan became the head of the Mathematical Tables Project in 1938.

Ph.D. dissertation of Lottie June Greiff, a student of La Mer's, and much of the calculations for this work remained unpublished. Again the conjoining of pure analytic skill with the ability to process a solution for computation provided a combination resulting in useful contributions to a science. If Farrington Daniels' dictum about the lack of expertise in mathematics being the largest impediment to progress in chemical research in the country were true, Gronwall's expertise provided a notable advance. "Only a mathematician with Gronwall's gift for analysis and most uncommon grasp of the literature of chemistry and physics could have contributed the elegant solution which he gave," remarked La Mer in Hille's memorial [Hille, 1932, 780].

One other aspect of this work needs to be discussed. The work done by Gronwall and La Mer dealt only with low-concentration solutions. The original intent was to see whether "the very marked discrepancies which frequently persisted *to extreme dilutions* in the most significant data could be attributed to an incomplete mathematical development" [Hille, 1932, 780], to quote La Mer again from Hille (emphasis added). The introduction to the 1928 paper makes it clear that the results obtained show good agreement only in the case of low concentration.[69]

The issue is of more than scientific import, since industry typically is interested only in the properties of high-concentration solutions.[70] Since Karl Sandved, one of the collaborators on the paper, was partially supported by a Du Pont Fellowship, it is natural to ask if this support was a result of interest by Du Pont in the subject matter at hand, the more so since a 1926 research announcement in *Science* by Gronwall and La Mer [Gronwall and La Mer, 1926] spoke to their interest in extending the Debye–Hückel theory to concentrated solutions.

It is not possible to give a definitive answer to this question, though there are some possibilities to consider. In the 1920s Du Pont was reorganizing their already existing research department to take account of the use of pure science research in their endeavors. Part of this strategy was the initiation of fellowships, which at this point were frankly to be used as recruiting tools to increase their research staff. "Typically, Du Pont annually gave a [university] department one or more industrial fellowships, and the recipient department rotated the Du Pont fellowships among its professors, who in turn granted them to their students" [Hounshell and Smith, 1988]. By 1927 the effectiveness of this scheme was called into question; one aspect up for criticism was that the recipients might not be doing work of interest to the company. The result was the suggestion that the fellowships be assigned to particular professors. Part of this plan involved the formation of a committee to "maintain a complete scholastic record of the individuals to whom these awards are assigned by the colleges, details of the research work performed and other data pertaining to the subject" [Hounshell and Smith, 1988, 290]. Such a committee may have been behind the request made to Sandved on June 9, 1927 to "write them [Du Pont] a letter giving an outline of his [Sandved's] previous training and also a brief description of the research work he proposes to undertake while holding the fellowship" [F.D. Fackenthal to C.H. Sandved, 1927], indicating that the new program was in effect at this point. The alternative is that the fellowship funds were distributed to Columbia University's Department of Chemistry as evidence of general recruiting interest, or of interest in La Mer himself, regardless of the nature of the research undertaken.

69 "Es stellt sich daraus, dass für kleine Konzentrationen—und für diese allein ist ja die ganze Debye–Hückelsche Theorie berechtigt—unsere Formeln mit den beobachtungen gut übereinstimmen" [Gronwall et al., 1928, 358].

70 La Mer himself made this point in a 1935 review of *Electrolytes* by Hans Falkenhagen: "The title of Chapter 11, "More Concentrated Solutions," may prove somewhat disappointing in that one who has not been dealing with the subject might expect that the concentrated solutions of industrial importance are to be discussed" [La Mer, 1935, 154].

The work of Gronwall and La Mer found its way into texts on physical chemistry of the day: Falkenhagen's 1932 volume [Falkenhagen, 1934] discussed it and compared it to other competing theories.[71] The work was cited in texts of the late 1950s, but by the late 1980s it appears that citations had ceased. One factor working against its impact was the complexity of the equations used to express the measurable quantities and the tedium involved in using the tables, which could have had errors. But at the time the paper was considered a solid contribution to the theory of electrolytes in solution.

11. Last years

On April 1, 1929 Gronwall wrote to Oswald Veblen detailing his computing activities in support of the work with Victor La Mer, in particular noting that an assault on the seventh order term in his series solution was desirable, and that he had "also other schemes in quantum theory which call for extensive numerical calculation..." To carry out these activities he requested funds from the National Research Fund "to engage the services of a computer... [T.H. Gronwall to Oswald Veblen, April 1, 1929]. The tables in the 1928 paper had taken up three pages of the work, so clearly there was much computing taking place. The last of the papers we consider is a brief treatment involving algorithms for solving a well-worked-over problem and some attendant computational issues of the digital kind, as opposed to the analog nomographical work Gronwall had done early in his career.

In the June–July 1929 issue of the *American Mathematical Monthly* Gronwall published "The Number of Arithmetical Operations Involved in the Solution of a System of Linear Equations" [Gronwall, 1929]. The problem was to make a determination of the count for each of the additions, multiplications, and divisions necessary to solve a nondegenerate n by n linear system of algebraic linear equations in variables $x_1, \dots, x_n$. Gronwall first notes that the problem has practical applications, citing the use of calculus of variations in the elastic vibrations of a plate, which yields such a system for large n. He rules out the use of determinants immediately, "...since determinants of high order are among the most unpleasant objects to handle numerically" [Gronwall, 1929, 325]. Thus the issue is one of efficiency of computation: given the elementary problem at hand, what is the least costly method? He states that systematic substitution appears to be the best approach: dividing the first equation through to achieve a coefficient of 1 on the x_1 variable, he solves for x_1 in terms of the remaining variables and substitutes this into the remaining equations.[72] He finds the simple difference equations which relate the operation counts for the original n by n system and the new $n-1$ by $n-1$ system, and notes their solution follows quickly. The results give familiar cubic polynomials in n for the operation counts. He investigates the count in the case of a symmetric coefficient matrix, and tries alternative substitution techniques, but states that none of the other methods attempted produce fewer total operations than his first approach.

In the final sentence he notes that the choice of method for solution "may be influenced by the type of calculating machine used; with a machine with automatic division, the first method is preferable, while

[71] Falkenhagen states that after the setting of the original Poisson–Boltzmann equations with initial conditions, "The subsequent calculations consist merely [!] of replacing [the differential equation] by a single integral equation with boundary conditions. The solution of this equation may be obtained in the form of a series" [Falkenhagen, 1934, 271].

[72] In a 1900 paper which dealt with the same problem within the context of fitting a line to data [Goedseels, 1900, 53], the author remarks: "Nous supposons qu'on résolve les équations finales par élimination successive, parce que c'est ce procédé qui est le plus souvent suivi par les calculateurs."

with a machine such as the "Millionaire," where division is cumbersome but multiplication is extremely rapid, the second method may have its advantages" [Gronwall, 1929, 327]. This remark reminds us that the increasing complexity of mathematical calculations was subject to the restriction of the calculating devices of the day.

The Millionaire calculator was a Swiss machine patented in 1893 and put on the market in 1899. One of its features was a mechanical multiplication table in which only one turn of the operating handle was required for each digit of the multiplier; this was considered a speedup of the usual multiplication process. Division was clumsy for, among other reasons, an estimate of the answer had to be entered before the calculation of the actual answer [Baxandall and Pugh, 1975]. Thus the method chosen was dependent on a knowledge of calculating devices, which Gronwall surely had at this point.[73]

This is a mere note, and it would be unfair to saddle it with more significance than is appropriate, but it is clear that, within its confines, it shows both the pure mathematician's awareness of approaches to the simple problem at hand as well as the applied knowledge of computational devices of the day. This was not the first consideration of the problem: as early as 1853 Jules Bienaymé [1853] stated without proof a cubic polynomial in n as the total operation count for solving an n by n system, and in 1900 Goedseels provided another estimate; both papers are within the context of fitting a line to data.[74]

In his last years Gronwall worked on problems involving quantum mechanics, as suggested by his letter to Veblen. He published a paper on the hydrogen wave equation in 1931, and his preliminary work on the helium wave equation was collected after his death in 1932, ultimately to be used by J.H. Bartlett, Jr. to good effect in a 1937 publication, for which Gronwall is also credited [Bartlett, 1937; Gronwall, 1937]. He was invited in 1929, along with five others, to participate in a symposium on the mathematics of engineering, sponsored by the American Mathematical Society on a Saturday of its annual meeting, the specific topic being the differential equations of engineering [Richardson, 1929].[75] But his material circumstances appeared to have deteriorated during these last years.[76] Hille speaks only of Gronwall's "final illness and death" [Hille, 1932, 776] on May 9, 1932. In addition to Hille's piece in the *Bulletin*, Gronwall was remembered by J.A. Shohat of the University of Pennsylvania in "The Life and Work of T.H. Gronwall" during the 17th annual meeting of the MAA in December of 1932 [Shohat, 1933], and by an obituary by Columbia University mathematician Joseph F. Ritt in *Science* [Ritt, 1932].

Gronwall's career in America highlights several aspects of the role of mathematics in scientific and industrial settings in the first decades of the 20th century. It is convenient to address these issues by referring to one final contemporary account of the relationship of mathematics to industry, that of the Cornell University Professor of Engineering Vladimir Karapetoff, whose career spans a time overlapping

[73] How much familiarity a typical American pure mathematician would have which such instruments at this time is hard to judge, but a remark in [Locke, 1924] states "Certain it is that the calculating machine has not attracted the attention of the mathematician to the extent it deserves, witness the complete absence of literature on the subject in American technical journals and an almost equal void in foreign journals" [Locke, 1924, 422].

[74] Goedseels' count was made by treating addition and subtraction as having value one each, and finding the log or antilog of a number as one operation each; no multiplications or divisions were calculated as such. The references for Goedseels and Bienaymé may be found in [Farebrother, 1999]. Neither considered machine dependence.

[75] "This part of the program is being arranged because of a wish expressed by some members of each of the two groups—mathematicians and research engineers—for closer cooperation."

[76] One sign of this can be seen in the United States Census of 1930, which lists Gronwall as a "roomer" with address 3609 Broadway, a 29-unit complex with lower rent than his earlier 68 Bank Street dwelling, where he occupied one of five units. Of course the Stock Market crash of 1929 may also have played a role in this change.

that of Gronwall. Karapetoff, a Russian-born and educated electrical engineer, came to America in 1902, worked for Westinghouse Electric and Manufacturing Company in East Pittsburgh, Pennsylvania until being offered a position at Cornell University in 1904, where he stayed until his retirement in 1939. In an article entitled "The Mathematical Thread in My Life" [Karapetoff, 1939], written at the end of his career, he offered a few observations which are useful in summarizing our work.

Karapetoff's first comment is that "There would be less waste, more speed, better performance, and better chance for our products on international markets if more mathematics, theoretical physics, and analytical mechanics were used in our industries" [Karapetoff, 1939, 65]. He did not feel that it was necessary, however, to give engineering students more instruction in these matters, but preferred a "bucket brigade" arrangement, as he experienced in Germany at that time:

> A practical radio man is troubled with the performance of a vacuum tube and is at the end of his resources as to remedies. He discusses the difficulty with a theoretical practician who feels that the electrons could not be made to move in paths desired by the practical man. He sees the elements and the factors in the problem but cannot express them mathematically. So he strips the problem of the unessentials and lays it before a practical theoretican who expresses the relationships and the desiderata in a mathematical form. However, he is not able to solve the resulting equations, lays the problem before a theoretical theoretican in that particular field of equations, and gets advice on how to proceed. The problem then travels back and finally reaches the practical practician, perhaps in the form of a few numerical data, a curve, or a simple formula, with which he can proceed with the problem. [Karapetoff, 1939, 65]

This description fits in well with the conception of Gronwall as a consulting mathematician, clearly identifiable with Karapetoff's "theoretical theoretician." This kind of activity was Gronwall's chief role in each of the four consultations highlighted above; his solution of Saint-Venant's equations for the stress on a keyed shaft assumed that the theory had already been worked out, for example. There was a place for his outstanding mathematical abilities, provided a theory already existed, as was the case for the Neumann integral, the ballistics equations, and the Poisson–Boltzmann equations of the Debye–Hückel theory. Karapetoff's bucket brigade idea is somewhat at odds with the idea that more science and applied mathematics needed to be taught in universities, a position taken for example by Bliss and Birkhoff and Gronwall himself (recall his complaint regarding the lack of useful engineering mathematics in university settings, upon reviewing Runge's lectures.) In either case a lack of this kind of knowledge in higher education is revealed.

Karapetoff then remarks that "There is a widespread naive belief among engineers (the belief being fostered by elementary courses in engineering) that a practical problem can always be solved step by step. You first decide upon the length of the shaft; from this you determine its diameter. Then you compute the size of the flywheel, etc. In reality, this is a problem in simultaneous equations, all the variables being interdependent in a rather complicated manner... The important point is for the engineer to see this interdependence clearly" [Karapetoff, 1939, 65–66]. He mentions that he had constructed several instruments which mechanically displayed the relations among several variables satisfying equations arising in engineering problems. Here we encounter the desire for computational devices which incorporate higher mathematics into a usable form, reducing sophisticated mathematics to routine calculation. This reminds us that Gronwall had taken pains in each of the works discussed to make his solutions usable to computers, as was required for the absorption of higher mathematical ideas. Also inherent in this remark is the observation that during this era mathematics began to be used as a tool for application because, among other things, it had the ability to express the relationships between many relevant variables simultaneously; the idea that these relationships should be acknowledged and used, a commonplace of today's mathematical modeling, was apparently not universally accepted at this time. Bliss and Campbell had mentioned this idea in their essays. The best of our examples of the inclusion of many variables is the treatment of ballistics given by the Aberdeen group, where such factors as air density, angle of fire, wind, rain, and other variables were simultaneously incorporated into the model of the ballistic trajectory.

The last of the three major points made by Karapetoff is that "Theoretical achievements outside this country remain unknown to our engineers, or are disregarded by them for long periods of time, to the detriment of the industry, and indirectly of the country at large. The same is true of new branches of mathematics, first proposed for assistance in a new engineering problem... As a practical man once said to me: 'You cannot generate electricity out of the square root of minus one'... Conjugate functions, Fourier Series, differential equations of damped oscillations, matrices, tensors—all these had to fight their way into our engineering circles" [Karapetoff, 1939, 66]. Gronwall clearly was working in a time when there was an increasing use of this kind of higher-level mathematics, especially in the treatment of electrical problems, but it was not readily accepted by all, and those who practiced it were only on rare occasions organized into laboratory settings: "A few large industrial organizations, such as the Bell Telephone System and General Electric Co., are on a fair way to permanent self-sufficiency [with regard to employing scientific and mathematical "middlemen"], but smaller concerns still are at the stage thus expressed to me by an owner: "I hire a Ph.D. and prod him to solve my problems; then I fire the Ph.D" " [Karapetoff, 1939, 66]. The response to the 1922 publication of the Saint-Venant treatment of torsion in bars illustrates the willingness on the part of some to indulge the newer approaches, but the reservations expressed by P. Schwartz to the new methods of calculating ballistic trajectories also demonstrates the wariness of others of the new higher mathematical ideas.

Gronwall's career in America illustrates issues discussed by Karapetoff as well as other commentators we have cited, perhaps the more so because his role as consulting mathematician was more a matter of default than design, not a choice he would have preferred to make. His first love was clearly pure mathematics, and his outstanding work there is universally acknowledged and cited. Even in the last years of his life he continued to publish papers on harmonic functions, summability, and Bessel functions. However, his eccentricities did not allow for a permanent position either in the academic world or in an industrial setting such as Bell Laboratories, and his drifting life thus illustrates the many changes of the day rather than revealing him as a major figure in any of them. The rise of the industrial laboratory and its slow acceptance of mathematics as a tool, the pioneering use of mathematics in a war-time setting, the deficiencies of college engineering mathematics, the attitudes of various parties to the use of increasingly sophisticated mathematics in applied settings, the importance of the reduction of theory to computation, and the need for more high-level mathematics in physics and chemistry were all issues experienced by Gronwall, but his interest in this work and devotion to it was not comparable to that of full-time practitioners such as Steinmetz, Campbell, Fry, or Karapetoff. Yet his contributions show an incisive mathematical mind quite aware of the needs of the users of this mathematics, which include some major scientists and engineers such as Victor La Mer and J.R. Carson.

His life also indirectly reflects the growth and consolidation of the American mathematical community. As the mathematics departments of major universities such as University of Chicago, Harvard, Princeton, and Columbia were established and grew, we find Gronwall associated with them in various ways, contributing to their growth, and we find mathematicians attempting to help him in his wayward course. This assistance took the form of both job placement at the Aberdeen Proving Grounds and Government Ordnance and funding for research in pure mathematics (the conformal mapping work). The most significant helper in this regard was Oswald Veblen, but G.D. Birkhoff argued on his behalf and E.H. Moore administered grant funds for him after applying for a Bache Fund award for research in pure mathematics. The well-documented communications among these men show a degree of organization of mathematical culture, another obvious instance of which was the concentration of mathematical talent on an emergency basis at the Aberdeen Proving Grounds.

An observation made to the author by Karen Parshall at the beginning of this work has proven quite relevant: sometimes the contours of a mathematical community become clearer when one considers the life of someone who does not readily fit into any of its existing categories. T.H. Gronwall was such a figure in American mathematics of his day, and it might be appropriate to summarize what roles he did not play. He was not a pure mathematician associated long term with a university in a teaching position, though he published over 80 papers in pure mathematics and consulted with, among others, Edward Kasner and Joseph Ritt of Columbia University as well as J.L. Walsh and Caroline Seeley. He was not an applied mathematician at a university whose publications would be of occasional interest to engineers, though some of his work resembles such material. He was not a physicist or chemist at a university, though he worked with such people and published papers on physical and chemical topics. He was not a long-term employee of the National Bureau of Standards or other government agency, as was Edward Rosa (the NBS physicist who contributed to a large literature in electrical metrology) though his mutual inductance paper would have fit in well with that agency's publications. He was not a mathematician employed long term by an industrial concern, as were J.R. Carson and George Campbell, though he worked with such people. He was not a professor of engineering at a university, though some of his work might have come from the theoretical side of such an employee.

It would seem that Gronwall's self-description as a consulting mathematician in 1925 provides the best summary of his professional life not just at that point, but from the beginning of his career in America, though not by premeditation. His analytical abilities were available to those who desired the full use of pure mathematics, be it industry, government, or academia, and each experienced an increasing need for these services in the period we have described.

In a letter of October 17, 1924 to L.E. Dickson, occasioned by his unsuccessful attempts to secure an NAS membership for Gronwall, Oswald Veblen expressed, with some impatience, his friend Gronwall's single-minded devotion to his work: "The actual fact is that Gronwall is a man who has no interest in anything except scientific work, and he consequently appears to all normal people as somewhat wrong" [Oswald Veblen to L.E. Dickson, October 17, 1924]. A gentler assessment is that given by Hille when he wrote, "His [Gronwall's] life shows that his unruly spirit found expression, joy, and satisfaction in scientific thinking and creation" [Hille, 1932, 780].

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