Andrew M. Gleason 1921–2008

Ethan D. Bolker, coordinating editor

Photo by Bachrach.

Andrew M. Gleason

Andrew M. Gleason was one of the quiet giants of twentieth-century mathematics, the consummate professor dedicated to scholarship, teaching, and service in equal measure.

He was too modest to write an autobiography. The folder marked "memoir" in his files contains just a few outdated copies of his impressive CV. But those of us lucky to have known him will offer in the essays that follow some reflections on his mathematics, his influence, and his personality: codebreaking during the Second World War; his role in solving Hilbert's Fifth Problem; Gleason's Theorem in quantum mechanics; contributions to the study of operator algebras; work in discrete mathematics; concern for mathematics education as a teacher, author, and reformer; and his service to the profession.

Vita

Andrew Mattei Gleason was born November 4, 1921, in Fresno, California, to Eleanor Theodolinda Mattei and Henry Allan Gleason. He died in Cambridge, Massachusetts, on October 17, 2008.

He grew up in Bronxville, New York, and was graduated from Roosevelt High School, Yonkers, in 1938. He received his B.S. from Yale in 1942. While at Yale he placed in the top five in the Putnam Mathematical Competition in 1940, 1941, and 1942, and was the Putnam Fellowship winner in 1940.

In 1942 he enlisted in the navy, where he served as a cryptanalyst until the end of the war. He was recalled to active duty during the Korean War and retired from the navy in 1966 with the rank of commander.

Gleason went to Harvard in 1946 as a Junior Fellow of the Society of Fellows. He was appointed assistant professor of mathematics in 1950 and associate professor in 1953, when Harvard awarded him his highest degree, an honorary A.M. He became a full professor in 1957. From 1969 until his retirement in 1992 he was the Hollis Professor of Mathematicks and Natural Philosophy.

Throughout his time at Harvard he maintained his association with the Society of Fellows, serving as a Senior Fellow for nineteen years and as its chairman from 1989 to 1996.

In 1952 the American Association for the Advancement of Science awarded Gleason the Newcomb Cleveland Prize for his work on Hilbert's Fifth Problem. He was elected to the American Academy of Arts and Sciences in 1956, to the National Academy of Science in 1966, and to the American Philosophical Society in 1977.

From 1959 to 1964 he chaired the Advisory Board of the School Mathematics Study Group; he was cochairman of the Cambridge Conference on School Mathematics in 1963 and a member of the Mathematical Sciences Education Board from 1985 to 1989.

Gleason delivered the Mathematical Association of America's Hedrick Lectures in 1962. He was president of the American Mathematical Society in 1981–82 and served on the Council of Scientific Society Presidents 1980–83. He was chairman of the organizing committee and president of the International Congress of Mathematicians, Berkeley, 1986.

On January 26, 1959, he married Jean Berko, who is now professor emerita of psychology at Boston University. They have three daughters: Katherine, born in 1960; Pamela, born in 1961; and Cynthia, born in 1963.

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Ethan D. Bolker

50+ Years...

I first met Andy in 1956, when he taught sophomore abstract algebra at nine in the morning (even on fall football Saturdays). He agreed to let me audit his course and submit homework papers.

It took me several years and two graduate courses to realize how deceptive a lecturer he was. The proofs scrolled by. You could read his writing. He literally dotted his i's and crossed his t's. I know; I recently found the purple dittoed handwritten linear algebra notes he wrote for us in the spring of 1957. Strangely, those notes were sometimes subtly hard to study from. Now I know why. He took such care preparing and searched so hard for economy and elegance that the rough places were made plain. The hard parts didn't seem so in the seamless flow, so it could be hard to find the crux of a proof. George Mackey once told me it was good that one of his teachers (I choose to forget who) was disorganized because it forced him (George) to master the material for himself. What Andy's style proves is that disorganization may be sufficient but is not necessary.

Andy was the reader for my undergraduate thesis on multiplicity theory for eigenvalues of bounded self-adjoint operators on Hilbert space. In those days (perhaps still) each senior was set a special exam on the thesis topic. One question on mine asked me to apply my theorems to the multiplication operator $g \mapsto fg$ for g in a Hilbert space $L^2(\mu)$. The function f was a cubic polynomial, and, μ was Lebesgue measure on $[0, 2]$ with an extra atom of weight 1 at 1. Fortunately, I'd thought of putting an example like that in the thesis, so I knew how to do the problem. What mattered was where the cubic was $1:1$, $2:1$, or $3:1$. But my answer seemed not to need the atom at 1. When I asked Andy later about that, he gently pointed out how he'd carefully constructed his cubic with a local maximum at 1, so there was a set of positive measure on which the cubic was $2:1$. I missed that, because when finding the critical points I calculated $2 \times 3 = 12$. He graciously said only that I'd spoiled a good problem.

When I chose Andy as a doctoral thesis advisor I had neither a topic nor a direction. I thought I was an analyst and thought he was one and knew him, so I opted to try to work with him. I struggled with p-adic groups for a year, going nowhere. But I did have one idea about a way I might prove the Radon-Nikodym theorem for measures on lattices like those that come up in quantum mechanics. The idea didn't work, but I did manage to say some new things about measures on Boolean algebras even while the generalizations to lattices eluded me. Andy encouraged that play and said after a while that what I was working on was in fact my thesis. He told me he liked it better when his students found topics than when he had to suggest one.

In the spring of 1964 I thought my thesis was done. I found the central theorem in February, wrote it up, and sent it off to Andy. When I telephoned to ask what he thought of it, he asked if I needed my degree in hand to accept my new job at Bryn Mawr. When I said "no" he said, "Work on it another year." I know that if I'd said "yes" he'd have accepted what I'd written. But then I'd have had a thesis with just a theorem. The central mechanism for producing examples and counterexamples showing the theorem was sharp came later that spring. Moreover, I think the idea was his, although I didn't give him due credit then. So Andy was right to care about the quality of the work and to ask for the extra year. The thesis was better and better written and ready for publication soon after the degree was awarded—and my year-old daughter got to go to my commencement. (He and Jean sent her a Raggedy Andy when she was born.)

Eighteen years later Andy employed her as a painter. That's how I learned how he applied logic outside mathematics. She saw him eating breakfast hurriedly one day—peanut butter spread on bread right out of the freezer. He said the nutritional value was the same.

I was telling Andy once about a bijection I'd found for counting permutations with particular cycle structures. He was interested and had some further ideas and references. When he suggested a joint paper [BG] I jumped at the chance to earn a Gleason number of 1. When I wanted to say something numerical about the asymptotics which called for $\Gamma(1/3)$, I looked up nearby values in a table and interpolated. In response to a draft I sent Andy he wrote back:

> There is one not terribly important thing where I can't check you. You obtain
>
> $$\frac{3^{1/6} e^{\pi\sqrt{3}/18}}{\Gamma(1/3)} \approx 0.6057624.$$
>
> With my hand calculator I found $\Gamma(1/3) \approx 2.678938543$ (of which at least 8 figures ought to be right) and hence the above number comes out 0.6065193. Hand calculators make substantial errors in exponentials, so I really don't know which is right.

Andy's "With my hand calculator I found ..." is a little disingenuous. There's no Γ key on the calculator—he programmed the computation. Today Mathematica quickly finds $\Gamma(1/3) \approx 2.67893853471$ with twelve significant figures, so Andy's intuition about eight was right.

Over the years I had lunch with Andy often, sampling Chinese, Vietnamese, and Indian food in Cambridge and nearby towns. Over lunch once, thinking about geometry, he told me he'd give a lot for "one good look at the fourth dimension." Any mathematical topic, at any level of sophistication, was fair game. I'd tell him why I thought the convention for writing fractions was upside down; he'd tell me he was thinking about the foundations of geometry or the Riemann hypothesis. Often in the past year I've wanted to ask him about something that came up in my teaching or while editing these essays and was stunned anew by the realization that I couldn't ever do that again.

Solving Cubics by Trisecting Angles

Andy was a problem solver more than a theory builder. He liked hard problems, like Hilbert's Fifth, about which you can read more below. Others less deep interested him no less. I think he even enjoyed the problems in spherical trigonometry and navigation on the exams he took to maintain his naval commission while in the reserves.

Once he set out to discover which regular polygons you could construct if you add the ability to trisect angles to the tasks available with Euclidean straightedge and compass. His answer, in "Angle trisection, the heptagon, and the triskaidecagon" [G1]: just the n-gons for which the prime factorization of n is of the form $2^r 3^s p_1 p_2 \cdots p_k,$ where the p_i are distinct primes greater than 3, each of the form $2^t 3^u + 1$. His proof depends on the observation that these are precisely the primes for which the cyclotomic field has degree $2^t 3^u$ and so can be constructed by a sequence of adjunctions of roots of quadratics and of cubics, all of whose roots are real.

You solve such a cubic by trisecting an angle, because when the cubic has three real roots (the *casus irreducibilis*), finding them with Cardano's formula requires extracting the cube root of a complex number. To do that you trisect its polar angle and find the cube root of the modulus. For the particular cubics that come up in the construction of these regular polygons, the modulus is the $3/2$ power of a known quantity, so a square root computes the cube root.

Andy's solution to that problem allowed him to indulge several of his passions. The paper is full of historical references, including the corollary that the ability to trisect angles doesn't help you duplicate the cube. That requires solving the *other* kind of cubic. He cites (among others) Plemelj, Fermat, Euler, and Tropfke and concludes with a quote from Gauss's *Disquisitiones Arithmeticae.*[1]

The "triskaidecagon" in the title, where most of us would be satisfied with "13-gon", exemplifies Andy's love of language. He had lots of ideas he never got around to publishing. I wonder if he wrote this paper in part just so he could use that word.

Andy loved to compute too. About his construction of the triskaidecagon he writes:

> After considerable computation we obtain

$$12\cos\frac{2\pi}{13} = \sqrt{13} - 1 + \sqrt{104 - 8\sqrt{13}}\cos\frac{1}{3}\arctan\frac{\sqrt{3}(\sqrt{13}+1)}{7-\sqrt{13}}.$$

Mathematica confirms this numerically to one hundred decimal places. I don't think there's software yet that would find the result in this form.

I first explicitly encountered Andy's passion for precision of expression when in graduate school he told me that the proper way to read "101" aloud is "one hundred one" without the "and". That passion stayed with him to the end: when he was admitted to the hospital and asked to rate his pain on a scale of 1 to 10, he's reputed to have said first, "That's a terrible scale to use…"

Andy told me once that he knew he wanted to be a mathematician just as soon as he outgrew wanting to be a fireman.[2] He succeeded.

Stories

In the essays that follow you'll find more about Andy's mathematics and more stories. I'll close here by quoting some that aren't included there.

Persi Diaconis writes about Andy's legendary speed:

> Andy was an (unofficial) thesis advisor. This was illuminating and depressing. My thesis was in analytic number theory, and I would meet with Andy once a week. A lot of the work was technical, improving a power of a logarithm. I remember several times coming in with my current best estimates after weeks of work. Andy glanced at these and said, "I see how you got this, but the right answer is probably . . . " I was shocked, and it turns out he was right.

Jill Mesirov describes a similar experience:

> I remember quite clearly the first time that I met Andy Gleason. I was working at IDA in Princeton at the time, and Andy was a member of the Focus Advisory Committee. The committee met twice a year to review the work being done, and I had been asked to give a

[1] *Andy seems to have missed Viète's construction [V]. My thanks to Robin Hartshorne for this reference and for some clarifying comments on this section.*

[2] *Perhaps I'm misremembering. His wife, Jean Berko Gleason, said, "He loved looking at the stars. He knew every star in the sky and could tell you their names. Early on, he was planning on becoming an astronomer, but then he learned how cold it was to sit outside and watch."*

> presentation of some work on speech I had done jointly with Melvin Sweet. I worked hard on the presentation, and designed it to give some idea of how we were led step by step to the answer. The groundwork was laid for revealing each insight we had gained, but in such a way that it should come as a surprise to the audience and thus make them appreciate the sense of discovery we had enjoyed as we did the research and solved the puzzle ourselves. Needless to say, I hadn't counted on Andy's "infamous" speed!
>
> Twice I carefully led the audience through some twisted trail to end with the question, "So, what do you think we tried next?" Twice, before the words had begun to leave my mouth, Andy was saying, "Oh, I see, then you want to do this, this, and this, after which you'll observe that …" While I appreciated his quick grasp of the issues, I was beginning to see my carefully laid plans falling by the wayside. Therefore, as I was reaching the next crescendo, and I saw Andy leaning forward in his seat, I turned around, pointed my finger at him and shouted, "You, be quiet!" He smiled, and left me to lead the rest of the crowd through the revelations.

Finally, Victor Manjarrez, a graduate school contemporary of mine, offers this summary:

> In the late fifties and early sixties I took graduate algebra and a reading course from Andrew Gleason. Whenever we spoke at meetings in later years I was struck by how unfailingly polite he always was. The English word "polite" (marked by consideration, tact, or courtesy) evokes the French "politesse" (good breeding, civility), and the Greek "polites" (citizen—of the mathematics community and the world), all of which Andrew Gleason exemplified to the fullest. This, of course, in addition to his amazing erudition.

References

[BG] Ethan D. Bolker and Andrew M. Gleason, Counting permutations, *J. Combin. Theory Ser. A* **29** (1980), no. 2, 236–242. MR583962 (82b:05009).

[G1] A. M. Gleason, Angle trisection, the heptagon, and the triskaidecagon, *Amer Math. Monthly* **95** (1988), 185–194. Addendum, p. 911. MR 90c:51023.

[V] François Viète, *Supplementum Geometriae*, 1593, (Opera, 1646, pp. 240–257). (See Robin Hartshorne, *Geometry: Euclid and Beyond*, Springer, 2000, p. 265.)

John Burroughs, David Lieberman, Jim Reeds

The Secret Life of Andy Gleason

Andrew Gleason was a senior at Yale on December 7, 1941, when the Japanese bombed Pearl Harbor. He applied for a commission in the navy; upon his graduation the following June he reported to their crypanalytic service: the Office of Chief of Naval Operations (OPNAV), 20th Division of the Office of Naval Communications, G Section, Communications Security (OP-20-G). There he joined a group of eight to ten mathematicians working to crack enemy codes. The group included Robert E. Greenwood and Marshall Hall Jr., one of Andy's Yale professors. The National Archives contains declassified documents describing much of the wartime work of OP-20-G. We found there a set called *Enigma Studies* [ES], which describes the group's contributions to the attack on the German Enigma machine. These documents showcase that part of Gleason's work which we will describe.

The Enigma ciphers presented diverse and rapidly mutating challenges. The Germans used several different models of the Enigma machine. Each day they changed the keys on each of perhaps a hundred or so different communications networks. Three of those networks were "Shark", used by the Atlantic U-boat fleet; "Sunfish", by blockade runners and the German U-boats in the Pacific; and "Seahorse", for traffic between German navy HQ and their attaché in Tokyo. Breaking one system or one day's traffic provided only clues towards breaking the others, clues which were sometimes misleading. Several times during the war the Germans made significant modifications to the Enigma.

OP-20-G worked on Enigma in collaboration with the British cryptanalysts at the Government Code and Cypher School at Bletchley Park, in particular, with "Hut 8", whose most famous member

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We are also grateful to R. Erskine and F. Weierud for searching through the wartime diaries of OP-20-G and the history of Coral, which they had copied at the National Archives. They located information about Gleason's activities and forwarded many interesting items to us.

Andy Gleason in uniform—left, active duty (1940s) and right, Naval Reserve (1960s).

was Alan Turing. Shortly before the U.S. entered the war the British code breakers began teaching their American counterparts about the Enigma problem: the general theory and notation (largely due to Turing), a host of particular solution methods, and the design of a special-purpose electromechanical computing machine, the "Bombe", which carried out one of the steps of the arduous constrained trial-and-error solution process. Some of these lessons the British had learned from the Poles just before the war; some they had developed during the first two years of the war. At about the time the U.S. entered the war the German navy began using a four-wheel model of the Enigma machine, against which the existing Bombes (designed for attacking three-wheel Enigmas) were comparatively ineffective. This ended the Allies' ability to read Shark in a timely manner during 1942 and early 1943, with devastating consequences to Allied shipping.

In November 1942 Turing visited the U.S. to assist the Americans in mastering Bletchley's methods and to consult on the construction of the American Bombes designed to attack the four-wheel Enigma. Andrew Hodges's biography of Turing discusses Turing's report back to Bletchley, in which he expresses some dismay that the Americans really had not grasped the British work-saving algorithmic ideas, relying instead on technological overkill. Nevertheless, he was impressed with the mathematicians being hired at OP-20-G, in particular "the brilliant young Yale graduate mathematician, Andrew Gleason" [H, p. 243]. Hodges relates an anecdote from this visit:

Gleason and Joe Eachus "looked after Alan during his period in Washington. Once Gleason took Alan to a crowded restaurant on 18th Street. They were sitting at a table for two, just a few inches from the next one, and talking of statistical problems, such as that of how to best estimate the total number of taxicabs in a town, having seen a random selection of their license numbers. The man at the next table was very upset by hearing this technical discussion, which he took to be a breach of 'security'. … Alan said, 'Shall we continue our conversation in German?'"

At first OP-20-G was the junior partner to Hut 8, but later it took the lead in attacking Seahorse. A recent paper [EM] describes the Seahorse story in considerable detail. We draw on that account. The Hut 8 team had failed to solve Seahorse because traffic externals convinced them that encryption was being done not by a standard naval Enigma machine but by a simpler version of Enigma. In March 1943 the problem was turned over to OP-20-G. Their analysis revealed new structure in the traffic that allowed them to reject the hypothesized simpler version of Enigma to correctly diagnose an underlying naval Enigma and to specify a menu (i.e., a program) for the naval Bombes so they could efficiently read the traffic.

Reading the first-hand accounts of this work in the *Enigma Studies* E-2 and E-4, one can share the excitement, the frustrations, and finally the elation when success was achieved. Greenwood's "History of Kriegsmarine attack" (paper 6 in E-2) describes how the group first discovered the working of Seahorse's "indicators", which told the message recipient how to set up his receiving Enigma machine. These were appended to the message, encrypted in a "throw-on" cipher. The standard method of attack required interception of a large number of messages from the same day. A successful attack revealed which set of four wheels was being used in the machine and the setting used to encipher all the indicators. One could then decipher the indicators and in turn use them to decipher all the day's messages.

Marshall Hall noticed an interesting feature of the throw-on indicators. On a given day the set of first letters of encrypted indicators for Berlin-to-Tokyo messages was disjoint from the set of first letters for Tokyo to Berlin, but the sets changed from day to day. Gleason came up with and statistically tested a simple hypothesis explaining this, namely, that the wheel settings—the unencrypted form of the indicators—started with letters A–M for messages from Tokyo to Berlin and letters N–Z for the opposite direction.

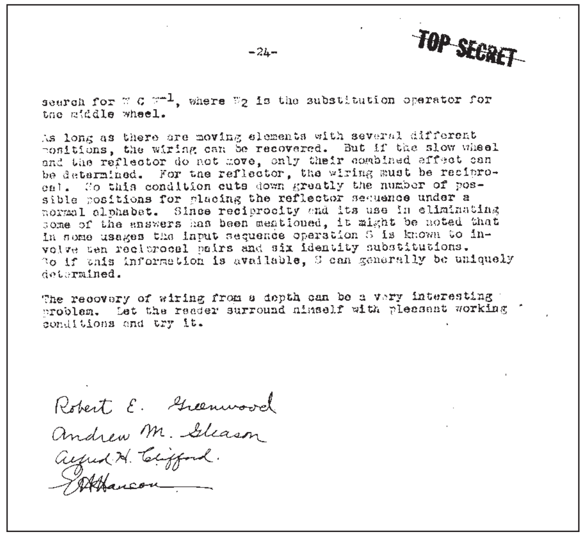
In "Kriegsmarine indicators" (paper 5 in E-2) Gleason, Greenwood, and Hall show how this structure allowed one to derive extra equations, which led to much more efficient Bombe runs. The practical implication was that far fewer messages needed to be intercepted in any one day to be able to work out that day's key. In August 1943 a Bombe run at Bletchley using these ideas broke the first Seahorse messages. This confirmed the model for the underlying Enigma and its indicator system and verified the correctness of their attack programs. This concluded the research phase. The problem was then turned over to a development team. Using the newly available four-wheel naval Bombes and the special tricks discovered for Seahorse, they were able to read Seahorse sporadically in 1943 and almost continuously in 1944 and 1945, resulting in the decryption of thousands of messages.

An excellent discussion of the application of group theoretic ideas to the solution of Enigma problems is given in Greenwood, Gleason, Clifford, and Hanson's "Enigma wiring recovery from the reading of a depth" (paper 7 in E-4, dated 19 April 1945).[1] Simplifying slightly, they reduce the problem of finding the four permutations performed by the Enigma wheels to one of solving a system of simultaneous equations in permutations. The right side of the t-th equation is the permutation effected by Enigma to encipher the t-th plain text character. These are assumed "known" (at least in part) by "depth reading". The left side of the t-th equation expresses the known way the Enigma machine composes the unknown rotor wirings at time t. The paper shows how this problem can be broken into a series of subproblems of the form "given several permutation pairs (π_i, σ_i), find a single permutation x for which $\pi_i = x\sigma_i x^{-1}$ for all i." Interestingly, one of these subproblems is solved by exhibiting an isomorphism of a pair of labeled graphs. The explanation, unlike most technical Enigma exposition of the era, such as found in [T], is couched in standard mathematical terminology.[2] According to one modern commentator, [W], the method in this paper "is a lot more powerful than the 'Rodding' and 'Buttoning-up' methods described by Alan Turing, mainly because it allows recovery of the wiring even when the Stecker is unknown." The exposition is both compelling and charming. One can imagine one is listening to the young Andy Gleason in some of the informal asides:

> The reader may wonder why so much is left to the reader. A book on swimming strokes may be nice to read, but one must practice the strokes while actually in the water before one can claim to be a swimmer. So if the reader desires to actually possess the knowledge for recovering wiring from a depth, let the reader get his paper and pencils, using perhaps four colors to avoid confusion in the connecting links, and go to work.... Note: the writing of $C^{-1}C^2$ instead of C^1 is a whim of the writer. Please humor him to this extent....

[1] *There is a copy of this paper on the Net in Frode Weierud's Enigma Archive website,* `http://cryptocellar.org/Enigma/EnigmaWiringRecovery.pdf`.

[2] *This use of group theory had to be modified in a novel and clever way to exploit the extra structure available in the Seahorse indicator problem. This is described in "Kriegsmarine indicators" (paper 4 in E-4) by Gleason and Greenwood.*



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search for $W C W^{-1}$, where W_2 is the substitution operator for the middle wheel.

As long as there are moving elements with several different positions, the wiring can be recovered. But if the slow wheel and the reflector do not move, only their combined affect can be determined. For the reflector, the wiring must be reciprocal. So this condition cuts down greatly the number of possible positions for placing the reflector sequence under a normal alphabet. Since reciprocity and its use in eliminating some of the answers has been mentioned, it might be noted that in some usages the input sequence operation S is known to involve ten reciprocal pairs and six identity substitutions. So if this information is available, S can generally be uniquely determined.

The recovery of wiring from a depth can be a very interesting problem. Let the reader surround himself with pleasant working conditions and try it.

Robert E. Greenwood
Andrew M. Gleason
Alfred H. Clifford
E.H. Hanson

The final page of the typescript (shown above) concludes:

> The recovery of wiring from a depth can be a very interesting problem. Let the reader surround himself with pleasant working conditions and try it.

—as if this were a problem in pure mathematics, not an urgent wartime endeavor.

The mass production of four-wheel Bombes led to dramatic successes against Seahorse and the other naval Enigma problems. Research then focused on Japanese machine ciphers. A major achievement of this effort was the diagnosis of the naval cipher Coral and the subsequent decryption of Coral traffic throughout 1944–45. This required, as in Enigma, a painstaking examination of the traffic to find and explain statistical structure and the design and programming of new special purpose machines. Gleason's mathematical contributions to this work included the eponymous "Gleason crutch", a method for estimating extreme tail probabilities for sums of random variables. It can be regarded as a version of Chernoff's theorem in large deviations theory, and like it, it is based on the idea of exponential tilting.

As the war came to an end, Gleason participated in efforts to systematically document the techniques developed by OP-20-G and to set up a postwar curriculum for training cryptanalysts. He also participated in courses and seminars on both applied methods and mathematical foundations, including (prophetically) one based on Pontrjagin's new book on topological groups.[3]

Gleason's codebreaking work exhibits some of his characteristic traits. One is his extraordinary quickness in grasping the heart of a problem and

[3] *Our comments on the post-Enigma work are based on extracts from the wartime diaries of OP-20-G and the voluminous Coral history, which were culled out for us by R. Erskine and F. Weierud.*

rethinking how to solve it from first principles. He says of himself, "I have always felt that it's more crucial for me to come to grips on my terms with the most elementary aspects of a subject. I haven't worried much about the advanced aspects" [AAR, p. 88]. He was particularly effective as a cryptanalyst, because (in his own words) he "learned to do something that a lot of pure mathematicians don't know how to do…how to do quick and dirty mathematics. It's an interesting knack to be able to make a quick appraisal as to whether there is sufficient statistical strength in a situation so that hopefully you will be able to get an answer out of it" [AAR, p. 87].

Gleason's insight into the mathematics underlying cryptography was greater than that of most of his colleagues. Even during the war he prepared lectures and notes for them to help develop their understanding and working knowledge of the mathematical fundamentals. His OP-20-G lecture notes and exercises on probability and statistics were later gathered up and edited into a short textbook used for years in introductory courses at NSA and subsequently reprinted commercially [G]. We were amused to find that one of the exercises in the book is to estimate the number of taxicabs in a town, having seen a random selection of their license numbers.

Legend has it that, in general, the cryptanalysts in World War II did not think much of the mathematicians down the hall, who were always telling them what was wrong with their counts and suggesting "proper" statistics, which in the end didn't produce plain text. But Gleason was different. He was approachable. He listened carefully to their problems and ideas, and his advice was always useful.

After the war Gleason began his academic career at Harvard. He was reactivated in 1950, at the start of the Korean War, and served at the naval facility on Nebraska Avenue. Cryptographic systems had increased in complexity, incorporating digital technology that posed new challenges and dramatically increased the need for trained mathematicians. Many of Gleason's colleagues at Nebraska Avenue went on to significant careers at the newly formed NSA and in emerging academic areas of mathematics and computer science. These included Marshall Hall, Joe Eachus, Dick Leibler, Oscar Rothaus, Howie Campaigne, Bill Blankinship, and Ned Neuburg. In the spring of 1951, Lt. Cmdr. Gleason, Lt. Cmdr. Hall, and Cmdr. Miller[4] were sent to visit mathematics departments around the country to recruit mathematicians with advanced degrees. They found sixty to eighty. One of them, R. Highbarger, told us, "After a few weeks at training school we located in a basement room next to the kitchen grease pit at Arlington Hall Station.… There, we took various CA [cryptanalysis] courses and a bunch of math courses, the best of which were taught by Andy Gleason." Some twenty of these "junior mathematicians" were to become the professional leaders of the nation's cryptanalytic effort in the 1960s and 1970s.

Most of Gleason's applied work during this period remains classified. In his spare time he worked on Hilbert's Fifth Problem. He later said, "there wasn't a single day that I didn't think about it some of the time.… I made a real breakthrough on the problem around February of 1952" [AAR, p. 91]. R. A. Leibler drove Gleason to Princeton to present a talk on his new result at the Institute for Advanced Study. It was snowing hard. Going up Alexander Road they were nearly hit when their car slid through a red light. Leibler told us that Gleason came in dressed in his navy uniform. This caused some initial surprise, which soon turned to excitement and enthusiasm. Gleason lectured all day.

After the Korean War, Gleason returned to the Harvard faculty but continued as an advisor to the nation's intelligence and security programs for fifty years. He served on the NSA Scientific Advisory Board from the mid-1950s through the mid-1960s, where he helped shape the NSA's response to the evolving challenges of the cold war. He continued as an active recuiter for the NSA and for the Communications Research Division (CRD) of the Institute for Defense Analyses, often writing or calling the director to recommend a mathematician who might be particularly well suited for an appointment to the program. He participated in NSA summer research projects (SCAMP) and in the formative technical programs of the CRD. He was a member of the first CRD advisory committee in 1959, then served from 1976 to 1979, from 1986 to 1988, and again from 2004 to 2006.

L. P. Neuwirth, writing in 1992, recalled Gleason's participation in the CRD programs:

> He invariably had something useful to contribute, and the work of others benefited enormously, either directly or indirectly (sometimes with attribution, sometimes without) from his ideas. His contributions have in many cases been lasting, and were made in sufficient generality and depth that they still find application 45 years later. His name is associated with some of these notions… the Gleason semigroup, Gleason weights, and the Gleason crutch.…[his] 22 papers in cryptologic mathematics span the time period from 1945 to 1980. Their content range is wide, and includes algebra, combinatorics, analysis, statistics and computer science.

Gleason did pioneering work in computational algebra in response to the emerging need for good

[4] *We believe this was D. D. Miller, the semigroup theorist, who was also an alumnus of OP-20-G.*

pseudorandom number generators and efficient error correcting codes. In 1955 the Gleason-Marsh Theorem [GM][5] provided a method for generating irreducible polynomials of huge degree over GF(q). (For any d, one could generate irreducibles of degree $q^d - 1$.) In 1961 in a 10-page typescript [G61] Gleason described algorithms he devised for factoring and irreducibility testing of univariate polynomials over GF(q). Programs implementing these ideas had many years of utility. We do not undertake to compare Gleason's unpublished approach to other methods soon to follow: Berlekamp (1967), Zassenhaus (1969), Cantor-Zassenhaus (1981). For a recent historical survey of the field, see, for example, [VGG].

Neuwirth concluded:

> This unfortunately restricted list of some of the ideas he had and some of the areas to which he contributed perhaps sheds a little light on his many contributions to a very much hidden science, and gives some understanding of the unusually high regard in which he is held by the intelligence community …[GLIM, pp. 65–66].

References

[AAR] D. J. Albers, G. L. Alexanderson, and C. Reid, eds., *More Mathematical People*, Harcourt Brace Jovanovich, Boston, 1990.

[EM] R. Erskine and P. Marks, Naval Enigma: Seahorse and other Kriegsmarine cipher blunders, *Cryptologia* **28**:3 (2004), 211–241.

[ES] *Enigma Studies*, branch of the U.S. National Archives and Records Administration, College Park, MD, Record Group 38, "Radio Intelligence Publication Collection", boxes 169 through 172.[6]

[G] A. M. Gleason, *Elementary Course in Probability for the Cryptanalyst*, Aegean Park Press, Laguna Hills, CA, 1985.

[G61] ____, Factorization of polynomials over finite fields with particular reference to the cyclotomic polynomials, August 1961.

[GM] A. M. Gleason and R. Marsh, A method for generating irreducible polynomials, May 1955. Problem 4709, *MAA Monthly*, December 1957, 747–748.

[GLIM] *Andrew M. Gleason: Glimpses of a Life in Mathematics*, privately printed Festschrift, Boston, 1992.

[H] A. Hodges, *Alan Turing: The Enigma*, Simon and Schuster, New York, 1983.

[T] Alan Turing, *Treatise on the Enigma*, Kew: The National Archives [of the UK], HW 25/3; photocopy College Park: U.S. National Archives and Records Administration, Historic Cryptologic Collection, RG 457, Entry 9032, Box 201, Item 964; facsimile on the Web: `http://www.turingarchive.org/browse.php/C/30`.

[VGG] J. von zur Gathen and J. Gerhard, *Modern Computer Algebra*, 2nd ed., Cambridge Univ. Press, 2003, pp. 396–407.

[W] Frode Weierud, Enigma Archive website, `http://cryptocellar.org/Enigma`.

[5] *A rediscovery of Theorem 1 in Ore, Contributions to the theory of finite fields,* Trans. Amer. Math. Society ***36*** *(1934), 260.*

[6] *A nine-volume anthology of technical papers, compiled at the end of the war, covering all of OP-20-G's Enigma research activities. The volume titles are E-1, Click Process; E-2, Indicator Attacks; E-3, Statistical Studies; E-4, Wiring Recovery; E-5, Bomb Computation; E-6, Duenna; E-7, Miscellaneous; E-8, Reports from England; and E-9, Bulldozer. Each bears a Radio Intelligence Publication (RIP) number. Volumes 1 through 8 are RIP numbers 603 through 610, and volume 9 is RIP 601. We will make available online articles from E-2, E-4, and E-9 written by Gleason and his colleagues.*

Richard Palais

Gleason's Contribution to the Solution of Hilbert's Fifth Problem

What *Is* Hilbert's Fifth Problem?

Andy Gleason is probably best known for his work contributing to the solution of Hilbert's Fifth Problem. We shall discuss this work below, but first we need to know just what the "Fifth Problem" is. In its original form it asked, roughly speaking, whether a continuous group action is analytic in suitable coordinates. But as we shall see, the meaning has changed over time.

As Hilbert stated it in his lecture delivered before the International Congress of Mathematicians in Paris in 1900 [Hi], the Fifth Problem is linked to Sophus Lie's theory of transformation groups [L], i.e., Lie groups acting as groups of transformations on manifolds. The "groups" that Lie dealt with were really just neighborhoods of the identity in what we now call a Lie group, and his group actions were defined only locally, but we will ignore such local versus global considerations in what follows. However, it was crucial to the techniques that Lie used that his manifolds should be analytic and that both the group law and the functions defining the action of the group on the manifold should be analytic, that is, given by convergent power series. For Lie, who applied his theory to such things as studying the symmetries of differential equations, the analyticity assumptions were natural enough. But Hilbert wanted to use Lie's theory as part of his logical foundations of geometry, and for this purpose Hilbert felt that analyticity was unnatural and perhaps superfluous. So Hilbert asked if analyticity could be dropped in favor of mere continuity. More precisely, if one only assumed a priori that the group G was a locally Euclidean topological group,

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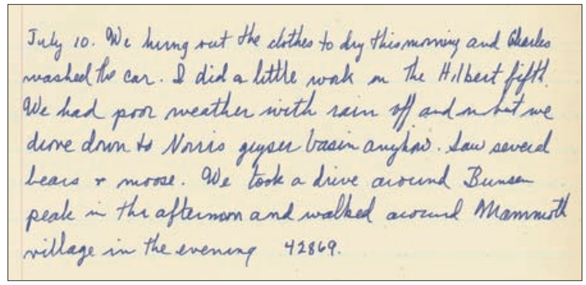
that the manifold M was a topological manifold, and that the action of G on M was continuous, could one nevertheless always choose local coordinates in G and M so that both the group operations and the action became analytic when expressed in these coordinates? We shall speak of the problem in this generality as the *unrestricted* Hilbert Fifth Problem. The *restricted* problem is the important special case in which $G = M$ and the action is left translation. Asking whether we can always find analytic coordinates in the restricted problem is clearly the same as asking whether a locally Euclidean group is necessarily a Lie group.


July 10. We hung out the clothes to dry this morning and Charles washed the car. I did a little work on the Hilbert fifth. We had poor weather with rain off and on but we drove down to Norris geyser basin anyhow. Saw several bears & moose. We took a drive around Bunsen peak in the afternoon and walked around Mammoth village in the evening. 42869.

Excerpt from Gleason's vacation journal, July 1947, in which he mentions working on the "Hilbert fifth".

Counterexamples

It turned out that there are many—and in fact many different kinds of—counterexamples to the unrestricted Hilbert Fifth Problem. Perhaps the first published counterexample, due to R. H. Bing [Bi], is an action of $\mathbf{Z}_2$ on $\mathbf{S}^3$ whose fixed-point set is the Alexander Horned Sphere Σ. Now Σ is not "tamely embedded" in $\mathbf{S}^3$, meaning that there are points where it is impossible to choose coordinates so that locally Σ looks like the usual embedding of $\mathbf{R}^2$ in $\mathbf{R}^3$. If the action were even differentiable in some suitable coordinates, then it is easy to see that the fixed-point set would in fact be tamely embedded. (For an even more bizarre type of counterexample, recall that in 1960 M. Kervaire [Kerv] constructed a topological manifold that did not admit any differentiable structure, providing what can be considered a counterexample even for the case when G is the trivial group.)

One could make a case that these examples are "monsters" that could have been ruled out if Hilbert had phrased his statement of the Fifth Problem more carefully. But there is a more serious kind of counterexample that is so elementary that it makes one wonder how much thought Hilbert had given to the Fifth Problem before proposing it. Here is a particularly elementary example, due to Montgomery and Zippin [MZ3], with $G = \mathbf{R}$, the additive group of the real numbers, and $M = \mathbf{C}$, the complex plane. Let f be a continuous real-valued function defined on the positive real axis, and define the action $\phi : \mathbf{R} \times \mathbf{C} \to \mathbf{C}$ by $\phi(t, re^{i\theta}) := re^{i(\theta + f(r)t)}$. (In words, ϕ is a one-parameter group of homeomorphisms of the plane that rotates each circle centered at the origin into itself, the circle of radius r being rotated with angular velocity $f(r)$.) If we choose $f(r)$ to equal 1 for $r \leq 1$ and 0 for $r \geq 2$, the action is the standard one-parameter group of rotations of $\mathbf{C}$ inside the unit disk and is trivial outside the disk of radius 2, so by the Principle of Analytic Continuation, this action cannot be made analytic in any coordinate system. What is worse, we can choose f to have these properties and also be smooth (meaning C^∞), so we see that even if we assume a priori that the action of a Lie Group on a manifold is smooth, it does not follow that it can be made analytic!

After these counterexamples to the unrestricted Hilbert Fifth Problem became known, a tacit understanding grew up to interpret "the Fifth Problem" as referring to the restricted version: Is every locally Euclidean group a Lie group? and we shall follow this convention below.

Early History of the Fifth Problem

It was fairly easy to settle the one-dimensional case. The only (paracompact) connected manifolds of dimension one are the real line, $\mathbf{R}$, and the circle, $\mathbf{S}^1$, and both of course are Lie groups. In 1909 L. E. J. Brouwer [Br] showed that a topological group that is homeomorphic to either of these is in fact isomorphic to it as a topological group. Using results from Brouwer's paper, B. Kerékjártó [Kere] settled the two-dimensional case in 1931. There seems to have been little if any published work on the Fifth Problem between the papers of Brouwer and Kerékjártó, but that is not too surprising considering that much of the modern mathematical infrastructure required for a rigorous discussion of topological groups and the Fifth Problem became available only after a 1926 paper by O. Schreier [Sch]. The three-dimensional and four-dimensional cases of the Fifth Problem were settled much later, by Montgomery [M1] in 1948 and by Montgomery and Zippin [MZ1] in 1952.

The first major breakthrough in the general theory came in 1933, when J. von Neumann [VN], using the recently discovered Haar [Ha] measure, extended the Peter-Weyl Theorem [PW] to general compact groups and used it to settle the Fifth Problem in the affirmative for compact groups. We will sketch a proof of von Neumann's theorem below. Several years later, building on von Neumann's work, Pontryagin [Po] settled the abelian case, and Chevalley [Ch1] the solvable case.

The No Small Subgroups (NSS) Condition

The first time I encountered the phrase "group without small subgroups" I wondered what kind of subgroup a "small" one could possibly be. Of course, what the phrase means is a topological group without *arbitrarily* small subgroups, i.e., one having a neighborhood of the identity that includes no subgroup except the trivial group. We shall follow Kaplansky [Ka] and call this the NSS Condition and a group satisfying it an NSS group. Since NSS may seem a little contrived, here is a brief discussion of the "why and how" of its use in solving the Fifth Problem.

It turns out to be difficult to draw useful conclusions about a topological group from the assumption that it is locally Euclidean. So the strategy used for settling the Fifth Problem was to look for a more group-oriented "bridge condition" and use it in a two-pronged attack: on the one hand show that a topological group that satisfies this condition is a Lie group, and on the other show that a locally Euclidean group satisfies the condition. If these two propositions can be proved, then the positive solution of the Fifth Problem follows—and even a little more.

As you may have guessed, NSS turned out to be ideally suited to play the role of the bridge. In retrospect this is not entirely surprising. A powerful but relatively elementary property of Lie groups is the existence of so-called canonical coordinates, or equivalently the fact that the exponential map is a diffeomorphism of a neighborhood of zero in the Lie algebra onto a neighborhood U of the identity in the group (see below). Since a line through the origin in the Lie algebra maps to a one-parameter subgroup of the group, it follows that such a U contains no nontrivial subgroup and hence that Lie groups satisfy NSS.

Starting in the late 1940s Gleason [G1],[1] Montgomery [M2], and Iwasawa [I] made several solid advances related to the Fifth Problem. This led in 1952 to a satisfying denouement to the story of the Fifth Problem, with Gleason and Montgomery-Zippin carrying out the above two-pronged attack. First Gleason [G3] proved that a locally compact group satisfying NSS is a Lie group, and then immediately afterwards Montgomery and Zippin [MZ1] used Gleason's result to prove inductively that locally Euclidean groups of any dimension satisfy NSS. Their two papers appeared together in the same issue of the *Annals of Mathematics*, and at that point one knew that for locally compact topological groups:

$$\text{Locally Euclidean} \iff \text{NSS} \iff \text{Lie.}$$

(Actually, the above is not quite the full story; Gleason assumed a weak form of finite dimensionality in his original argument that NSS implies Lie, but shortly thereafter Yamabe [Y2] showed that finite dimensionality was not needed in the proof.)

[1] *As far as I can tell, this 1949 paper was the first journal article to define the NSS condition. But there is clear evidence (see journal entry, above) that Gleason was already preoccupied with the Fifth Problem in mid-1947.*

Cartan's Theorem

Starting with von Neumann, all proofs of cases of the Fifth Problem, including Gleason's, were ultimately based on the following classic result that goes back to É. Cartan. (For a modern proof, see Chevalley [Ch2], page 130.)

Theorem (Cartan). *If a locally compact group has a continuous, injective homomorphism into a Lie group and, in particular, if it has a faithful finite-dimensional representation, then it is a Lie group.*

Here is a quick sketch of how the proof of the Fifth Problem for a compact NSS group G follows. Let $\mathcal{H}$ denote the Hilbert space $L^2(G)$ of square-integrable functions on G with respect to Haar measure. Left translation induces an orthogonal representation of G on $\mathcal{H}$, the so-called regular representation, and, according to the Peter-Weyl Theorem, $\mathcal{H}$ is the orthogonal direct sum of finite-dimensional subrepresentations, $\mathcal{H}_i$, i.e., $\mathcal{H} = \bigoplus_{i=1}^{\infty} \mathcal{H}_i$. Define $W_N := \bigoplus_{i=1}^{N} \mathcal{H}_i$. We will show that for N sufficiently large, the finite-dimensional representation of G on W_N is faithful or, equivalently, that for some N the kernel K_N of the regular representation restricted to W_N is the trivial group $\{e\}$. Since the regular representation itself is clearly faithful, K_N is a decreasing sequence of compact subgroups of G whose intersection is $\{e\}$. Thus if U is an open neighborhood of e that contains no nontrivial subgroup, $K_N \setminus U$ is a decreasing sequence of compact sets with empty intersection and, by the definition of compactness in terms of closed sets, some $K_N \setminus U$ must be empty. Hence $K_N \subseteq U$, and since K_N is a subgroup of G, $K_N = \{e\}$.

Following in Gleason's Footsteps

Andy Gleason put lots of remarks and clues in his papers about his motivations and trains of thought, and it is an enjoyable exercise to read these chronologically and use them to guess how he developed his strategy for attacking the Fifth Problem.

Let's start with a Lie group G, and let $\mathfrak{g}$ denote its Lie algebra. There are (at least !) three equivalent ways to think of an element of the vector space $\mathfrak{g}$. First as a vector v in TG_e, the tangent space to G at e; second as the left-invariant vector field X on G obtained by left translating v over the group; and third as the one-parameter subgroup ϕ of G obtained as the integral curve of X starting at the identity. The exponential map $\exp : \mathfrak{g} \to G$ is defined by $\exp(v) = \phi(1)$. It follows immediately from this definition that $\exp(0) = e$ and that the differential of exp at 0 is the identity map of TG_e, so by the inverse function theorem, exp maps a neighborhood of 0 in $\mathfrak{g}$ diffeomorphically onto a neighborhood of e in G. Such a chart for G is called a canonical coordinate system (of the first kind).

Now, suppose we somehow "lost" the differentiable structure of G but retained our knowledge of G as a topological group. Is there some way we could use the latter knowledge to recover the differentiable structure? That is, can we reconstruct $\mathfrak{g}$ and the exponential map? If so, then we are clearly close to a solution of the Fifth Problem. Let's listen in as Andy ponders this question.

"Well, if I think of $\mathfrak{g}$ as being the one-parameter groups, that's a group theoretic concept. Let's see—is there some way I can invert exp? That is, given g in G close to e, can I find the one-parameter group ϕ such that $\phi(1) = \exp(\phi) = g$? Now I know square roots are unique near e and in fact $\phi(1/2)$ is the square root of g. By induction, I can find $\phi(1/2^n)$ by starting with g and taking the square root n times. And once I have $\phi(1/2^n)$, by simply taking its m-th power I can find $\phi(m/2^n)$ for all m. So, if I know how to take square roots near e, then I can compute ϕ at all the dyadic rationals $m/2^n$, and since they are dense in $\mathbf{R}$, I can extend ϕ by continuity to find it on all of $\mathbf{R}$!"

Photograph courtesy of Richard Palais.

Andy with George Mackey (2000). Although Andy never earned a Ph.D., he thought of George as his mentor and advisor and lists himself as George's student on the Mathematics Genealogy Project website.

This was the stated motivation for Gleason's paper "Square roots in locally Euclidean groups" [G1], and in it he goes on to take the first step and show that in any NSS locally Euclidean group G, there are neighborhoods U and V of the identity such that every element in U has a unique square root in V. Almost immediately after this article appeared, in a paper called "On a theorem of Gleason", Chevalley [Ch3] went on to complete the program Andy outlined. That is, Chevalley used Gleason's existence of unique square roots to construct a neighborhood U of the identity in G and a continuous mapping $(g, t) \mapsto \phi^g(t)$ of $U \times \mathbf{R}$ into G such that each ϕ^g is a one-parameter subgroup of G, $\phi^g(t) \in U$ for $|t| \leq 1$, and $\phi^g(1) = g$.

In his key 1952 *Annals* paper "Groups without small subgroups" [G3], Gleason decided not to follow up this approach to the solution of the Fifth Problem and instead used a variant of von Neumann's method. His approach was based on the construction of one-parameter subgroups, but these were used as a tool to find a certain finite-dimensional invariant linear subspace Z of the regular representation of G on which G acted faithfully and appealed to Cartan's Theorem to complete the proof. The construction of Z is a technical tour de force, but it is too complicated to outline here, and we refer instead to the original paper [G2] or the review by Iwasawa.

Andy Gleason as Mentor

Looking back at how it happened, it seems almost accidental that I became Andy Gleason's first Ph.D. student—and David Hilbert was partly responsible.

As an undergraduate at Harvard I had developed a very close mentoring relationship with George Mackey, then a resident tutor in my dorm, Kirkland House. We had meals together several times each week, and I took many of his courses. So, when I returned in 1953 as a graduate student, it was natural for me to ask Mackey to be my thesis advisor. When he inquired what I would like to work on for my thesis research, my first suggestion turned out to be something he had thought about himself, and he was able to convince me quickly that it was unsuitably difficult for a thesis topic. A few days later I came back and told him I would like to work on reformulating the classical Lie theory of germs of Lie groups acting locally on manifolds as a rigorous modern theory of full Lie groups acting globally. Fine, he said, but then explained that the local expert on such matters was a brilliant young former Junior Fellow named Andy Gleason who had just joined the Harvard math department. Only a year before he had played a major role in solving Hilbert's Fifth Problem, which was closely related to what I wanted to work on, so he would be an ideal person to direct my research.

I felt a little unhappy at being cast off like that by Mackey, but of course I knew perfectly well who Gleason was and I had to admit that George had a point. Andy was already famous for being able to think complicated problems through to a solution incredibly fast. "Johnny" von Neumann had a similar reputation, and since this was the year that *High Noon* came out, I recall jokes about having a mathematical shootout—Andy vs. Johnny solving math problems with blazing speed at the OK Corral. In any case, it was with considerable trepidation that I went to see Andy for the first time.

Totally unnecessary! In our sessions together I never felt put down. It is true that occasionally when I was telling him about some progress I had made since our previous discussion, partway through my explanation Andy would see the crux of what I had done and say something like, "Oh! I see. Very nice! and then…," and in a matter of minutes he would reconstruct (often with improvements) what had taken me hours to figure out. But it never felt like he was acting superior. On the contrary, he always made me feel that we were colleagues, collaborating to discover the way forward. It was just that when he saw his way to a solution of one problem, he liked to work quickly through it and then go on to the next problem. Working together with such a mathematical powerhouse put pressure on me to perform at top level—and it was sure a good way to learn humility!

My apprenticeship wasn't over when my thesis was done. I remember that shortly after I had finished, Andy said to me, "You know, some of the ideas in your thesis are related to some ideas I had a few years back. Let me tell you about them, and perhaps we can write a joint paper." The ideas in that paper were in large part his, but on the other hand, I did most of the writing, and in the process of correcting my attempts he taught me a lot about how to write a good journal article.

But it was only years later that I fully appreciated just how much I had taken away from those years working under Andy. I was very fortunate to have many excellent students do their graduate research with me over the years, and often as I worked together with them I could see myself behaving in some way that I had learned to admire from my own experience working together with Andy.

Let me finish with one more anecdote. It concerns my favorite of all Andy's theorems, his elegant classification of the measures on the lattice of subspaces of a Hilbert space. Andy was writing up his results during the 1955–56 academic year, as I was writing up my thesis, and he gave me a draft copy of his paper to read. I found the result fascinating, and even contributed a minor improvement to the proof, as Andy was kind enough to footnote in the published article. When I arrived at the University of Chicago for my first position the next year, Andy's paper was not yet published, but word of it had gotten around, and there was a lot of interest in hearing the details. So when I let on that I was familiar with the proof, Kaplansky asked me to give a talk on it in his analysis seminar. I'll never forget walking into the room where I was to lecture and seeing Ed Spanier, Marshall Stone, Saunders Mac Lane, André Weil, Kaplansky, and Chern all looking up at me. It was pretty intimidating, and I was suitably nervous! But the paper was so elegant and clear that it was an absolute breeze to lecture on it, so all went well, and this "inaugural lecture" helped me get off to a good start in my academic career.

References

[Bi] R. H. Bing, A homeomorphism between the 3-sphere and the sum of two solid horned spheres, *Ann. of Math.* **56** (1952), 354–362.

[Br] L. E. J. Brouwer, Die theorie der endlichen kontinuierlichen Gruppen, ubhängig von den Axiomen von Lie, *Math. Annalen* **67** (1909), 246–267 and 69; (1910), 181–203.

[Ch1] C. Chevalley, Two theorems on solvable topological groups, *Michigan Lectures in Topology* (1941), 291–292.

[Ch2] ______ , *Theory of Lie Groups*, Princeton Univ. Press, 1946.

[Ch3] ______ , On a theorem of Gleason, *Proc. Amer. Math. Soc.* **2** (1951), 122–125.

[G1] A. Gleason, Square roots in locally Euclidean groups, *Bull. Amer. Math. Soc.* **55** (1949), 446–449.

[G2] ______ , On the structure of locally compact groups, *Duke Math. J.* **18** (1951), 85–104.

[G3] ______ , Groups without small subgroups, *Ann. of Math.* **56** (1952), 193–212. Reviewed by Iwasawa: MR0049203 (14,135c).

[Ha] A. Haar, Der Massbegriff in der theorie der kontinuierlichen gruppen, *Ann. of Math.* **34** (1933), 147–169.

[Hi] D. Hilbert, *Mathematische Probleme*, Nachr Akad. Wiss., Göttingen, 1900, 253–297.

[I] K. Iwasawa, On some types of topological groups, *Ann. of Math.* **50** (1949), 507–557.

[Ka] I. Kaplansky, *Lie Algebras and Locally Compact Groups*, Univ. of Chicago Press, 1971; Corrected second printing, 1974.

[Kere] B. Kerékjártó, Geometrische theorie der zweigliedrigen kontinuierlichen gruppen, *Hamb. Abh.* **8** (1931), 107–114.

[Kerv] M. A. Kervaire, A manifold which does not admit any differentiable structure, *Comment. Math. Helv.* **34** (1960), 257–270.

[L] S. Lie, *Theorie der Transformationsgruppen, unter Mitwirkung von F. Engel*, Leipzig, 1893.

[M1] D. Montgomery, Analytic parameters in three-dimensional groups, *Ann. of Math.* **49** (1948), 118–131.

[M2] ______ , Properties of finite-dimensional groups, *Proc. Int. Congr. Math. (Cambridge, U.S.A.)*, Vol. II, 1950, pp. 442–446.

[M3] ______ , Topological transformation groups, *Proc. Int. Congr. Math. (Amsterdam)*, Vol. III, 1954, pp. 185–188.

[MZ1] D. Montgomery and L. Zippin, Small subgroups of finite-dimensional groups, *Ann. of Math.* **56** (1952), 213–241.

[MZ2] ______ , Examples of transformation groups, *Proc. Amer. Math. Soc.* **5** (1954), 460–465.

[MZ3] ______ , *Topological Transformation Groups*, Interscience Publishers, New York–London, 1955.

[PW] F. Peter and H. Weyl, Die Vollständigkeit der primitiven Darstellungen einer geschlossenen kontinuierlichen Gruppe, *Math. Ann.* **97** (1927), 737–755.

[Po] L. Pontryagin, *Topological Groups*, Princeton Univ. Press, 1939.

[Sch] O. Schreier, Abstrakte kontinuierliche gruppen, *Hamb. Abh.* **4** (1926), 15–32.

[Se] J.-P. Serre, Le cinquième probleme de Hilbert. État de la question en 1951, *Bull. Soc. Math. France* **80** (1952), 1–10.

[VN] J. von Neumann, Die einführung analytischer parameter in topologischen gruppen, *Ann. of Math.* **34** (1933), 170–190.

[Y1] H. Yamabe, On the conjecture of Iwasawa and Gleason, *Ann. of Math.* **58** (1953), 48–54.
[Y2] ______ , A generalization of a theorem of Gleason, *Ann. of Math.* **58** (1953), 351–365.

John Wermer

Gleason's Work on Banach Algebras

Introduction

I first came to know Andy Gleason in the early 1950s. I found him friendly, natural, and interesting. Of course, I knew that his work had recently led to the solution of Hilbert's Fifth Problem. One thing that impressed me strongly about Andy was that he understood, in detail, every colloquium we attended independently of the subject matter.

A link between the Gleason and Wermer families at that time was Philip, Jean and Andy's Siamese cat. I was a visitor at Harvard in 1959–60, and Andy was going abroad for that year. We rented their apartment. They asked us to take care of Philip for the year, which my two boys and my wife, Christine, and I were happy to do. When spring 1960 came and we knew we should soon have to surrender Philip, it turned out that the Gleasons would not be able to keep him and asked us whether we would take him along to Providence. We accepted with a whoop and a holler. We called him Philip Gleason, and he became a much-valued member of our household. Philip often disappeared for days, but always returned, thinner and wiser, and definitely had more than nine lives.

A mathematical link between Andy and me came out of the former Soviet Union. Gelfand and Silov had recently started a study of commutative Banach algebras and their maximal ideal spaces, and this theory was intimately related to the theory of holomorphic functions. This area aroused the strong interest of a group of young American mathematicians. Andy Gleason was a prominent member of this group and made fundamental contributions to this field of study.

Let $\mathcal{A}$ be a commutative semisimple Banach algebra with unit, and let $\mathcal{M}$ be the space of all maximal ideals of $\mathcal{A}$. Gelfand [1] showed that $\mathcal{M}$ can be endowed with a topology which makes it a compact Hausdorff space such that there is an isomorphism : $f \to \hat{f}$ which maps $\mathcal{A}$ to a subalgebra of the algebra of all continuous functions on $\mathcal{M}$. Silov [2] then showed that there exists a minimal closed subset $\check{S}$ of $\mathcal{M}$ such that to every f in $\mathcal{A}$ and each point m in $\mathcal{M}$ we have the inequality

$$|\hat{f}(m)| \leq \max_{s \in \check{S}} |\hat{f}(s)|. \tag{1}$$

$\check{S}$ is called the Silov boundary of $\mathcal{M}$.

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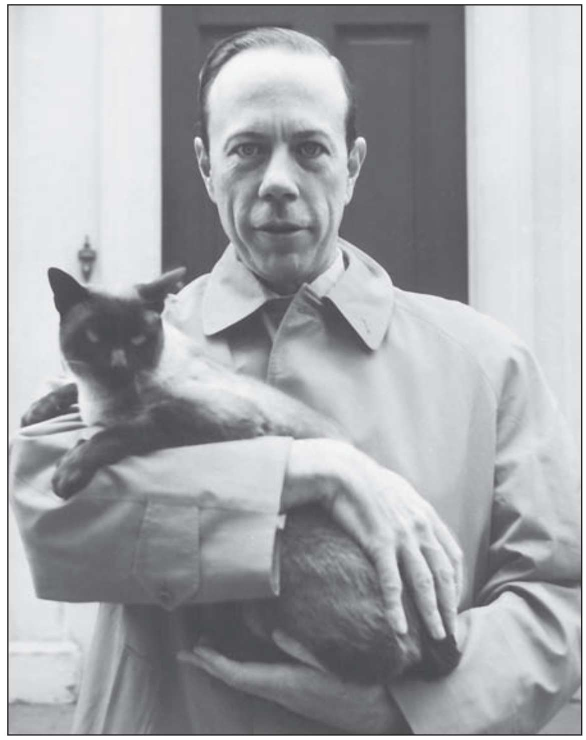


Gleason and Fred, Philip's successor in the Gleason household.

The star example of all this is given by the "disk algebra" A, consisting of all continuous functions on the unit circle Γ which admit analytic continuation to the open unit disk. We take $||f|| = max|f|$ taken over Γ for f in A. Here $\mathcal{M}$ can be identified with the closed unit disk Δ, and $\check{S}$ becomes the topological boundary of Δ. For f in A, $\hat{f}$ is the analytic continuation of f to the interior of Δ.

Another key example is provided by the bidisk algebra A_2 which consists of all functions continuous on the closed bidisk Δ_2 in $\mathbb{C}^2$ which are holomorphic on the interior of Δ_2. The maximal ideal space of $\mathcal{M}$ can be identified with Δ_2; the Silov boundary is not the topological boundary of Δ_2, but instead the torus $T^2 : |z| = 1, |w| = 1$.

Classical function theory gives us, in the case of the disk algebra, not only the maximum principle (1) but also the local maximum principle:
For every f in A, if z_0 lies in the open unit disk and U is a compact neighborhood of z_0 contained in intΔ then

$$|f(z_0)| \leq \max_{z \in \partial U} |f(z)|. \tag{2}$$

It is a fundamental fact, proved by Rossi in [3], that the analogue of (2) holds in general. We have

Theorem 1. (Local Maximum Modulus Principle) Fix a point m in $\mathcal{M} \backslash \check{S}$ and fix a compact neighborhood U of m in $\mathcal{M} \backslash \check{S}$. Then we have for each f in $\mathcal{A}$

$$|\check{f}(m)| \leq \max_{u \in \partial U} |\check{f}(u)|. \tag{3}$$

This result suggests that for an arbitrary $\mathcal{A}$, where $\mathcal{M}\backslash\check{S}$ is nonempty, we should look for some kind of analytic structure in $\mathcal{M}\backslash\check{S}$. In the 1950s Gleason set out to find such analytic structure. He focused on a class of Banach algebras he called "function algebras".

Let X be a compact Hausdorff space. The algebra $C(X)$ of all continuous complex-valued functions on X, with $||f|| = max|f|$ over X, is a Banach algebra. A closed subalgebra $\mathcal{A}$ of $C(X)$ which separates the points of X and contains the unit is called a "function algebra" on X. It inherits its norm from $C(X)$.

Let $\mathcal{M}$ be the maximal ideal space of $\mathcal{A}$. Then X is embedded in the compact space $\mathcal{M}$ as a closed subset, and each f in $\mathcal{A}$ has $\hat{f}$ as a continuous extension to $\mathcal{M}$.

Parts

Let $\mathcal{A}$ be a function algebra on the space X, with maximal ideal space $\mathcal{M}$. Fix a point m in $\mathcal{M}$. The map: $f \to \hat{f}(m)$ is a bounded linear functional on $\mathcal{A}$. We use this map to embed $\mathcal{M}$ into $\mathcal{A}^\star$, the dual space of $\mathcal{A}$. $\mathcal{M}$ then lies in the unit ball of $\mathcal{A}^\star$.

Hence, if m and m' are two points in $\mathcal{M}$, $||m - m'|| \leq 2$. Gleason [4] defined a relation on the points of $\mathcal{M}$ by writing : $m \bullet m'$ if $||m - m'|| < 2$. He proved:

Proposition. The relation "bullet" is an equivalence relation on $\mathcal{M}$.

Note: At first sight, this proposition is counterintuitive, since $m \bullet m'$ and $m' \bullet m''$ are equivalent to $||m - m'|| < 2$ and $||m' - m''|| < 2$. The triangle inequality for the norm yields $||m - m''|| < 4$, whereas we need $||m - m''|| < 2$.

For each $\mathcal{A}$ the space $\mathcal{M}$ splits into equivalence classes under $\bullet$. Gleason called these equivalence classes the "parts" of $\mathcal{M}$.

Observe what these parts look like when $\mathcal{A}$ is the bidisk algebra A_2. Here $\mathcal{M}$ is the closed unit bidisk $\Delta_2 : |z| \leq 1, |w| \leq 1$. Some calculation gives the following: the interior of Δ_2, $|z| < 1, |w| < 1$, is a single part. Each of the disks $(e^{it}, w)|0 \leq t \leq 2\pi, |w| < 1$, $(z, e^{is}), |z| < 1, 0 \leq s \leq 2\pi$ is a part of $\mathcal{M}$. Finally, each point (exp(it), exp(is)), s,t real, is a one-point part lying on the torus $|z| = 1, |w| = 1$. Thus $\mathcal{M}$ splits into the pieces: one analytic piece of complex dimension 2, two families of analytic pieces of complex dimension 1, and uncountably many one-point parts on the Silov boundary of the algebra.

In complete generality, Andy's hopes that for each function algebra the parts of $\mathcal{M}$ would provide analytic structure of the complement of the Silov boundary were not fully realized. Stolzenberg, in [6], gave an example of a function algebra $\mathcal{A}$ such that the complement of the Silov boundary of $\mathcal{A}$ in $\mathcal{M}$ is nonempty but contains no analytic structure. However, an important class of Banach algebras, the so-called "Dirichlet algebras", and their generalizations behaved as Andy had hoped. We turn to these algebras in the next section.

Dirichlet Algebras

Let X be a compact Hausdorff space and let $\mathcal{A}$ be a function algebra on X. In [4], Gleason made the following definition: $\mathcal{A}$ is a *Dirichlet* algebra on X if $Re(\mathcal{A})$, the space of real parts of the functions in $\mathcal{A}$, is uniformly dense in the space $C_R(X)$ of all real continuous functions on X.

The name "Dirichlet" was chosen by Gleason because in the case when $\mathcal{A}$ is the disk algebra A, this density condition is satisfied and has as a consequence the solvability of the Dirichlet problem for harmonic functions on the unit disk.

He stated, "It appears that this class of algebras is of considerable importance and is amenable to analysis." This opinion was born out by developments.

A typical Dirichlet algebra is the disk algebra A on the circle Γ. By looking at A we are led to the basic properties of arbitrary Dirichlet algebras. A has the following properties:

(i) For each point z in Δ, there exists a unique probability measure μ_z on Γ such that for all f in A

$f(z) = \int_{-\pi}^{\pi} f(exp(it))d\mu_z$,

(ii) $\mu_z = \frac{1}{2\pi} p_z dt$,

where p_z is the Poisson kernel at z unless $|z| = 1$, and then μ_z is the point mass at z.

He proved in [4]:

Theorem 2. *Let $\mathcal{A}$ be a Dirichlet algebra on the space X, and let $\mathcal{M}$ be its maximal ideal space.*

(i') Fix m in $\mathcal{M}$. There exists a unique probability measure μ_m on X such that

$$\hat{f}(m) = \int_X f d\mu_m \text{ , for all } f \text{ in } \mathcal{A}.$$

(ii') Fix points m and m' in $\mathcal{M}$. Then m and m' lie in the same part of $\mathcal{M}$ if and only if the measures μ_m and $\mu_{m'}$ are mutually absolutely continuous. In this case, the corresponding Radon-Nikodym derivative is bounded above and below on X.

Note: For m in $\mathcal{M}$, μ_m is called "the representing measure for m".

It turned out that when $\mathcal{A}$ is a Dirichlet algebra with maximal ideal space $\mathcal{M}$, then each part of $\mathcal{M}$ is either a single point or an analytic disk. Explicitly, it is proved in Wermer [7]:

Theorem 3. *Let $\mathcal{A}$ be a Dirichlet algebra with maximal ideal space $\mathcal{M}$. Let Π be a part of $\mathcal{M}$. Then either Π consists of a single point or there exists a continuous one-one map τ of the open unit disk onto Π such that for each f in $\mathcal{A}$ the composition $\hat{f} \circ \tau$ is holomorphic on the unit disk.*

Examples

The following are three examples of Dirichlet algebras.

Example 1: Let K be a compact set in the complex plane C with connected complement, and let X be the boundary of K. The uniform closure $P(X)$ of polynomials on X is a Dirichlet algebra on X.

Example 2: Fix $\alpha > 0$. A_α denotes the space of all continuous functions f on the torus T^2 consisting of all points $(e^{i\theta}, e^{i\phi})$ in C^2 such that f has the Fourier expansion on T^2:

$$\sum_{n+m\alpha\geq 0} c_{nm} e^{in\theta} e^{im\phi}$$

These algebras are studied by Helson and Lowdenslager in [9] and by Arens and Singer in [10]. Each A_α is a Dirichlet algebra on T^2.

Example 3: Let γ be an arc on the Riemann sphere S. Let $B(\gamma)$ denote the algebra of all continuous functions on γ which have a continuous extension to the full sphere S which is holomorphic on S outside of γ. For a certain class of arcs, studied by Browder and Wermer in [8], $B(\gamma)$ is a Dirichlet algebra on γ.

It turned out that substantial portions of the theory of Hardy spaces H^p on the unit disk have natural generalizations when the disk algebra is replaced by an arbitrary Dirichlet algebra.This was pointed out by Bochner in [11] in a slightly different context. It was carried out in [9] for Example 2, and in an abstract context by various authors. (See Gamelin [15].)

Further, Hoffman in [12] introduced a generalization of Dirichlet algebras, called "logmodular algebras", to which the theory of Dirichlet algebras has a natural extension. In particular, parts of the maximal ideal space of such an algebra are either points or disks.

Let H^∞ denote the algebra of all bounded analytic functions on the unit disk, with $||f|| = sup|f|$, taken over the unit disk. Then H^∞ is a Banach algebra. Let X denote the Silov boundary of this algebra. The restriction of H^∞ to X is a function algebra on X. This restriction is not a Dirichlet algebra on X, but it is a log-modular algebra on X. By what was said above, the parts of the maximal ideal space of $H^\infty$are points or analytic disks.

Let M be the maximal ideal space of H^∞, taken with the Gelfand topology. M is compact and contains the open unit disk D as a subset. Lennart Carleson proved in 1962 the so-called Corona Theorem, which implies that D is dense in M. The question had arisen earlier as to the (possible) analytic structure in the complement $M\backslash D$.

Partial results on this question were obtained in 1957 by a group of people talking at a conference, and this result was published under the pseudonym "I. J. Schark"[1] in the paper [16]. Hoffman and Gleason were prominent participants in this enterprise.

[1] *I. Kaplansky, J. Wermer, S. Kakutani, C. Buck, H. Royden, A. Gleason, R. Arens, K. Hoffman.*

Gleason's Problem

Let $\mathcal{A}$ be a function algebra and $\mathcal{M}$ be its maximal ideal space. Fix a point m_0 in $\mathcal{M}$. As a subset of $\mathcal{A}$, m_0 is the set of f such that $\hat{f}(m_0) = 0$. We ask: when does m_0 have a neighborhood in $\mathcal{M}$ which carries structure of a complex-analytic variety? By this we mean the following: there exists a polydisk Δ^n in $\mathbb{C}^n$ and an analytic variety V in Δ^n, and there exists a homeomorphism τ of a neighborhood $\mathcal{N}$ of m_0 on V such that for all f in $\mathcal{A}$ the composition of $\hat{f}$ with the inverse of τ has an analytic extension from V to Δ^n.

Gleason proved the following in [5]:

Theorem 4. *Let $\mathcal{A}$, $\mathcal{M}$, m_0 be as above. Assume that m_0, as an ideal in $\mathcal{A}$, is finitely generated (in the sense of algebra). Then there exists a neighborhood $\mathcal{N}$ of m_0 which has the structure of a complex-analytic variety.*

This result leads naturally to the following question, raised by Gleason:

Let D be a bounded domain in $\mathbb{C}^n$ and denote by $A(D)$ the algebra of continuous functions on the closure of D which are analytic on D. Fix a point $a = (a_1, \ldots, a_n)$ in D. Given f in $A(D)$ with $f(a) = 0$, do there exist functions $g_1, \ldots, g_n$ in $A(D)$ such that $f(z) = \sum_{j=1}^n (z_j - a_j) g_j(z)$ for every z in D?

It is now known that the answer is yes if D is a strictly pseudo-convex domain in $\mathbb{C}^n$. A history of the problem is given by Range in [14], Chapter VII, paragraph 4.

References

[1] I. M. Gelfand, Normierte ringe, *Mat. Sb. (N.S.)* **51** (1941), 3–24.

[2] G. E. Silov, On decomposition of a normed ring in a direct sum of ideals, *Mat. Sb (N.S.)* **32** (74) (1953), 353–364.

[3] H. Rossi, The local maximum modulus principle, *Ann. of Math.* (2) **72** (1960), 1–11.

[4] A. Gleason, *Function Algebras*, Seminar on Analytic Functions, Vol. II, Inst. Adv. Study, Princeton, 1957, pp. 213–226.

[5] ——, Finitely generated ideals in Banach algebras, *J. Math. Mech.* **13** (1964), 125–132.

[6] G. Stolzenberg, A hull with no analytic structure, *J. Math. Mech.* **12** (1963), 103–112.

[7] J. Wermer, Dirichlet algebras, *Duke Math. J.* **27** (1960), 373–382.

[8] A. Browder and J. Wermer, Some algebras of functions on an arc, *J. Math. Mech.* **12** (1963), 119–130.

[9] H. Helson and D. Lowdenslager, Prediction theory and Fourier series in several variables, *Acta Math.* **99** (1958), 165–202.

[10] R. Arens and I. M. Singer, Generalized analytic functions, *Trans. Amer. Math. Soc.* **81** (1956), 379–393.

[11] S. Bochner, Generalized conjugate and analytic functions without expansions, *Proc. Nat. Acad. Sci. U.S.A.* **45** (1959), 855–857.

[12] K. HOFFMAN, Analytic and logmodular Banach algebras, *Acta Math.* **108** (1962), 271–317.
[13] _____ , Bounded analytic functions and Gleason parts, *Ann. Math. (2)* **86** (1967), 74–111.
[14] M. RANGE, *Holomorphic Functions and Integral Representations in Several Complex Variables*, Springer-Verlag, 1986.
[15] T. W. GAMELIN, *Uniform Algebras*, Prentice Hall, 1969.
[16] I. J. SCHARK, Maximal ideals in an algebra of bounded analytic functions, *J. Math. Mech.* **10** (1961), 735–746.

Joel Spencer

Andrew Gleason's Discrete Mathematics

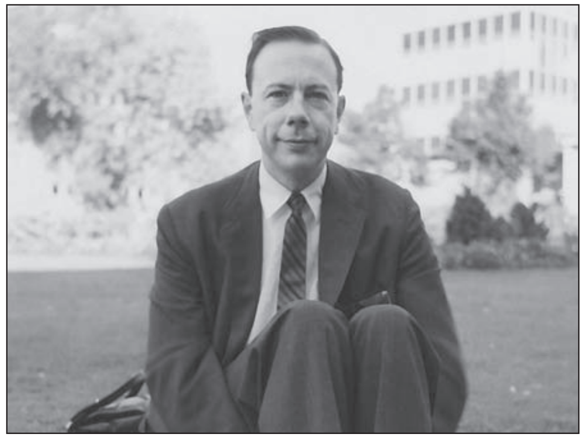
Ramsey Theory

> Six points are in general position in space (no three in a line, no four in a plane). The fifteen line segments joining them in pairs are drawn, and then painted, some segments red, some blue. Prove that some triangle has all its sides the same color.
>
> —*William Lowell Putnam Competition, 1953 ([3], page 38)*

Andrew Gleason's fascination with combinatorial puzzles, his computational skills, and his algebraic insights often led to interesting deep results in discrete mathematics. We sample some of them here.

The Putnam problem quoted above introduces *Ramsey theory*, where Gleason made one of his first contributions. Ramsey theory starts with the fact that for any $k_1, \ldots, k_r$ there is a least $n = R(k_1, \ldots, k_r)$ such that when each of the $\binom{n}{2}$ line segments joining n points in pairs is painted with one of r colors, then for some $1 \le i \le r$ there are k_i points with all segments between them given color i. R is generally called the Ramsey function and the $n = R(k_1, \ldots, k_r)$ are called Ramsey numbers. Solving the Putnam problem above proves $R(3,3) \le 6$. The arguments for the existence of $R(k,l)$ had been given by Ramsey and, independently, by Erdős and Szekeres in the early 1930s (see [4] for general reference). Ramsey theory fascinated Gleason.

In 1955 Gleason and coauthor R. E. Greenwood [1] calculated some small Ramsey numbers. They found $R(3,3) = 6$, $R(3,4) = 9$, $R(3,5) = 14$, $R(4,4) = 18$, and $R(3,3,3) = 17$. The lower bounds sometimes called for ingenious algebraic constructions to provide counterexamples. For instance, to show $R(3,3,3) > 16$ they consider the 16 points



Gleason in Berlin, 1959.

as $GF(16)$. Let $H \subset GF(16)^*$ consist of the nonzero cubes. Then they color the edge between $\alpha, \beta \in GF(16)$ by the coset of $GF(16)^*/H$ containing $\alpha - \beta$. Despite great efforts and high speed computers, only a few other values are known today. Even the value of $R(5,5)$ seems out of reach.

As a graduate student at Harvard in the late 1960s I chose to write a thesis partially on Ramsey numbers. Gleason told me he had spent a great deal of time looking for other exact values. Since I knew of his legendary calculating powers, I took this as sage advice to restrict my attention to their asymptotics.

Coding Theory

Gleason's research in discrete mathematics began not with Ramsey theory but with his cryptographic work during World War II [5]. That's when he first collaborated with Greenwood. After the war he participated in the burgeoning development of coding theory. Although he published little, he had a significant influence on others.

Vera Pless (her [6] is a good general reference for coding theory) recalls "Gleason meetings" in the 1950s on error-correcting codes.

> These monthly meetings were what I lived for. No matter what questions we asked him on any area of mathematics, Andy knew the answer. The numerical calculations he did in his head were amazing.

A *binary code* is a subset of $\{0,1\}^n$. The elements are called codewords. The *weight* of a codeword is the number of coordinates with value one. Gleason studied codes with an algebraic representation.

To define a *quadratic residue code*, begin by identifying $\{0,1\}^n$ with $Z_2[x]/(x^n - 1)$. Suppose n is a prime congruent to ± 1 mod 8. Let Q be the quadratic residues of Z_n^* and set $e(x) = \sum_{i \in Q} x^i$. Then let $C = (1 + e(x))$, the ideal generated by $1 + e(x)$ in $Z_2[x]/(x^n - 1)$. Then C is a code of

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dimension $(n+1)/2$. These codes have proven particularly useful, in part because of their symmetries.

Let $\overline{C}$ be the code of dimension $n+1$ given by adding a parity bit. (That is, the first n bits are in C, and the last is such that the weight is even.) A symmetry of $\overline{C}$ is a permutation $\sigma \in S_{n+1}$ of the coordinates for which $\sigma(\overline{C}) = \overline{C}$. The *Gleason-Prange Theorem* [6] asserts that

Theorem 1. *The Projective Simple Linear Group $PSL_2(n)$ is a subgroup of the group of symmetries of $\overline{C}$.*

A *linear code* is a $C \subseteq \{0,1\}^n$ which is a subspace of $\{0,1\}^n$. For such C, $C^\perp$ is the usual (over Z_2) orthogonal subspace. When C has A_i vectors of weight i, its weight enumerator is defined by

$$W_C(x,y) = \sum_{i=0}^{n} A_i x^{n-i} y^i.$$

The *Gleason polynomials* are finite sets of polynomials that generate all weight enumerators of a certain type.

Gleason found a particularly striking example for self-dual codes, those for which $C = C^\perp$.

Theorem 2. *If C is self-dual, then W_C is generated by $g_1(x,y) = x^2 + y^2$ and $g_2(x,y) = x^8 + 14x^2y^2 + y^8$.*

There are deep connections to invariant theory here.

The weight enumerator of C determines that of $C^\perp$. The exact relationship was given by Jessie MacWilliams (1917–1990), one of Gleason's most accomplished students, in her thesis.

Theorem 3. *The MacWilliams Identity:*

$$W_{C^\perp}(x,y) = \frac{1}{|C|} W_C(x+y, x-y).$$

Gleason proved much more along these lines in [2]. Neil Sloane starts his paper on *Gleason's Theorem on Self-Dual Codes and Its Generalizations* [8] with "One of the most remarkable theorems in coding theory is Gleason's 1970 theorem about the weight enumerators of self-dual codes."

The Putnam Exam

Gleason was the first three-time winner of the Putnam competition, finishing in the top five while at Yale in 1940, 1941, and 1942. He was disappointed in his first attempt, because he solved only thirteen of the fifteen problems.

For many years he selected one of the five finishers for the Putnam Fellowship at Harvard, a fellowship he was awarded and declined in 1940 in order to remain at Yale. He wrote (with R. E. Greenwood and L. M. Kelly) a beautiful book [3] on the Putnam competition for the years 1938–64. For many problems his solutions (and there are often several) are splendid lectures in the varied subjects. Elwyn Berlekamp (also a Putnam winner) recalls discussions with him:

> [Gleason] would usually offer two or three different solutions to the problem he wanted to talk about, whereas I rarely ever had more than one. He believed that Putnam problems encouraged very strong mastery of what he considered to be the fundamentals of mathematics.

Gleason was always eager to share his passion for mathematics in general and problems in particular. Bjorn Poonen (a multiple Putnam winner and the coauthor of a follow-up book on the Putnam competition 1985–2000 [7]) recalls his undergraduate days:

> Andy struck me as someone genuinely interested in helping younger mathematicians develop. When I was an undergrad at Harvard, he volunteered an hour or two of his time each week for an informal meeting in his office with a group consisting of me and one or two other math undergrads to discuss whatever mathematics was on our minds.

Amen. As a graduate student I had the good fortune to be Andy Gleason's teaching assistant. We would meet in the small preparation room before the classes. Andy would discuss the mathematics of the lecture he was about to give. He was at ease and spoke of the importance and the interrelationships of the various theorems and proofs. My contributions were minimal, but I listened with rapt attention. It was in those moments that I learned what being a mathematician was all about.

References

[1] A. M. Gleason and Robert E. Greenwood Jr., Combinatorial relations and chromatic graphs, *Canadian J. Math* **7** (1955), 1–7.

[2] A. M. Gleason, Weight polynomials of self-dual codes and the MacWilliams identities, in *Proceedings of the International Congress of Mathematicians*, volume 3, Nice, 1970, pp. 211–215.

[3] A. M. Gleason, R. E. Greenwood, and L. M. Kelly, *The William Lowell Putnam Mathematical Competition, Problems and Solutions: 1938–1964*, The Mathematical Association of America, 1980.

[4] R. L. Graham, B. L. Rothschild, and J. H. Spencer, *Ramsey Theory*, 2nd edition, Wiley, 1990.

[5] D. Lieberman (with J. Burroughs and J. Reed) [see page 1239 of this article].

[6] V. Pless, *Introduction to the Theory of Error-Correcting Codes*, 2nd edition, Wiley, 1989.

[7] K. Kedlaya, B. Poonen, and R. Vakil, *The William Lowell Putnam Mathematical Competition 1985–2000: Problems, Solutions, and Commentary*, MAA Problem Book Series, The Mathematical Association of America, second edition, September 2002.

[8] N. J. A. Sloane, *Gleason's Theorem on Self-Dual Codes and Its Generalizations*, http://arxiv.org/abs/math/0612535.

Paul R. Chernoff

Andy Gleason and Quantum Mechanics

About Andy

I met Andy at the beginning of my second year at Harvard when I signed up for his graduate analysis course. Andy briefly interviewed prospective students to see if they had enough background to benefit from this rather sophisticated course. I told Andy that I owned a number of advanced books which I hadn't read.

The course was both a challenge and a pleasure. I can only echo what others have said about Andy's luminous clarity and massive abstract power. But I must admit that the lectures, always exciting, weren't *absolutely* perfect; in the course of a year Andy made one genuine blunder. As to his famous speed, John Schwarz, the well-known string theorist, once said after class that Andy had "the metabolism of a hummingbird".

I was extremely lucky that Andy was affiliated with Lowell House, my undergraduate residence. Every week Andy came for lunch, where we sat around a large circular table. That's how Andy and I became friends. Of course we discussed a lot of mathematics around that table, but lots of other things, including Andy's "war stories". I am not surprised that someone kept a great treasure: all of Andy's napkin manuscripts.

Almost any mathematical problem could intrigue Andy. At one of the annual math department picnics, he had fun figuring out how to do cube roots on an abacus. But most important was his unpretentiousness, openness, and great interest in students. I suppose that all teachers are impatient at times; no doubt Andy was sorely tried on occasion. But he rarely, if ever, showed it. The students in one of his classes gave him a framed copy of Picasso's early painting *Mother and Child*. Perhaps they chose this gift to symbolize Andy's nurturing of them. It's regrettable that there are some teachers for whom *Guernica* would be more appropriate.

Quantum Mechanics

In this section we set the stage for a discussion of Andy's unique contribution to physics: his remarkable paper "Measures on the closed subspaces of a Hilbert space" [G57]. It's interesting in several ways: its history; its influence in mathematics; and especially its unexpected importance to the analysis of "hidden variable" theories of quantum mechanics by the physicist John Bell.

Gleason with koala, Cleland Wildlife Park, Adelaide, Australia, 1988.

In classical mechanics, the *state* of a particle of mass m is given by its position and momentum. The motion or dynamics of a set of particles with associated forces is determined by Newton's second law of motion, a system of ordinary differential equations. This yields a picture of the macroscopic world which matches our intuition. The ultramicroscopic world requires a quite different description. The state of a particle of mass m in $\mathbb{R}^3$ is a complex valued function ψ on $\mathbb{R}^3$. Its momentum is similarly described by the function ϕ, the *Fourier transform* of ψ, normalized by the presence in the exponent of the ratio $\frac{h}{m}$, where h is Planck's constant. (Using standard properties of the Fourier transform, one can deduce Heisenberg's uncertainty principle.) For n particles, ψ is defined on $\mathbb{R}^{3n}$. This is a brilliant extrapolation of the initial ideas of DeBroglie. The Schrödinger equation determines the dynamics. If both ψ and ϕ are largely concentrated around n points in position *and* momentum space respectively, then the quantum state resembles a blurry picture of the classical state. The more massive the particles, the less the blurriness (protons versus baseballs).

The fundamental interpretation of the "wave function" ψ is the work of Max Born.[1] His paper analyzing collisions of particles ends with the conclusion that $|\psi|^2$ should be interpreted as the probability distribution for the positions of the particles. Therefore the wave function must be a unit vector in L^2. Thus did Hilbert space enter quantum mechanics.

Prior to Schrödinger's wave mechanics, Heisenberg had begun to develop a theory in which observable quantities are represented by Hermitian-symmetric infinite square arrays. He devised a "peculiar" law for multiplying two arrays by an ingenious use of the physical meaning of their entries. Born had learned matrix theory when he was a student and realized (after a week of "agony") that Heisenberg's recipe was just matrix multiplication. Hence the Heisenberg theory is called matrix mechanics. (Schrödinger showed that

[1] *For more historical information, including translations of important original papers, see van der Waerden's excellent survey of the development of quantum mechanics [VW]. Jammer's book [J66] is a superb account of the development of quantum mechanics.*

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matrix mechanics and wave mechanics are mathematically equivalent.) As in classical mechanics, the dynamics of a quantum system is determined from its energy H. Periodic orbits correspond to the eigenvalues of H, i.e., the discrete energy levels. The calculation of the eigenvalues is very difficult, save for a few simple systems. The energy levels for the hydrogen atom were ingeniously calculated by Wolfgang Pauli; his results agreed with Bohr's calculations done at the very beginning of the "old" quantum theory.

Born was quite familiar with Hilbert's theory of integral equations in L^2. Accordingly, he was able to interpret Heisenberg's matrices as Hermitian symmetric kernels with respect to some orthonormal basis, which might just as well be regarded as the corresponding integral operators on L^2. Formally, every Hermitian matrix could be regarded as an integral operator, usually with a very singular kernel. (The most familiar example is the identity, with kernel the Dirac delta function.) In this way, Born initiated the standard picture of observables as Hermitian operators A on L^2. But at that time, the physicists did not grasp the important distinction between unbounded Hermitian operators and unbounded self-adjoint operators. That was greatly clarified by John von Neumann, major developer of the theory of unbounded self-adjoint operators.

Having interpreted $|\psi|^2$ as the probability distribution for the positions of particles, Born went on to devise what immediately became the standard interpretation of measurements in quantum mechanics: the probability that a measurement of a quantum system will yield a particular result.

Born's line of thought was this. A state of a quantum system corresponds to a unit vector $\psi \in L^2$. What are the possible values of a measurement of the observable represented by the operator A, and what is the probability that a specific value is observed? Born dealt only with operators with a discrete spectrum, namely, the set of all its eigenvalues. For simplicity, assume that there are no multiple eigenvalues. Let ϕ_n be the unit eigenvector with eigenvalue λ_n. These form an orthonormal basis of L^2. Expand ψ as a series $\sum_k c_k \phi_k$. Since $||\psi||^2 = 1$, we get $\sum_k |c_k|^2 = 1$. Born's insight was that any measurement must yield one of the eigenvalues λ_n of A, and $|c_n|^2$ is the probability that the result of the measurement is λ_n . This is known as *Born's rule*. It follows that the expected value of a measurement of A is $\sum_k |c_k|^2 \lambda_k$. Note that this sum equals the inner product $(A\psi, \psi)$. This is the same as $\mathrm{trace}(PA)$, where P is the projection onto the one-dimensional subspace spanned by ψ. (To jump ahead, George Mackey wondered if Born's rule might involve some arbitrary choices. Gleason ruled this out.)

John von Neumann was the creator of the abstract theory of quantum mechanics. In his theory, a *pure state* is a unit vector in a Hilbert space $\mathcal{H}$. Observables are self-adjoint operators, unbounded in general, whose spectrum may be any Borel subset of $\mathbb{R}$. Von Neumann also developed the important concept of a *mixed state*. A mixed state **D** describes a situation in which there is not enough information to determine the pure state ψ of the system. Usually physicists write **D** as a convex combination of orthogonal pure states, $\sum_k w_k \psi_k$. This notation is confusing; **D** is *not* a vector in $\mathcal{H}$! It may be interpreted as a list of probabilities w_k that the corresponding pure state is ψ_k. Associated with the state **D** there is a positive operator D with trace 1, given by the formula

$$D = \textstyle\sum_k w_k P_k$$

where P_n is the projection on the eigenspace of D corresponding to the eigenvalue w_n. The expected value of an observable A is quite clearly

$$E(A) = \textstyle\sum_k w_k (A\psi_k, \psi_k) = \mathrm{trace}(DA).$$

This is von Neumann's general Born rule.

The eigenvalues of a projection operator are 1 and 0; those are the only values a measurement of the corresponding observable can yield. That is why Mackey calls a projection a *question*; the answer is always either 1 or 0: "yes" or "no". The fundamental example is the following. Given a self-adjoint operator A, we will apply the spectral theorem. Let S be any Borel subset of $\mathbb{R}$ and let P_S be the corresponding "spectral projection" of A. (If the set S contains only some eigenvalues of A, then P_S is simply projection onto the subspace spanned by the corresponding eigenvectors.) Now suppose the state of the system is the mixed state **D**. From the general Born rule, the probability that a measurement of A lies in S is the expected value of P_S, namely, $\mathrm{trace}(DP_S)$. That is the obvious generalization of Born's formula for the probability that a measurement of A is a particular eigenvalue of A or a set of isolated eigenvalues.

Quite generally, consider a positive operator D with $\mathrm{trace}(D) = 1$. The nonnegative real-valued function $\mu(P) = \mathrm{trace}(DP)$ is a countably additive probability measure on the lattice of projections on $\mathcal{H}$. This means that if $\{P_n\}$ is a countable family of mutually orthogonal projections,

$$\mu(\sum_n P_n) = \sum_n \mu(P_n).$$

Also $\mu(I) = 1$. Mackey asked whether *every* such measure on the projections is of this form, i.e., corresponds to a state D. We already mentioned Mackey's interest in Born's rule. A positive answer to Mackey's question would show that the Born rule follows from his rather simple axioms for quantum mechanics [M57], [M63], and thus, given these weak postulates, Born's rule is not ad hoc but inevitable.

Gleason's Theorem

Mackey didn't try very hard to solve his problem for the excellent reason that he had no idea how to attack it. But he discussed it with a number of experts, including Irving Segal, who mentioned Mackey's problem in a graduate class at Chicago around 1949 or 1950. Among the students was Dick Kadison, who realized that there are counterexamples when $\mathcal{H}$ is two-dimensional. The higher-dimensional case remained open.

There matters stood for some years. Then Gleason entered the story. In 1956 he sat in on Mackey's graduate course on quantum mechanics at Harvard. To Mackey's surprise, Andy was seized by the problem "with intense ferocity". Moreover, Kadison was visiting MIT at the time, and his interest in Mackey's problem was rekindled. He quickly perceived that there were many "forced inter-relations" entailed by the intertwining of the great circles on the sphere and in principle a lot could be deduced from an analysis of these relations, though the problem still looked quite tough. He mentioned his observation to Andy, who found it a useful hint. (But Kadison informed me that his observation did not involve anything like Andy's key "frame function" idea.)

Theorem 1 *(Gleason's theorem). Let $\mathcal{H}$ be a separable Hilbert space of dimension greater than 2. Let μ be a countably additive probability measure on the projections of $\mathcal{H}$. Then there is a unique nonnegative self-adjoint operator D, with trace(D) = 1, such that, for every projection P,*

$$\mu(P) = \mathrm{trace}(DP).$$

The proof has three parts. First, using countable additivity and induction, it is easy to reduce the case of any separable real Hilbert space of dimension greater than 2 to the 3-dimensional case. (The complex case follows from the real case.)

Next, consider a vector x on the unit sphere. Let P_x be the one-dimensional subspace containing x, and define $f(x) = \mu(P_x)$. This function is called a *frame function*. The additivity of the measure μ implies that for any three mutually orthogonal unit vectors,

$$f(x) + f(y) + f(z) = 1.$$

The proof comes down to showing that the frame function f is quadratic and therefore is of the form $f(x) = \mathrm{trace}(DP_x)$, where D is as in the statement of the theorem. Gleason begins his analysis by showing that a *continuous* frame function is quadratic via a nice piece of harmonic analysis on the sphere. The centerpiece of the paper is the proof that f is continuous. Andy told me that this took him most of the summer. It demonstrates his powerful geometric insight. However, despite Andy's talent for exposition, much effort is needed to really understand his argument.

Quite a few people have worked on simplifying the proof. The paper by Cooke, Keane, and Moran [CKM] is interesting, well written, and leads the reader up a gentle slope to Gleason's theorem. The authors use an important idea of Piron [Pi]. (The CKM argument is "elementary" because it does not use harmonic analysis.)

Generalizations of Gleason's Theorem

In his paper Andy asked if there were analogues of his theorem for countably additive probability measures on the projections of von Neumann algebras other than the algebra of bounded operators on separable Hilbert spaces.

A von Neumann algebra, or W^* algebra, is an algebra $\mathcal{A}$ of bounded operators on a Hilbert space $\mathcal{H}$, closed with respect to the adjoint operation. Most importantly, $\mathcal{A}$ is closed in the *weak operator topology*. The latter is defined as follows: a net of bounded operators $\{a_i\}$ converges *weakly* to b provided that, for all vectors $x, y \in \mathcal{H}$,

$$\lim_{n\to\infty}(a_n x, y) = (bx, y).$$

A *state* of a von Neumann algebra $\mathcal{A}$ is a positive linear functional $\phi : \mathcal{A} \to \mathbb{C}$ with $\phi(I) = 1$. This means that $\phi(x) \geq 0$ if $x \geq 0$ and also $||\phi|| = 1$. The state ϕ is *normal* provided that if a_i is an increasing net of operators that converges weakly to a, then $\phi(a_i)$ converges to $\phi(a)$. The normal states on $B(\mathcal{H})$ are precisely those of the form $\mathrm{trace}(Dx)$, where D is a positive operator with trace 1.

Let $P(\mathcal{A})$ be the lattice of orthogonal projections in $\mathcal{A}$. Then the formula

$$\mu(P) = \phi(P)$$

defines a finitely additive probability measure on $P(\mathcal{A})$. If ϕ is normal, the measure μ is countably additive.

The converse for countably additive measures is due to A. Paszkiewicz [P]. See E. Christensen [C] and F. J. Yeadon [Y1], [Y2] for finitely additive measures. Maeda has a careful, thorough presentation of the latter in [M].

It is not surprising that the arguments use the finite-dimensional case of Gleason's theorem. A truly easy consequence of Gleason's theorem is that μ is a uniformly continuous function on the lattice of projections P, equipped with the operator norm.

A great deal of work has been done on Gleason measures which are unbounded or complex valued. A good reference is [D]. Bunce and Wright [BW] have studied Gleason measures defined on the lattice of projections of a von Neumann algebra with values in a Banach space. They prove the analogue of the results above. A simple example is Paszkiewicz's theorem for complex-valued measures, which had been established only for positive real-valued measures.

Nonseparable Hilbert Spaces

Gleason's theorem is true only for separable Hilbert spaces. Robert Solovay has completely analyzed the nonseparable case. (Unpublished. However, [SO] is an extended abstract.) I consider Solovay's work to be the most original extension of Gleason's theorem.

Definitions. *A* Gleason measure *on a Hilbert space $\mathcal{H}$ is a countably additive probability measure on the lattice of projections of $\mathcal{H}$. We say that a Gleason measure μ is standard provided there is a positive trace-class operator D with trace 1 such that $\mu(P)$ =trace(DP). Otherwise, μ is* exotic.

Definition. *A set X is gigantic if there is a continuous probability measure ρ defined on* ***all*** *the subsets of X. Continuity means that every point has measure 0.*

A countable set is not gigantic. Indeed, gigantic sets are very, very large. Also, in standard set theoretic terminology, a gigantic cardinal is called a measurable cardinal.

Gleason's theorem states that every Gleason measure on a separable Hilbert space is standard. But suppose $\mathcal{H}$ is a nonseparable Hilbert space with a gigantic orthonormal basis $\{e_i : i \in I\}$. Let ρ be the associated measure on I. Then the formula

$$\mu(P) = \int_I (Pe_i, e_i)\, d\rho(i)$$

defines an exotic Gleason measure, because $\mu(Q) = 0$ for every projection Q with finite-dimensional range.

On the other hand, it can be shown that if $\mathcal{H}$ is any Hilbert space of nongigantic dimension greater than 2, then every Gleason measure on $\mathcal{H}$ is standard. Solovay presents a proof. (A consequence is that an exotic Gleason measure exists if and only if a measurable cardinal exists.)

If I is any set, gigantic or not, and ρ is any probability measure, continuous or not, defined on all the subsets of I, then the formula above defines a Gleason measure. Solovay's main theorem says that every Gleason measure is of this form.

Theorem 2 (Solovay). *Let $\mathcal{H}$ be a nonseparable Hilbert space, and let μ be a Gleason measure on $\mathcal{H}$. Then there is an orthonormal basis $\{e_i : i \in I\}$ of $\mathcal{H}$ and a probability measure ρ on the subsets of I such that μ is given by the formula above.*

Observe that Gleason's theorem is analogous; ρ is a discrete probability measure on the integers; the numbers $\rho(n)$ are the eigenvalues, repeated according to multiplicity, of the operator D.

Solovay also proves a beautiful formula giving a canonical representation of a Gleason measure μ as an integral over the set $\mathcal{T}$ of positive trace-class operators A of trace 1: there is a measure ν defined on all subsets of $\mathcal{T}$ such that, for all P,

$$\mu(P) = \int_{\mathcal{T}} \text{trace } (AP)\, d\nu(A)$$

Moreover, there is a *unique* "pure, separated" measure ν such that the formula above holds. These two technical terms mean that ν is similar to the sort of measure that occurs in spectral multiplicity theory for self-adjoint operators. The reader may enjoy proving this formula when $\mathcal{H}$ is finite-dimensional; this simple case sheds some light on the general case.

Hidden Variables and the Work of John Bell

The major scientific impact of Gleason's theorem is not in mathematics but in physics, where it has played an important role in the analysis of the basis of quantum mechanics. A major question is whether probabilistic quantum mechanics can be understood as a phenomenological theory obtained by averaging over variables from a deeper nonprobabilistic theory. The theory of heat exemplifies what is wanted. Heat is now understood as due to the collisions of atoms and molecules. In this way one can understand thermodynamics as a phenomenological theory derived by averages over "hidden variables" associated with the deeper particle theory; hence the term "statistical mechanics". Einstein sought an analogous relation between quantum mechanics and—what? He is supposed to have said that he had given one hundred times more thought to quantum theory than to relativity.

The fourth chapter of John von Neumann's great book [VN] is devoted to his famous analysis of the hidden variable question. His conclusion was that no such theory could exist. He writes, "The present system of quantum mechanics would have to be objectively false, in order that another description of the elementary processes than the statistical one may be possible." That seemed to settle the question. Most physicists weren't much interested in the first place when exciting new discoveries were almost showering down.

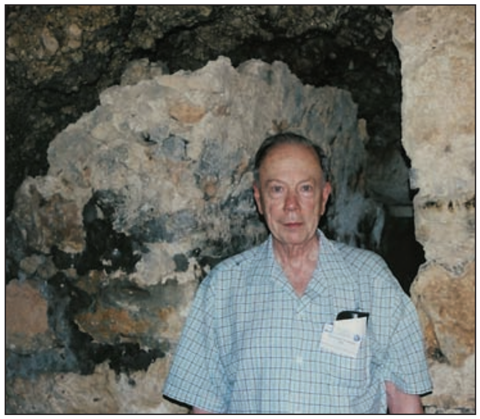
But in 1952 there was a surprise. Contrary to von Neumann, David Bohm exhibited a hidden variable theory by constructing a system of equations with both waves and particles which exactly reproduced quantum mechanics. But Einstein rejected this theory as "too easy", because it lacked the insight Einstein was seeking. Worse yet, it had the feature Einstein most disliked. Einstein had no problem understanding that there can easily be correlations between the behavior of two distant systems, A and B. If there is a correlation due to interaction when the systems are close, it can certainly be maintained when they fly apart. His objection to standard quantum mechanics was that in some cases a measurement of system A *instantly* determines the result of a related measurement of system B. Einstein dubbed this "weird action at a distance." Bohm's model has this objectionable property.

In fact, soon after its publication, von Neumann's argument was demolished by Grete Hermann [J74], a young student of Emmy Noether. Her point was that in quantum mechanics the expectation of the sum of two observables A and B is the sum of the expectations: $E(A+B) = E(A) + E(B)$, even if A and B don't commute. This is a "miracle" because the eigenvalues of $A+B$ have no relation to those of A and B unless A and B commute. It is true only because of the special formula for expectations in quantum mechanics. It is not a "law of thought". Yet von Neumann postulated that additivity of expected values must hold for all underlying hidden variable theories. That is the fatal mistake in von Neumann's argument. However, although Heisenberg immediately understood Hermann's argument when she spoke with him, her work was published in an obscure journal and was forgotten for decades.

The outstanding Irish physicist John Bell was extremely interested in the hidden variable problem. Early on he discovered a simple example of a hidden variable theory for a two-dimensional quantum system; it's in chapter 1 of [B], which is a reprint of [B66]. This is another counterexample for von Neumann's "impossibility" theorem. (Bell did a great deal of important "respectable" physics. He said that he studied the philosophy of physics only on Saturdays. An interesting essay on Bell is in Bernstein's book [BE].)

When Bell learned of Gleason's theorem he perceived that in Hilbert spaces of dimension greater than 2, it "apparently" establishes von Neumann's "no hidden variables" result *without* the objectionable assumptions about noncommuting operators. Bell is reported to have said that he must either find an "intelligible" proof of Gleason's theorem or else quit the field. Fortunately Bell did devise a straightforward proof of a very special case: nonexistence of frame functions taking only the values 0 and 1. Such frame functions correspond to projections. This case sufficed for Bell's purposes [B66]. See the first chapter of [B].[2]

[2] *Kochen and Specker [KS] proved a deeper theorem. But Si Kochen informed me that they didn't know of Gleason's theorem until they had almost completed their work.*



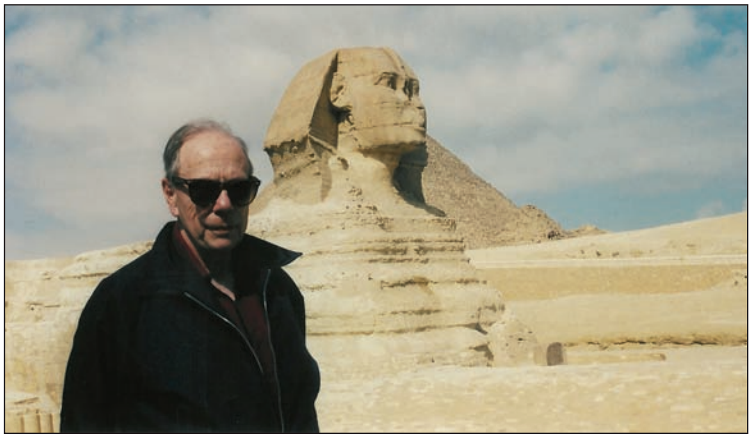
At the International Conference on the Teaching of Mathematics, Samos (home of Pythagoras), 1998.

The gist of von Neumann's proof is an argument that dispersion-free states do not exist. Here a state **D** is *dispersion-free* provided $E(A^2) = E(A)^2$ for any observable A. In other words, every observation of A has the value $E(A)$, its mean value. Quantum mechanics is supposedly obtained by averaging over such states. The frame functions considered by Bell correspond precisely to dispersion-free states. But these frame functions are not continuous. Gleason's theorem implies that no such frame functions exist. Therefore there are no dispersion-free states. But Gleason's theorem uses Mackey's postulate of additivity of expectations for *commuting* projections. Bell's argument based on Gleason's theorem avoids the unjustified assumption of additivity of expectation values for noncommuting operators.

Bell writes: "That so much follows from such apparently innocent assumptions leads one to question their innocence." He points out that if P, Q, and R are projections with P and Q orthogonal to R but not to each other, we might be able to measure R and P, or R and Q, but not necessarily both, because P and Q do not commute. Concretely, the two sets of measurements may well require different experimental arrangements. (This point was often made by Niels Bohr.) Bell expresses this fundamental fact emphatically: "The danger in fact was not in the explicit but in the implicit assumptions. It was tacitly assumed that measurement of an observable must yield the same value independently of what other measurements are made simultaneously." In other words, the measurement may depend on its context. This amounts to saying that Gleason's frame functions may not be well defined from the point of view of actual experiments. Accordingly, one should examine Mackey's apparently plausible derivation that projection-valued measures truly provide part of a valid axiomatization of quantum mechanics.

Finally, a few words about the famous "Bell's Inequality".

The second chapter of Bell's book is a reprint of [B64] (actually written after [B66]). In this very important paper, Bell derives a specific inequality satisfied by certain "local" hidden variable theory for *nonrelativistic* quantum mechanics. ("Locality" excludes "weird" correlations of measurements of widely separated systems.) There are many similar but more general inequalities. Moreover, the study of the "entanglement" of separated quantum



Gleason in Egypt in 2001.

systems has opened a new field of mathematical research.

Starting in 1969, difficult experimental work began, using variants of Bell's inequality, to test if very delicate predictions of quantum mechanics are correct. Of course, quantum mechanics has given superb explanations of all sorts of phenomena, but these experiments waterboard quantum mechanics. Many experiments have been done; so far there is no convincing evidence that quantum mechanics is incorrect. In addition, experiments have been done which suggest that influence from one system to the other propagates enormously faster than light. These experiments point toward instantaneous transfer of information.

Bell's papers on quantum philosophy have been collected in his book *Speakable and Unspeakable in Quantum Mechanics* [B]. The first paper [B66] discusses Gleason's theorem and the second "Bell's inequality". The entire book is a pleasure to read.[3]

Anagrams

Among his many talents, Andy was a master of anagrams. His fragmentary 1947 diary records a family visit during Harvard's spring break:

> March 30. …We played anagrams after supper and I won largely through the charity of the opposition.
>
> April 1. …Played a game of anagrams with Mother and won.
>
> April 2. …Mother beat me tonight at anagrams.

So we know a little about where he honed that talent.

Many years ago Andy and I had a little anagram "contest" by mail. (Dick Kadison said then, "You're having an anagram competition with Andy Gleason? That's like arm wrestling with Gargantua.") Anyhow, I figured out ROAST MULES, and I was proud to come up with I AM A WONDER AT TANGLES, which is an anagram of ANDREW MATTAI GLEASON. Unfortunately, it should be MATTEI. But I didn't have the chutzpah to ask Andy to change the spelling of his middle name.

I am grateful for very interesting correspondence and conversations with the late Andy Gleason and George Mackey, together with Dick Kadison, Si Kochen, and Bob Solovay.

[3]The Amer. Math. Monthly *published a nice elementary mathematical exposition of Bell's inequality [McA].*

References

[B] John S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, Cambridge University Press, 1987; 2004 edition with introduction by Alain Aspect and two additional papers.

[B64] J. S. Bell, On the Einstein-Podolsky-Rosen paradox, *Physics* **1** (1964), 195–200.

[B66] ______, On the problem of hidden variables in quantum mechanics, *Rev. Mod. Phys.* **38** (1966), 447–452.

[BE] J. Bernstein, *Quantum Profiles*, Princeton Univ. Press, 1991.

[BW] L. J. Bunce and J. D. Maitland Wright, The Mackey-Gleason problem, *Bull. Amer. Math. Soc. (New Series)* **26** (1992), 288–293.

[C] E. Christensen, Measures on projections and physical states, *Comm. Math. Phys.* **86** (1982), 529–538.

[CKM] R. Cooke, M. Keane, and W. Moran, An elementary proof of Gleason's theorem, *Math. Proc. Camb. Phil. Soc.* **99** (1985), 117–128.

[D] A. Dvurecenskij, *Gleason's Theorem and Its Applications*, Kluwer Academic Publishers, 1993.

[G57] A. M. Gleason, Measures on the closed subspaces of a Hilbert space, *J. Math. Mech.* **6** (1957), 885–893.

[GLIM] *Andrew M. Gleason: Glimpses of a Life in Mathematics*, privately printed Festschrift, Boston, 1992.

[J66] M. Jammer, *The Conceptual Development of Quantum Mechanics*, McGraw-Hill, 1996.

[J74] ______ , *The Philosophy of Quantum Mechanics*, Wiley-Interscience, 1974.

[KS] S. Kochen and E. P. Specker, The problem of hidden variables in quantum mechanics, *J. Math. Mech.* **17** (1967), 59–87.

[M] S. Maeda, Probability measures on projections in von Neumann algebras, *Rev. Math. Phys.* **1** (1990), 235–290.

[M57] G. W. Mackey, Quantum mechanics and Hilbert space, *Amer. Math. Monthly* **64** (1957), special issue, 45–57.

[M63] ______, *Mathematical Foundations of Quantum Mechanics*, Addison-Wesley, 1963; Dover reprint, 2004.

[McA] Stephen McAdam, Bell's theorem and the demise of local reality, *Amer. Math. Monthly* **110** (2003), 800.

[P] A. Paszkiewicz, Measures on projections of von Neumann algebras, *J. Funct. Anal.* **62** (1985), 87–117.

[Pi] C. Piron, *Foundations of Quantum Physics*, Benjamin, 1976.

[SO] R. M. Solovay, Gleason's theorem for non-separable Hilbert spaces: Extended abstract, `http://math.berkeley.edu/~solovay/Preprints/Gleason_abstract.pdf`.

[VN] J. von Neumann, *Mathematische Grundlagen der Quantenmechanik*, Springer, 1932. English translation:

Mathematical Foundations of Quantum Mechanics, Princeton Univ. Press, 1954.

[VW] B. L. van der Waerden, *Sources of Quantum Mechanics*, North-Holland, 1967; Dover edition, 1968.

[Y1] F. J. Yeadon, Measures on projections of W* algebras of type IIy_1, *Bull. London Math Soc.* **15** (1983), 139–145.

[Y2] ______ , Finitely additive measures on projections in finite W-star algebras, *Bull. London Math. Soc.* **16** (1984), 145–150.

Lida Barrett

Andy Gleason and the Mathematics Profession

I knew and respected Andy Gleason as a mathematician for most of my career and most of his. His contributions to mathematics are well known and worthy of respect, but his overall contribution to the mathematics profession goes far beyond the mathematics he did, the courses he taught, the students he influenced, his role on the Harvard campus, and his extensive commitment to mathematics education. For many years Andy was the consummate person to call upon to represent the profession in a variety of settings. His credentials were impeccable: a Yale graduate, a Harvard professor with a chair in mathematicks (with a "ck") and natural philosophy. What better person to send to Washington to testify before a congressional committee or to add to the Mathematical Science Education Board of the National Academy of Sciences or to have as a spokesperson at the Council of Scientific Society Presidents? Not only did he have the credentials, but when he spoke, he had something to say: thoughtful, well conceived, suitable to the audience, comprehensive, to the point, and, most likely, brief. His manner was gracious and his demeanor modest. Raoul Bott said it well at Andy's retirement party:

> The straightness Andy brings to his mathematics he extends to all that have dealings with him. In these many years together I have never heard a word that seemed false in what he had to say. Nor have I seen him hesitate to take on any task, however onerous, for the welfare of the Department or the University. Needless to say, the rest of us are masters of this art. For, of course, the best way to avoid a chore is to be out of earshot when it is assigned. Hungarians imbibe this principle with their mother's milk, but Andy, for all his brilliance, never seems to have learned it [GLIM].

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Within the profession Andy served in many ways. He was president of the American Mathematical Society in 1981 and 1982. At the Mathematical Association of America he served on the committee on the Putnam Prize Competition (he placed in the top five three years in a row during his years as an undergraduate) and the Science Policy Committee. In 1996 the MAA honored him with its Yueh-Gin Gung and Dr. Charles Y. Hu Award for Distinguished Service [Gung].

Andy chaired the committee in charge of the 1986 International Congress of Mathematicians in Berkeley, California. Hope Daly, the staff person from the AMS who handled the operation, says of him, "He was wonderful, a great leader. He quickly understood problems when they arose and had immediate answers. And he was really wonderful to work with, humble, pleasant." She offers as an example of the many ways in which he could help his action on the morning of the meeting when he saw the staff making direction signs for the somewhat confusing Berkeley campus to replace those the students had taken down the night before. Asking what needed to be done, he was told the signs had to be tacked up. So he took a stack and a hammer and went out and did just that. After the successful congress he edited the proceedings; see [ICM].

He was a master of exposition for audiences at any level. His 1962 Earle Raymond Hedrick Lectures for the MAA on "The Coordinate Problem" addressed the need for good names. The abstract[1] reads:

> In the study of mathematical structures, especially when computations are to be made, it is important to have a system for naming all of the elements. Moreover, it is essential that the names be so chosen that the structural relations between the various elements can be expressed by relations between their names. When the structure has cardinal $\aleph_0$ it is natural to take integers or finite sequences of integers as names. When the cardinal is c, it is appropriate to take real numbers or sequences of real numbers as names. Most mathematical systems are described initially in terms of purely synthetic ideas with no reference to the real number system. Theorems concerning the existence of analytic representations for different types of structures [are] discussed.

He also wrote for the general reader. In *Science* in 1964 he explained the relationship between

[1] *We have been unable to locate a copy of the text of the lectures.*

topology and differential equations [DE]. His first paragraph sets the tone for that hard task:

> It is notoriously difficult to convey the proper impression of the frontiers of mathematics to nonspecialists. Ultimately the difficulty stems from the fact that mathematics is an easier subject than the other sciences. Consequently, many of the important primary problems of the subject—that is, problems which can be understood by an intelligent outsider—have either been solved or carried to a point where an indirect approach is clearly required. The great bulk of pure mathematical research is concerned with secondary, tertiary, or higher-order problems, the very statement of which can hardly be understood until one has mastered a great deal of technical mathematics.

In spite of these formidable difficulties, he concludes his introduction:

> I should like to give you a brief look at one of the most famous problems of mathematics, the *n*-body problem, to sketch how some important problems of topology are related to it, and finally to tell you about two important recent discoveries in topology whose significance is only beginning to be appreciated.

Needless to say, he succeeds.

The other essays in this collection detail the depth and significance of his work in mathematics and mathematics education. Here I have sought to acknowledge how he has contributed both to our profession and far beyond it, to the understanding of the role of mathematics in today's world.

On a personal note, I found Andy the source of extraordinarily useful nonmathematical information. I have capitalized personally on his knowledge of interesting books, speeches, and other activities nationwide, and the latest scoop on restaurants and auto mechanics in the Cambridge area. It was fun, rewarding, and challenging to work with him. I will miss his presence in the mathematics community.

References

[DE] Andrew M. Gleason, Evolution of an active mathematical theory, *Science* **31** (July 1964), 451–457.

[GLIM] *Andrew M. Gleason: Glimpses of a Life in Mathematics*, privately printed Festschrift, Boston, 1992.

[Gung] H. O. Pollack, Yueh-Gin Gung, and Charles Y. Hu, Award for Distinguished Service to Andrew Gleason, *Amer. Math. Monthly* **103** (2) (1996), 105–106.

[ICM] A. M. Gleason, editor, *Proceedings of the International Congress of Mathematicians, Berkeley* (August 3–11, 1986), American Mathematical Society, Providence, RI, 1987.

Deborah Hughes Hallett (with T. Christine Stevens, Jeff Tecosky-Feldman, and Thomas Tucker)

Andy Gleason: Teacher

Andy Gleason was a teacher in the widest possible sense of the word: he taught us mathematics, he taught us how to think, and he taught us how to treat others.

From Andy I learned the importance of a teacher seeing mathematics through both a mathematician's and a student's eyes. Andy's mathematical breadth is legendary; his curiosity and empathy about the views of students, be they first-graders or graduate students, were equally remarkable. I vividly remember his concern in the early years of the AIDS epidemic that an example about the prevalence of HIV infections would upset students. Equally vivid in my memory is Andy's delight when his approach to the definite integral and his insight into student understanding came together to produce a much better way to teach integration. This was one of dozens of occasions when Andy made those around him rethink familiar topics from a fresh viewpoint. New ideas about teaching bubbled out of Andy's mind continuously; he was equally quick to recognize them in others. When one of his former Ph.D. students, Peter Taylor, sent Andy some calculus problems, Andy gleefully suggested that we try them. He regarded teaching mathematics—like doing mathematics—as both important and also genuinely fun.

In the Classroom and as an Advisor

At Harvard Andy regularly taught at every level. He never shied away from large, multisection courses with their associated administrative burden. He was always ready to step forward into the uncharted territory of a new course in real analysis,

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calculus, quantitative reasoning, or the history of mathematics.

Christine Stevens, one of Andy's doctoral students, writes:

> I first encountered Andy in the fall of 1971, when I enrolled in his course on The Structure of Locally Compact Topological Groups (Math 232). It goes without saying that the course was a model of lucid exposition, but I also remember Andy's enthusiastic and often witty responses to students' questions. Indeed, some of them are recorded in the margins of my notebook, alongside some rather deep mathematics. I also recall the cheerful energy with which he lectured one cold winter day when the heating system in Sever Hall had given out.
>
> I eventually wrote my dissertation on an issue that Andy had mentioned in that course. We mapped out an approach in which the first step involved proving something that he deemed "almost certainly true." When he commenced one of our subsequent appointments by asking me how things were going, I replied, "not too well." I explained that I had proved that the statement that was "almost certainly true" was equivalent to something that we had agreed was probably false. To be honest, I was kind of down in the dumps about the situation. Andy's response was immediate and encouraging. Without missing a beat, he replied, "Well, that's not a problem. Just change the hypotheses!"

Courses, Books, and Classroom Notes

In 1964 Andy instituted a new course at Harvard, Math 112, to provide math majors a transition from the three-year calculus sequence to Math 212, the graduate course in real analysis. It functioned as an introduction to the spirit of abstract mathematics: first-order logic, the development of the real numbers from Peano's axioms, countability and cardinality. This was the first of the "bridge" courses now ubiquitous for math majors, only twenty years before its time. Tom Tucker recalls:

> I was a student in that first Math 112, and it was my first experience with Andy. He chided me that the course might be too elementary for me, since most students from Math 55 went straight on to Math 212. But I had taken Math 55 as my first course at Harvard and was still in shock. I needed some encouragement, something I really could understand, and that is exactly what Andy gave me. He helped salvage my mathematical career.

Andy's work in Math 112 led to his only solo text in mathematics, *Fundamentals of Abstract Analysis.*[1] In his review of the book, Dieudonné captures the essence of Andy's pedagogy:[2]

> Every working mathematician of course knows the difference between a lifeless chain of formalized propositions and the "feeling" one has (or tries to get) of a mathematical theory, and will probably agree that helping the student to reach that "inside" view is the ultimate goal of mathematical education; but he will usually give up any attempt at successfully doing this except through oral teaching. The originality of the author is that he has tried to attain that goal in a textbook, and in the reviewer's opinion, he has succeeded remarkably well in this all but impossible task.

Over the course of his teaching career, Andy wrote hundreds of pages of lecture notes for his students, reworking them afresh each year. Some were handwritten on spirit duplicator sheets; some were typeset using macros he developed under an early version of Unix. More than lecture notes, these were complete with hand-drawn figures and exercises. His efforts in course development in the early 1970s included two complete unpublished texts. The first was for a new full-year integrated linear algebra/multivariable calculus course (Math 21) , the second for the history-based general education course Natural Sciences 1a: Introduction to Calculus.

Andy combined his interest in education, mathematics, and history in his design for Natural Sciences 1a. Nothing like a standard treatment of the material, this course took a historical approach to the development of the basic ideas of calculus, beginning with an explication of Archimedes' *The Sand Reckoner* and culminating with a derivation of Kepler's laws of planetary motion from Newton's physical laws.

Natural Sciences 1a was intended for the nonspecialist student with an interest in the history of ideas. Andy wanted the students to grapple with issues like irrationality and continuity. Many of his assignments asked students for nontechnical essays in which they explored the mathematics through personal contemplation. Students signing up for this course seeking an easy way to satisfy

[1] *Gleason, A. M.,* Fundamentals of Abstract Analysis*, published first by Addison Wesley, then by A K Peters (1991).*

[2] *Math Reviews: MR0202509 (34 #2378).*

Andy's Students (Thesis titles and year of degree)

Bolker, Ethan David
Functions resembling quotients of measures (1965).

Bredon, Glen Eugene
Some theorems on transformation groups (1958).

Brown, Julia May Nowlin
Homologies and elations of finite projective planes (1970).

Cohen, Daniel Isaac Aryeh
Small rings in critical maps (1975).

Cohn, Donald L
Topics in liftings and stochastic processes (1975).

Getchell, Charles Lawrence
Construction of rings in modular-lattices (1973).

Grabiner, Sandy
Radical Banach algebras and formal power series (1967).

Hales, Raleigh Stanton, Jr.
Numerical invariants and gamma products of graphs (1970).

Kennison, John Frederick
Natural functors in topology and generalizations (1963).

Krause, Ralph Mack
Minimal metric spaces (1959).

Kronstadt, Eric Paul
Interpolating sequences in polydisks (1973).

MacWilliams, Florence Jessie
Combinatorial problems of elementary abelian groups (1962).

Marcus, Daniel Alan
Direct decompositions of commutative monoids (1972).

Monash, Curt Alfred
Stochastic games: the minmax theorem (1979).

Oberg, Robert Joseph
Functional differential equations with general perturbation of argument (1969).

Palais, Richard Sheldon
A global formulation of the Lie theory of transformation groups (1956).

Phelps, Mason Miller
The closed subalgebras of a commutative algebra over the real numbers (1958).

Puckette, Miller Smith
Shannon entropy and the central limit theorem (1986).

Ragozin, David Lawrence
Approximation theory on compact manifolds and Lie groups, with applications to harmonic analysis (1967).

Rochberg, Richard Howard
Properties of isometries and almost isometries of some function algebras (1970).

Sidney, Stuart Jay
Powers of maximal ideals in function algebras (1966).

Spencer, Joel Harold
Probabilistic methods in combinatorial theory (1970).

Stevens, Terrie Christine
Weakened topologies for Lie groups (1979).

Stromquist, Walter Rees
Some aspects of the four-color problem (1975).

Taylor, Peter Drummond
The structure space of a Choquet simplex (1969).

Turyn, Richard Joseph
Character sums and difference sets (1964).

Wang, Helen Pi
Function-algebra extensions and analytic structures (1973).

Yale, Paul Blodgett
A characterization of congruence groups in geometries of the Euclidean type (1959).

a requirement got a lot more than they bargained for.

Educational Philosophy

Andy was always interested in how people learn. He really wanted to know what goes on in students' brains when they think about mathematics: the semantics, the grammar, the denotations and connotations, the cognition. His concern extended from teaching analysis to Harvard undergraduates to teaching arithmetic to grade school students. It was all important to him.

His educational philosophy combined the pragmatic and the radical. He could be a stickler about precision, insisting always on "the function f", rather than "the function $f(x)$", but the reasons were always cognitive—students often confuse the function with its formula. On the other hand, he did not insist on formality. He had no problem describing the continuity of the function f at $x = a$ as "you can make $f(x)$ as close as you want to $f(a)$ by making x close enough to a." The physicist Richard Feynman once criticized mathematicians for "preferring precision to clarity." Andy always preferred clarity.

Andy's inquiries about learning mathematics sometimes led to radical positions. In his article[3] "Delay the teaching of arithmetic" he suggested that the usual algorithms of arithmetic not be taught until grade 6. He cited work[4] of Benezet on just such an experiment in the Manchester, NH, schools in the 1930s. The students not taught the algorithms learned them perfectly well in seventh grade, but their problem-solving ability, their willingness to "take responsibility for their answers," was dramatically better than the control group's. In his paper Andy recalls his own childhood math classes requiring four calculations for each day: a sum of seven 6-digit numbers, a subtraction of two 7-digit numbers, a product of a 6-digit number by a 3-digit number, and a long division of a 6-digit number by a 3-digit number; answers were graded right or wrong and 75% was passing. Andy estimates the number of individual operations for each problem and concludes that a student getting each operation correct with 99.5% probability would still average only 73, failing. As Andy remarked once on long division, getting even one problem correct out of ten indicates sufficient understanding of the algorithm.

Andy was acutely aware of the importance of students' attitudes toward mathematics, as evidenced by his remarks[5] in the 1980s to the National Academy of Sciences:

[3] http://www.inference.phy.cam.ac.uk/sanjoy/benezet/gleason.pdf.

[4] *Benezet Centre*, http://www.inference.phy.cam.ac.uk/sanjoy/benezet/.

[5] *From unpublished notes in Gleason's files.*

> Right now there is debate apparently existing as to how mathematics should react to the existence of calculators and computers in the public schools. What should be the effect on the curriculum?…and so on. Now the unfortunate point of that is that there is even a very serious debate as to whether there should be an impact on the curriculum. That is what I regard as absolutely ridiculous. Let me just point out that… in this country there are probably 100,000 fifth grade children right now learning to do long division problems. In that 100,000 you will find very few who are not thoroughly aware that for a very small sum of money (like $10) they can buy a calculator which can do the problems better than they can ever hope to do them. It's not just a question of doing them just a little better. They do them faster, better, more accurately than any human being can ever expect to do them and this is not lost on those fifth graders. It is an insult to their intelligence to tell them that they should be spending their time doing this. We are demonstrating that we do not respect them when we ask them to do this. We can only expect that they will not respect us when we do that.

About ten years ago Andy gave a talk at the Joint Mathematics Meetings in which he described how he had, some years previously, spent a summer teaching arithmetic to young children. His goal had been to find out how much they could figure out for themselves, given appropriate activities and the right guidance. At the end of his talk, someone asked Andy whether he had ever worried that teaching math to little kids wasn't how faculty at research institutions should be spending their time. Christine Stevens remembers Andy's quick and decisive response: "No, I didn't think about that at all. I had a ball!"

Education at a National Level

Andy led in promoting the involvement of research mathematicians in issues of teaching and learning.

He was deeply involved with the reform of the U.S. mathematics K–12 curriculum in the post-Sputnik era. He chaired the first advisory committee for the School Mathematics Study Group (SMSG), the group responsible for "the new math". He was a codirector with Ted Martin of the 1963 Cambridge Conference on School Mathematics. The report of that conference proposed an ambitious curriculum for college-bound students that culminated in a full-blown course in multivariable calculus in n-dimensions including the Inverse Function Theorem, differential forms, and Stokes' Theorem. Although the proposed curriculum would appear to be far too sophisticated by today's standards, the space race loomed large in the public mind and the need for highly trained scientists, mathematicians, and engineers became a national crusade. The SMSG program begun in 1959 was aimed at all students and was roundly criticized at the time as being inappropriate for average students and teachers. The Cambridge Conference appeared to be an attempt to woo research mathematicians to school reform through consideration of an "honors" track for the most able students. In that context, some critics complained the proposed curriculum was "timid"!

In 1985–89, Andy helped establish the Mathematical Sciences Education Board to coordinate educational activities for all the mathematical professional organizations; his citation for the MAA Distinguished Service Award recognized the importance of this contribution. From the 1980s until his death, Andy was influential in calculus reform and the subsequent rethinking of other introductory college courses.

That a mathematician of Andy's stature would take the time to think deeply about the school curriculum made such work legitimate.

Quantitative Reasoning (QR)

In the late 1970s Harvard College undertook a sweeping reorganization of the General Education requirements. The new core curriculum replaced existing departmental offerings with specially designed courses in a broad variety of areas of discourse. It was hard to see how mathematics fit in the new core. Given his extensive contact with curricular projects and his interest in education, Andy was a natural choice to lead an investigation into what a mathematics requirement might be and how it was to be implemented.

Rather than drawing up a checklist of what kinds of mathematics a Harvard graduate should know, Andy instead started with the idea that at the very least, the core requirement in mathematics should prepare students for the kinds of mathematical, statistical, and quantitative ideas they'd be confronting in their other core courses. Working with faculty who were developing those courses, Andy quickly realized that the skills students required had more to do with the presentation, analysis, and interpretation of data than with any particular body of mathematics, such as calculus. Thus, the core Quantitative Reasoning Requirement, or QRR, was born.

So, long before quantitative literacy became a well-defined area of study with its own curriculum and textbooks, Andy and Professor Fred Mosteller of the Harvard statistics department developed a small set of objectives for the QRR. These included understanding discrete data and simple statistics, distributions and histograms, and simple hypothesis testing. There was no reliance on high school algebra or other mathematics that students had seen before, since high schools had not yet begun offering an Advanced Placement Statistics course. So the requirement leveled the playing field—both math majors and history majors would have to learn something new to satisfy the QRR.

Andy also thought about implementing the QRR—how to help 1,600 first-year students meet the requirement without mounting an effort as large, and costly, as freshman writing. He decided that the ideas students were being asked to master, while novel, were not very hard and that most students could learn them on their own, given the appropriate materials. For the small number of students who couldn't learn from self-study materials, there would be a semester-long course.

So, in the summer of 1979, Andy gathered a team of about a dozen undergraduates ("the Core corps") who wrote self-study materials and gathered newspaper articles for practice problems. These were published as manuals and supplied to all entering students. Andy invited the student authors to his home in Maine that summer, which was typical of his friendliness and openness. Jeff Tecosky-Feldman, then the student leader of the Core corps, helped organize the trip to Maine. He recalls:

> The other students were buzzing with the rumor that Andy had been involved in cracking the Japanese code in World War II, but were too timid to ask him about it themselves, so they put me up to it. When I asked Andy, his response was typical: "It would not be entirely incorrect to say so", and he left it at that.

Calculus

In January 1986 Andy participated in the Tulane Conference that proposed the "Lean and Lively" calculus curriculum. October 1987 saw Andy on the program at the "Calculus for a New Century" conference; in January 1988 the idea for the Calculus Consortium based at Harvard took shape.

Andy's role in the Calculus Consortium was without fanfare and without equal. He started by gently turning down my request that he be the PI on our first NSF proposal and, after a thirty-second silence that seemed to me interminable, suggested we be co-PIs. He then helped build one of the country's first multi-institution collaborative groups. Now commonplace, such arrangements were at the time viewed with some skepticism at the NSF, whose program officers wondered whether such a large group could get anything done.

Throughout his time with the consortium, Andy's words, in a voice that was never raised, were the keel that kept us on course. His view of the importance (or lack of it) of various topics in the calculus curriculum shaped many of our discussions, and his vision inspired many of our innovations. Andy hated to write—he saw the limitations of any exposition—so we quickly learned that the best way to get his ideas on paper was for one of us to write a first draft. This drew him in immediately as he reshaped, rephrased, and in essence rewrote the piece. That Andy could do this for twenty years without denting an ego is a testament to his skill as a teacher. Who else could say, as I responded to a flood of red ink by asking whether I'd made a mistake, "Oh no, much worse than that" and have it come across as a warm invitation to discussion? We all remember Andy remarking, "That's an interesting question!" and knowing that we were about to see in an utterly new light something we'd always thought we understood.

The 1988 NSF proposal led to a planning grant in 1989. The founding members of the consortium met for the first time in Andy's office. Faculty from very different schools discovered to their surprise that students' difficulties were similar in the Ivy League and in community colleges. A multiyear proposal followed, with features now commonplace in federally funded proposals but then unusual. Andy was skeptical about some of these and suggested we remove the section on dissemination—after all, he pointed out, we didn't know whether what we'd write would be any good. When the proposal went to the NSF for feedback before the final submission, I got a call from the program director, Louise Raphael, asking about the missing section on dissemination. When I explained, Louise, who knew how things worked in DC, responded by saying I should tell Andy "not to be a mathematician." We then understood our mandate from the NSF to disseminate the discussion of the teaching of calculus to as many departments and faculty as possible. Over the next decade we gave more than one hundred workshops for college faculty and high school teachers, in which Andy played a full part—presenting, answering questions, and listening to concerns.

The debate about calculus benefitted enormously from Andy's participation. He became a father figure for calculus reform in general and the NSF-supported project at Harvard in particular. His goal was never reform per se; it was to discuss openly and seriously all aspects of mathematics learning and teaching. In 1997 Hyman Bass wrote[6] "It is the creation of this substantial community of professional mathematician-educators that is the most significant (and perhaps least anticipated) product of the calculus reform movement. This is an achievement of which our community can be justly proud and which deserves to be nurtured and enhanced."

[6] *Bass, H., Mathematicians as Educators,* Notices of the AMS, *January 1997.*

Andy—reasoned, calm, soft-spoken, a gentleman in every sense of the word—was dedicated to this community throughout his life.

Outside the Classroom

Andy had an extraordinary range of knowledge. He talked about baseball scores, horsemanship,[7] Chinese food in San Francisco, and the architecture of New York with the same insight he talked about mathematics. He was fascinated by every detail of the world around him. He persuaded a cameraman to show him the inside of the video camera when we were supposed to be videotaping. When we were "bumped" to first-class on a plane, Andy was much less interested in the preflight drink service than in listening to the pilots' radio chatter so that he could calculate the amount of fuel being loaded onto the plane. To the end of his life, Andy investigated the world with a newcomer's unjaded curiosity.

Andy inspired rather than taught many of us. His transparent honesty and humility were so striking that they were impossible to ignore. For example, before publishing my first textbook, I asked him how authors got started, since publishers wanted established names. Andy replied matter-of-factly, "Most people never do," returning to me the responsibility to achieve this.

Andy's moral influence was enormous. Always above the fray and without a mean bone in his body, Andy commanded respect without raising his voice. His moral standards were high—very high—making those around him aspire to his tolerance, understanding, and civility. Andy's presence alone forged cooperation.

In his commentary on the first book of Euclid's *Elements*, Proclus described Plato as having "… aroused a sense of wonder for mathematics amongst students." These same words characterize Andy. Through the courses he taught and the lectures that he gave for teachers, Andy inspired thousands of students with his sense of the wonder and excitement of mathematics. Through him, many learned to see the world through a mathematical lens.

Leslie Dunton-Downer

Andrew Gleason—a Remembrance. Remarks Delivered at the Memorial Service, Memorial Church, Harvard University, November 14, 2008

I was a Junior Fellow in Comparative Literature at the Harvard Society of Fellows during the final years that Professor Andrew Gleason served as its chair, from 1993 to 1996. The society gathers researchers from all fields: from astrophysics, classics, economics, and others, clear through to zoology. Fellows at the society spend three years free from any requirement or examination, pursuing, and I now quote from the vows that all new Fellows take: "a fragment of the truth, which from the separate approaches every true scholar is striving to descry." On Monday nights in academic term-time, Junior Fellows converse with a dozen or so Senior Fellows, professors who not only elect Junior Fellows but also engage them in mind-opening conversations over suppers in a dining hall furnished to nourish these exchanges. Professor Gleason, who had himself been a Junior Fellow, officiated when I took my vows at the society, and as chair he presided over Monday night dinners during my three years in his fellowship. This is how I knew him, and my fondness for him grew exponentially with each passing season at the society.

Gleason on a horse farm, with the inevitable clipboard under his arm.

The first time I beheld Professor Gleason, here was the situation: It was a Monday morning at the so-called Yellow House, at 78 Mount Auburn Street here in Cambridge, the society's administrative base camp. I was surely extremely nervous, because I was to be interviewed that very afternoon by the full assemblage of Senior Fellows, a terrifying prospect—each Senior Fellow was an academic star in his respective field, and many were reputed to be intimidating. A Junior Fellow had been assigned the task of showing me about the building. At one point, noisily chattering, we made our way down a corridor, where an office door was opened widely. We paused there to look through the doorway. Inside the room, there was a man leaning back on his chair before an empty desk. His head was

[7] *Andy's daughters are accomplished equestrians.*

Leslie Dunton-Downer is a writer. She thanks Jean Berko Gleason, Diana Morse, Martha Eddison, and Melissa Franklin for their help in preparing this remembrance.

tilted skyward, and his eyes focused on a point that appeared to be on the ceiling, but may have been further off. We stood there for an awkward few beats. I believe that we were together unsure if the man was about to greet us or if we ought to take the initiative to greet him. But he remained still, absorbed in his own world. The Junior Fellow and I shrugged at each other and continued on our way until we reached a common room out of earshot. I turned to her, made a quizzical face, and asked, "What was that man doing?" "Math," she said.

I had never before seen a real mathematician in the act of doing math. I was mystified by the absence of any tools in his office. Wouldn't he require a calculator or a slide rule or something to inspire himself to be mathematical—maybe a chessboard or a Rubik's Cube? At the very least, what about a pencil and paper? She shook her head: "That mathematician is Andrew Gleason. He works in his head."

Professor Gleason was, I think by disposition, a decipherer. He had deciphered codes and mathematical problems and as a hobby took delight in deciphering the movements of celestial bodies. On Monday nights Professor Gleason sat at the head of a horseshoe-shaped table in the society's dining room. Like all chairs at the society, he would guide his flocks of Junior Fellows in his own way, leaving his own signature on the institution. He was not a garrulous chair—"Oscar Wildean" is not the first adjectival phrase that comes leaping to mind to describe his conversational style—but he could become animated suddenly, and with deep sincerity, when conversation turned to subjects close to his heart: astronomy, classical music, and, among so many others, of course, math.

Much of the time he would listen or observe with his extraordinary Gleasonian powers of concentration. Many of us wondered what he was thinking on those occasions when he was so sharply present yet enigmatically silent. Perhaps he was deciphering us. He never made a single judgmental remark; his leadership was delicate, trusting, and surefooted. He put out a strong aura of principled tranquility, as if his Junior Fellows' paths, and indeed the paths of all people and objects and ideas in his midst, no matter how rough, were part of a larger pattern that would eventually become clearer to him. I found a poem that captures this Professor Gleason, the one whom I and others came to know in a quiet way and to love with great respect, a man whose presence we now begin to sense expanding through all that he discerned. He seems to be present in these lines by Robinson Jeffers:

I admired the beauty
While I was human, now I am part of the beauty.
I wander in the air,
Being mostly gas and water, and flow in the ocean;
Touch you and Asia
At the same moment; have a hand in the sunrises
And the glow of this grass.

The last time that I saw Andrew Gleason was at the annual dinner held by the society in May of this year. The gathering took place at the Fogg Museum, a few meters to the east of here. On that occasion, many Junior Fellows from Professor Gleason's time as chair gathered to catch up with one another and with him. He had led us through our fellowship years with a light touch, a seemingly invisible touch. He always encouraged each fellow to wrestle with those daunting "fragments of truth" on his or her own terms, come what may. It had only been with hindsight, after leaving the society, that many of us came to appreciate the subtle qualities of his leadership, how he shaped our lives, both inwardly and in action, even as he had often seemed chiefly to be deciphering the world, working things out in his head.

Stephen Hawking published this observation twenty years ago, by chance on the eve of Professor Gleason's becoming chair at the society: "We do not know what is happening at the moment farther away in the universe: the light that we see from distant galaxies left them millions of years ago, and in the case of the most distant object that we have seen, the light left some eight thousand million years ago. Thus, when we look at the universe, we are seeing it as it was in the past."

Perhaps we are only now beginning to see Andrew Gleason. Those of us who had the privilege to know him will cherish the light that he casts out to us, even in his absence—perhaps all the more forcefully because of his absence or, rather, because he has now become a beautiful part of the beauty that he once admired.

References

[1] Robinson Jeffers, "Inscription for a Gravestone", *The Collected Poetry of Robinson Jeffers*, edited by Tim Hunt, Stanford University Press, 2001, p. 372.

[2] Stephen Hawking, *A Brief History of Time*, Bantam Dell, 1988.

Jean Berko Gleason

A Life Well Lived

I would like to begin these remarks by thanking everyone on behalf of our family—myself and our daughters, Katherine, Pam, and Cynthia—for the outpouring of hundreds of messages that we have received about Andy and your friendship with him. A number of themes stood out in these messages: you often talked of his brilliance, his kindness, his sense of humor, his generosity, fairness, and welcoming spirit. Newcomers to the Society of Fellows or to the mathematics department at Harvard were not only made to feel at home, but they had

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Andy and Jean in 1958.

rigorous intellectual discussions with Andy in which they found that their views and opinions were both challenged and respected. An hour's talk left you with weeks of things to think about.

Others will speak about Andrew Gleason's lasting contributions to science and to education. I would like to tell you a little bit about him as a person. I met Andy Gleason by accident over fifty years ago. I was a graduate student at Radcliffe College and he was a young Harvard professor, luckily not in my field, which is psycholinguistics. But I had friends in the Harvard mathematics department who were giving a party. When they told me that the famous Tom Lehrer was going to be at the party and that he might also play the piano and sing, I decided to go. Tom did sing, but I never got across the crowded room to meet him. Instead I met this slim young fellow who invited me out to dinner. So that was the beginning of our relationship, which soon led to a marriage that lasted forty-nine years. Since Andy was not the type of person to talk about himself very much, I'd like to tell you a few things about his origins that you may not know.

You may think of Andy as quintessentially New England, white Anglo-Saxon Protestant—the blue eyes, the pale skin, the disinterest in worldly goods. It is mostly true: his father was a member of the Mayflower Society. Andy was a direct descendent of four people who came on the *Mayflower*, including Mary Chilton, who by tradition was the first woman to come ashore at Plymouth Rock. But perhaps you did not know that Andy was also just a little bit Italian. His middle name, Mattei, came from his grandfather, Andrew Mattei, an Italian-Swiss winemaker who came to Fresno, California, and established vineyards, where he prospered and produced prizewinning wine. Andrew Mattei's daughter, Theodolinda Mattei, went to Mills College in California and on graduation did what all wealthy, well-bred young women of the day did: she embarked on the grand tour, a trip around the world via steamship, with, of course, a chaperone. On board ship Theodolinda met a dashing young botanist on his way to collect exotic plant specimens. This quickly became a classic shipboard romance and led to the marriage in 1915 of Theodolinda Mattei and Henry Allan Gleason, who was to become not only Andrew's father but a famous botanist, chief curator of the New York Botanical Garden, and early taxonomist and ecologist whose work is still cited—he wrote the classic works on the plants of North America. Andy had an older brother, Henry Allan Gleason Jr.; Andy's older sister, Anne, is one of the smartest people I have ever met.

We were married on January 26, 1959. This was actually the day of the final examination in the course Andy was teaching. So he gave out the blue books at 2:15 and came here to the Appleton Chapel of The Memorial Church to get married at 3 p.m. We took a wedding trip to New Orleans, and he did not bring the exams. Over the next forty-nine years we raised our three talented daughters, bought a house in Cambridge and a wonderful house on a lake in Maine, and traveled all over the world, sometimes to see some of Andy's favorite things, which included total eclipses of the sun, most recently in 2006 sailing off the coast of Turkey. We were both teaching, of course, and maintaining our own careers, but we managed to have a lot of fun too. During those forty-nine years Andy maintained the calm spirit he was known for and really never raised his voice in anger. He had a great sense of humor and was extraordinarily generous, giving away surprisingly large sums of money, often to his favorite schools: Harvard and his alma mater, Yale.

Because mathematics was truly his calling, Andy never stopped doing mathematics. He carried a clipboard with him even around the house and filled sheets of paper with ideas and mysterious (to me) numbers. When he was in the hospital during his last weeks, visitors found him thinking deeply about new problems. He was an eminent mathematician. He was also a good man, and he led a good life. We are sorry it did not last a little longer.

Note: Unless otherwise indicated, all photographs and other images in this article are courtesy of Jean Berko Gleason.