1996 Steele Prizes

Three Leroy P. Steele Prizes were awarded at the Summer Mathfest held at the University of Washington in Seattle in early August. These prizes were established in 1970 in honor of George David Birkhoff, William Fogg Osgood, and William Caspar Graustein and are endowed under the terms of a bequest from Leroy P. Steele.

The Steele Prizes are awarded in three categories: for expository writing, for a research paper of fundamental and lasting importance, and for cumulative influence extending over a career, including the education of doctoral students. The current award is $4,000 in each category.

The recipients of the 1996 Steele Prizes are BRUCE C. BERNDT and WILLIAM FULTON for Mathematical Exposition, DANIEL STROOCK and S. R. S. VARADHAN for Seminal Contribution to Research, and GORO SHIMURA for Lifetime Achievement.

The Steele Prizes are awarded by the AMS Council acting through a selection committee whose members at the time of these selections were Richard Askey, Ingrid Daubechies, Eugene Dynkin, H. Blaine Lawson, Andrew J. Majda, Barry Mazur, Marina Ratner, Gary M. Seitz, and William P. Thurston.

The text that follows contains, for each award, the committee's citation, the recipient's response, and a brief biographical sketch of the recipient.

Steele Prize for Mathematical Exposition: Bruce C. Berndt

Citation

To Bruce C. Berndt for the four volumes *Ramanujan's Notebooks*, Parts I, II, III, and IV, Springer, 1985, 1989, 1991, and 1994. In recognition of Berndt's heroic and extraordinary achievement in exposing to the general mathematical researcher a trove of results that were utterly inaccessible before, the AMS decided this year, exceptionally, to broaden the standard interpretation of "exposition". In an impressive scholarly accomplishment spread out over 20 years, Berndt has provided a readable and complete account of the notebooks, making them accessible to other mathematicians. Ramanujan's enigmatic, unproved formulas are now readily available, together with context and explication, often after the most intense and clever research efforts on Berndt's part.

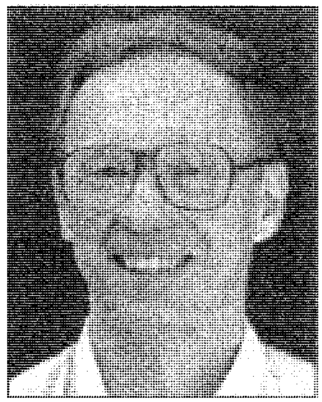
Response

I owe my first debt of gratitude to the late Emil Grosswald. It was on a cold winter day in early February 1974, while on leave at the Institute for Advanced Study, that I was reading two papers of Grosswald in which he proved some formulas from Ramanujan's notebooks. I suddenly realized that I could also prove these formulas by using some transformation formulas for Eisenstein series that I had proved about two years earlier. I was naturally curious to determine if there were other formulas in the notebooks that I could prove using my theorems. Fortunately, the library at Princeton University had a copy of the Tata Institute's photostat edition of Ramanujan's Notebooks. I found a few more formulas which I could prove, but I also found a few thousand others which I could not prove.

In the spring of 1977, I set myself the task of attempting to prove all the results in Chapter 14 (87 altogether) of the second notebook, where the formulas which Grosswald had proved can be found. After I had been working on this project for nearly a year, George Andrews informed me that the library at Trinity College, Cambridge, possessed the notes that B. M. Wilson and G. N. Watson had accumulated in their efforts to edit the notebooks in the 1930s. I decided that, with these notes, I could possibly edit further chapters, and so wrote Trinity for a copy. To make a long story short, since May 1977, I have devoted all of my research efforts to establishing the 3,000–4,000 unproved claims made by Ramanujan in his notebooks. In particular, Watson's notes were extremely helpful in the massive amount of material on modular equations.

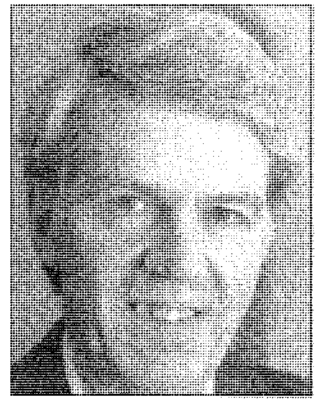
During this time I have been stymied numerous times by Ramanujan's formulas, and after months or years of frustration I often turned to other mathematicians for help. At the University of Illinois, we have been blessed with a large number of very gifted graduate students in number theory, and several of them have made important contributions to my work, both while at Illinois and more frequently in the years that followed. I particularly owe a huge debt of gratitude to my first Ph.D. student, Ron Evans, at UCSD, and to my most recent Ph.D. student, Heng Huat Chan, on his way from the Institute for Advanced Study to National Chung Cheng University. I also express my thanks to the following mathematicians (including former students), without whose help the task of editing the notebooks would not have been completed: George Andrews, Richard Askey, Gennady Bachman, S. Bhargava, Tony Biagioli, David Bradley, Henri Cohen, Frank Garvan, Jim Hafner, Lisa Lorentzen, Kenneth Williams, Don Zagier, and Liang-Cheng Zhang. A more complete list can be found in *Ramanujan's Notebooks*, Part V, which will be submitted to Springer-Verlag early this fall.

I also am grateful to my colleagues in number theory at the University of Illinois for their many suggestions and support. In particular, my association with Springer began in the early 1980s when Heini Halberstam called me to his office to meet Walter Kaufmann-Bühler, who suggested that I compile my work into volumes for Springer. Thanks for the completeness of the bibliographies goes to Nancy Anderson, mathematics librarian at the University of Illinois, for helping me dig up many obscure references. The early years of my work were supported by the Vaughn Foundation, and I extend my sincere gratefulness to James Vaughn for this support. Most of all, I express my thanks (and my continual amazement) to Ramanujan for leaving so many beautiful theorems and formulas to mathematics.



Photograph by Hiram Paley

Bruce C. Berndt



William Fulton

Biographical Sketch

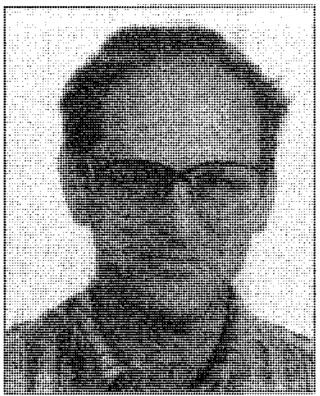
Professor Berndt has his bachelor's degree from Albion College and received his Ph.D. in 1966 from the University of Wisconsin, Madison. His first position after receiving his Ph.D. was at the University of Glasgow (1966–67). While most of his academic life has been spent at the University of Illinois, Urbana-Champaign, he was a member of the Institute for Advanced Study (1973–74). Since 1986 he has served as associate editor of the *Journal of Mathematical Analysis and Applications*. Berndt is one of two coordinating editors for *The Ramanujan Journal*, the first issue of which is scheduled to be published in January 1997 by Kluwer. He has been the recipient of the Young College Educator Award (University of Illinois, 1972), two Lester R. Ford Awards (1989 and 1994), and the Carl B. Allendoerfer Award.

A fifth and final volume of the notebooks, Part V, will be submitted this October. His book *Ramanujan: Letters and Commentary* (coauthored with Robert A. Rankin) was published by the AMS last year.

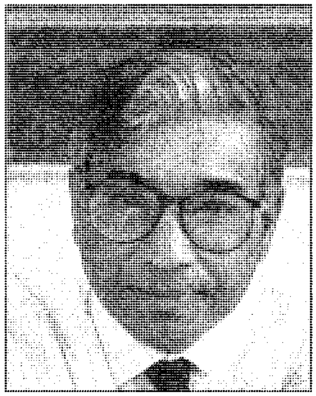
Steele Prize for Mathematical Exposition: William Fulton

Citation

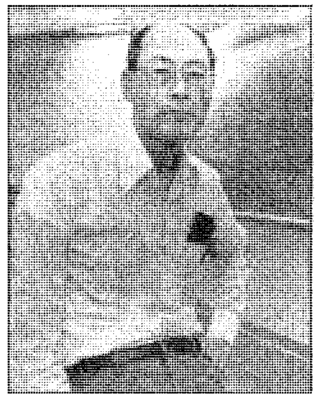
To William Fulton for his book *Intersection Theory*, Springer-Verlag, "Ergebnisse series", 1984. It introduced a new order into a field that had been in disarray, by introducing a new and simpler approach that gave all the old results and more. Moreover it gave clarifying expositions of many classical computations in intersection theory, often reducing lengthy old arguments to a few lucid paragraphs. By its very clear exposi-



Daniel Stroock



S. R. S. Varadhan



Goro Shimura

tion and the high quality of its content, this book has had an enormous impact on the field.

Response

I am very grateful for this Steele Prize for mathematical exposition. As a firm believer that the attempt to say things right is an important part of mathematical research, it is particularly gratifying to have these efforts acknowledged. Intersection theory in algebraic geometry has a long history—much longer and more colorful than I expected when I began work on this project. In classical geometry the varieties being intersected corresponded to conditions on some geometric objects in some parameter space, and one wanted to describe (often to count) the objects satisfying several conditions at once. A problem in the development of intersection theory was what to do about excess intersections, where the varieties being intersected meet in larger-than-the-expected dimension. The standard solution in the 1960s was to move the varieties so they meet properly and work with the deformed intersections. Aside from technical difficulties, this had the disadvantage that the deformed varieties rarely have much geometric significance, so it is hard to relate the deformed intersections to the original problem.

In 1976 R. MacPherson and I realized that we could use "deformation to the normal bundle", which had arisen in our work with P. Baum, in place of moving the subvarieties to general position. This produces a class of cycles living on the original intersection of the varieties; the foundations become simpler and, at least in many cases, the intersection classes are easier to compute. Contributions in this direction were also made by many others, including J. P. Murre, J.-L. Verdier, H. Gillet, R. Lazarsfeld, and S. Kleiman.

While working on this book, it was interesting to discover that many of the ideas we consider modern had been anticipated by the Italian school of geometry earlier in this century, but it was particularly enjoyable to find some of these ideas in papers dating back before 1850. There are surely many more treasures waiting to be discovered in the classical literature related to intersection theory.

Another reason for my appreciation of this award is the hope that it may spur Springer-Verlag to print a new edition, so that the corrections that readers have generously sent to me since 1984 can be incorporated!

Biographical Sketch

William Fulton received his B.A. from Brown University (1961) and his Ph.D. from Princeton University (1966). He held junior positions at Princeton University (1965-66, 1969-70) and at Brandeis University (1966-69). At Brown University Professor Fulton served as associate professor (1970-75), professor (1975-87), and chair (1985-86). He moved to the University of Chicago in 1987, where since 1995 he has been the Charles L. Hutchinson Distinguished Service Professor.

Professor Fulton has published extensively in his fields of research interest. His books include *Algebraic Curves*, *Introduction to Intersection Theory in Algebraic Geometry*, *Introduction to Toric Varieties*, and *Algebraic Topology*. He has served as associate editor of the *Duke Mathematical Journal* (1984-93) and the *Journal of Algebraic Geometry* (1992-93). He is presently serving as editor (1993-) and managing editor (1995-) for the *Journal of the American Mathematical Society*. He is on the editorial boards for two series: Cambridge Studies in Advanced Mathematics and Chicago Lectures in Mathematics.

Professor Fulton was a Guggenheim Fellow during 1980-81. The Swedish Natural Science Research Council has appointed him the Tage Erlander Guest Professor for 1996-97.

Steele Prize for Seminal Contribution to Research: Daniel Stroock and S. R. S. Varadhan

Citation

To Daniel Stroock and Srinivasa Varadhan for their four papers

[1] *Diffusion processes with continuous coefficients I and II*, Comm. Pure Appl. Math. **22** (1969), 345-400, 479-530.

[2] *On the support of diffusion processes with applications to the strong maximum principle*, Sixth Berkeley Sympos. Math. Statist. Probab., vol. III, 1970, pp. 333-360.

[3] *Diffusion processes with boundary conditions*, Comm. Pure Appl. Math. **34** (1971), 147-225.

[4] *Multidimensional diffusion processes*, Springer-Verlag, 1979.

in which they introduced the new concept of a martingale solution to a stochastic differential equation, enabling them to prove existence, uniqueness, and other important properties of solutions to equations which could not be treated before by purely analytic methods; their formulation has been widely used to prove convergence of various processes to diffusions.

Response from Professor Stroock

I am honored to share the Steele Prize with my old friend and colleague, S. R. S. Varadhan. I am also amused to realize that the articles cited might never have been written had TeX been available to our teacher, H. P. McKean, Jr. Indeed, Varadhan's and my collaboration grew out of a seminar on stochastic integration which McKean was conducting at Rockefeller University in 1967. The seminar was based on the preliminary, typewriter-produced manuscript of what would become McKean's famous little treatise on the topic. Using the Springer-Verlag color-coding scheme, McKean had scrupulously marked the original to distinguish between various typefaces. Unfortunately, his efforts were obliterated by xerography. As I recall, there was one page on which the letter "e" was to be typeset in five different fonts. On the Xerox copies, four of the five appeared with indistinguishable grey underlines. As a result, even K. Itô, the father of stochastic integration theory, would have found McKean's handouts a challenging exercise in cryptography. Thus, it should be no surprise that a couple of novices such as Varadhan and I would have been confused sufficiently to seek an alternative formulation of the whole subject. What is surprising is that, nearly thirty years later, our alternative has been deemed worthy of the Steele Prize.

Biographical Sketch of Daniel Stroock

Daniel Stroock received his A.B. from Harvard College in 1962 and did his doctoral research at Rockefeller University under the direction of Mark Kac, receiving his Ph.D. in 1966. From 1966 to 1972, he was at the Courant Institute of Mathematical Sciences at New York University, first as a postdoc and then as an assistant professor. In the fall of 1972 he decamped to the University of Colorado at Boulder, where he rose to the rank of professor before departing in the fall of 1984 for his present position at the Massachusetts Institute of Technology. Aside from the work with Varadhan which has been cited by the Steele Prize Committee, the accomplishment for which he is best known is the popularization of the name (if not the topic) that he called Malliavin's Calculus.

Response from Professor Varadhan

I want to thank the American Mathematical Society as well as the members of the Steele Prize committee for selecting me as a recipient this year. I am very pleased that my colleagues have chosen to single out some of my work with Dan Stroock in the late sixties as important. The Courant Institute, where most of the work was done, provided us with an ideal intellectual environment. We had the active encouragement and support of our senior colleagues, particularly Louis Nirenberg and Monroe Donsker. With the presence of Henry McKean and Mark Kac at Rockefeller, New York was indeed a very exciting place to be for an aspiring probabilist. Dan and I worked closely during this period, and to me it was very exciting and fruitful. I thank him, not just because he was a great person to work with, but for the years of close friendship as well. I am particularly pleased to be sharing this prize with him.

I was fortunate to have been a graduate student at the Indian Statistical Institute in Calcutta, which provided a very stimulating environment for my education. I want to express my appreciation to my advisor, C. R. Rao, and my colleagues V. S. Varadarjan, K. R. Parthasarathy, and R. Ranga Rao, from whom I learned a lot. Finally, I wish to express my thanks to my wife, Vasu, whose love and understanding have always been a source of strength to me.

Biographical Sketch of S. R. S. Varadhan

Srinivasa R. S. Varadhan received his B.Sc. degree from Presidency College, Madras (1959), and his Ph.D. from the Indian Statistical Institute (1963). Professor Varadhan began his academic career at the Courant Institute of Mathematical Sciences at New York University as a postdoctoral visitor (1963–66). At Courant he served as assistant professor (1966–68), associate professor (1968–72), and professor (1972–). He has twice served as director of the Institute (1980–84 and 1992–94). He has held visiting positions at Stanford University (1976–77), the Mittag-Leffler Institute (1972), and the Institute for Advanced Study (1991–92).

Professor Varadhan has been elected a member of the American Academy of Arts and Sciences (1988), the Third World Academy of Sciences (1988), and the National Academy of Sciences (1995), and was elected as Fellow of the Institute of Mathematical Statistics (1991). Professor Varadhan was an Alfred P. Sloan Fellow (1970–72) and a Guggenheim Fellow (1984–85). His awards and honors include the Birkhoff Prize (1994) and the Margaret and Herman Sokol Award of the Faculty of Arts and Sciences, New York University (1995).

Steele Prize for Lifetime Achievement: Goro Shimura

Citation

To Goro Shimura for his important and extensive work on arithmetical geometry and automorphic forms; concepts introduced by him were often seminal, and fertile ground for new developments, as witnessed by the many notations in number theory that carry his name and that have long been familiar to workers in the field.

Response

I always thought this prize was for an old person, certainly someone older than I, and so it was a surprise to me, if a pleasant one, to learn that I was chosen as a recipient. Though I am not so young, I am not so old either, and besides, I have been successful in making every newly appointed junior member of my department think that I was also a fellow new appointee. This time I failed, and I should be grateful to the selection committee for discovering that I am a person at least old enough to have his lifetime work spoken of.

There are many prizes conferred by various kinds of institutions, but in the present case, I view it as something from my friends, which makes me really happy. So let me just say thank you, my friends!

I would like to take this opportunity to give a historical perspective of a topic on which I worked in the 1950s and 1960s, intermingled with some of my personal recollections. It concerns arithmetic Fuchsian groups which can be obtained from an indefinite quaternion algebra B over a totally real algebraic number field F. For such a B one has

$$B \otimes_{\mathbf{Q}} \mathbf{R} = M_2(\mathbf{R})^r \times \mathbf{H}^{d-r},$$

where $d = [F : \mathbf{Q}]$, $0 \le r \le d$, $M_2(\mathbf{R})$ is the matrix algebra over $\mathbf{R}$ of size 2, and $\mathbf{H}$ is the Hamilton quaternions. Assuming $r > 0$ and taking a subring R of B that contains $\mathbf{Z}$ and spans B over $\mathbf{Q}$, denote by Γ the group of invertible elements of R whose projection to any factor $M_2(\mathbf{R})$ has determinant 1. Then we can view Γ as a subgroup of $SL_2(\mathbf{R})^r$ through the projection map to $M_2(\mathbf{R})^r$, and so we can let Γ act on the product H^r of r copies of the upper half plane H. In this way we obtain an algebraic variety $\Gamma \backslash H^r$, which is an algebraic curve if $r = 1$. It is known that $\Gamma \backslash H^r$ is compact if and only if B is a division algebra. In particular, we can take B to be the matrix algebra $M_2(F)$ over F of size 2, in which case $r = d$ and the meromorphic functions on $\Gamma \backslash H^d$ are called *Hilbert modular functions*.

If $F = \mathbf{Q}$, the group Γ was first discovered by Poincaré [7] "when he was walking on a cliff," apparently in 1886, as he reminisced in his *Science et Méthode*. One interesting aspect of this work is that the quotient $\Gamma \backslash H$ is compact if B is a division algebra. Until then the only Fuchsian groups he or anybody else knew were those obtained from hypergeometric series, among which the arithmetically defined ones were the classical modular groups; in all those cases the quotient is not compact. (Uniformization of an arbitrary compact Riemann surface was proved independently by Koebe and Poincaré only in 1907.) Poincaré's group was generalized to the case $1 = r \le d$ with an arbitrary F by Fricke [3] in 1893. It is also discussed in the last chapter of the thick volume [4] of Fricke and Klein published in 1897. These mathematicians employed an indefinite ternary quadratic form instead of a quaternion algebra. Since $SO(2, 1)$ is covered by $SL_2(\mathbf{R})$, the unit group of the given ternary form produces a discrete subgroup of $SL_2(\mathbf{R})$.

After Fricke's investigations, which showed that the action of the groups on H is properly discontinuous, no significant progress was made in this area for the next fifty years. In 1912 Hecke published his thesis work [5] concerning Hilbert modular functions in the case of $M_2(F)$ with $d = 2$. In its introduction he said that the results of Fricke on the Fuchsian groups of the above type seemed to be "without specific meaning in number theory". Later developments proved that he was wrong. Taking his tender age of 25 into consideration, we may forgive him and may even justify his comment, allowing him a 30-year warranty, since it could apply to all papers on this subject in that period—one by Heegner [6] for example, which I cite here in order to show that the topic was not forgotten, but was being treated without any new ideas. It should also be pointed out that Hecke's own work was critically flawed, though generally speaking he was headed in the right direction, except for that comment.

Eichler may have been the first person who was seriously interested in this group. He wrote his dissertation with Brandt on quaternion algebras and later worked on more general types of simple algebras. He once told me that Brandt did not think much of nonquaternion algebras and was unhappy with Eichler's turning to them. In reality, there was no need for him to be unhappy, since the fact that Eichler started with quaternion algebras determined his course thereafter, which was vastly successful. In a lecture he gave in Tokyo he drew a hexagon on the blackboard and called its vertices clockwise as follows: automorphic forms, modular forms, quadratic forms, quaternion algebras, Riemann surfaces, and algebraic functions. Anyway, in the mid-1950s Eichler was developing the theory of Hecke operators for the Fuchsian groups of Poincaré's type (see [1], for example). He also gave a formula for the genus of $\Gamma \backslash H$ somewhat earlier. However, there were no other number-theoretical investigations on these algebraic curves by that time.

In 1957 while in Paris I became interested in this class of groups. I had just finished my first work on the zeta functions of elliptic modular curves. Though I knew that it needed elaboration, I was more interested in finding other curves whose zeta functions could be determined. I was also trying to formulate the theory of complex multiplication in higher dimension in terms of the values of automorphic functions of several variables—Siegel modular functions, for example. It turned out that these two problems were inseparably connected to each other. Also, nobody else was working on such questions. I can assure the reader that I had no intention of humiliating Hecke posthumously.

So I took up the group of the above type. My aim was to find an algebraic curve C defined over an algebraic number field k that is complex analytically isomorphic to $\Gamma\backslash H$ and to determine the zeta function of C. Such a C is called a *model of* $\Gamma\backslash H$ *over* k. Naturally I started with the simplest case, $F = \mathbf{Q}$. Since it was relatively easy to see that $\Gamma\backslash H$ in this case parametrizes a family of certain two-dimensional abelian varieties, I was soon able to prove that the curve had a $\mathbf{Q}$-rational model. The proof required a theory of the field of moduli of a polarized abelian variety, but luckily I had it at my disposal, since I had been forced to develop such a theory in order to get a better formulation of complex multiplication, as mentioned above. In June 1958 I visited three schools in Germany: Münster, Göttingen, and Marburg. I gave a talk at each place, but remember only that at Göttingen I spoke about the field of moduli of a polarized abelian variety and its application to the field of definition for the field of automorphic functions. At the end of my talk I mentioned briefly the $\mathbf{Q}$-rationality of the curve $\Gamma\backslash H$ for Poincaré's Γ.

Siegel was among the audience, and pressed on the last point. I began to explain the idea, but he interrupted me and simply wanted to know whether I really had the proof. So I said, "Yes," and that was that. Siegel said nothing, but apparently he was not convinced and expressed his doubts to Klingen, who in 1970 told me about Siegel's skepticism at that time. I can easily guess the rationale behind his disbelief: Since $\Gamma\backslash H$ is compact, there is no natural Fourier expansion of an automorphic form, so there is no way of defining the rationality of automorphic functions, and that was exactly why Hecke made the comment mentioned above. Eventually I determined the zeta function of the curve and gave a talk at the ICM, Edinburgh, in September 1958. The full details were published in [8] in 1961.

There was no such incident at Marburg, where I met Eichler. I remember that after dinner at his home, he played a religious piece of music on the phonograph, which I think was by Bach. I am sure it was not by Mozart, as he did not think much of the composer. He was a tall and handsome man, whose look immediately reminded me of the knight in the movie "The Seventh Seal" by Ingmar Bergman, which I had seen a few months earlier. As for Siegel, who was 61 at that time, calling him a big mass of flesh would have been misleading and even derogatory, but that was my first impression. Though he must have looked awesome to many, he assumed no airs, and there was a certain homely atmosphere around him, which made him less intimidating, at least to me.

Coming back to my work, at first I thought that these curves obtained from a division quaternion algebra B over $\mathbf{Q}$ might not be modular, (and strictly speaking, that is true; see the next paragraph), but I realized that *no nonmodular* $\mathbf{Q}$*-rational elliptic curves could be obtained* for the following reason: Eichler had shown, by means of his trace formula, the following fact: the Euler products on B are already included in those obtained from elliptic modular forms [2]. The Tate conjecture on this was explicitly stated much later, but the idea was known to many people, and so it was natural for me to think that two elliptic curves with the same zeta function are isogenous. This fact concerning B, in addition to the results I had about the zeta functions of modular curves, may have been the strongest reason for my stating the conjecture that every $\mathbf{Q}$-rational elliptic curve is modular.

Let me insert here a remark on the curves obtained from a division algebra B. I showed much later in [11] that the natural models of the curves have *no real points* even when the genus of $\Gamma\backslash H$ is 1, and in that sense they are not modular! They are not *elliptic curves* in the strict sense, though their jacobian varieties are. This point may explain the raison d'être of those curves. I wonder if there is any recent investigation on this phenomenon.

The curves with $F \neq \mathbf{Q}$ were more difficult. After going back to Tokyo in the spring of 1959, I decided to investigate more general families of abelian varieties. By specifying the types of endomorphism algebra and polarization of abelian varieties, one obtains a quotient $\Delta\backslash S$ that parametrizes abelian varieties of a prescribed type, where S is a hermitian symmetric domain of noncompact type, and Δ is an arithmetic subgroup of a certain algebraic group. The above $\Gamma\backslash H$ for Poincaré's Γ is the easiest example of $\Delta\backslash S$; one simply takes B to be the endomorphism algebra. For certain reasons, however, the algebra B with $0 < r < d$ never appears as the endomorphism algebra of an abelian variety, which was the main difficulty. Then I realized that by choosing an algebra different from B, one obtains $\Delta\backslash S$ that is essentially the same as $\Gamma\backslash H$ for an arbitrary B of the above type. I think that was sometime in the fall of 1960. I knew at that point that the problem was approachable, and even knew that the curves had models over a number field, but did not know how to state the theorems in the best possible form, not to mention how to prove them.

In a series of papers published in 1963–65 I investigated the number fields over which the varieties $\Delta\backslash S$ can be defined. In many higher-dimensional cases, the results were best possible, but in the one-dimensional case that was the main question, I was not satisfied. So I turned to a higher-dimensional case of a different nature. In a famous paper on symplectic geometry [12] Siegel defined a certain arithmetic subgroup Γ' of $Sp(n, \mathbf{R})$ which was a generalization of Fricke's group and which was also defined relative to F. If $n > 1$ and $F \neq \mathbf{Q}$, this group does not appear as the above group Δ associated with a family of abelian varieties. But in the summer of 1963, while in Boulder, Colorado, I found that there was an injection $\Gamma' \to \Delta$ with some Δ, which produced a holomorphic embedding $\Gamma'\backslash S' \to \Delta\backslash S$, where S' is the Siegel upper half space of degree n. If $n = 1$, $\Gamma'\backslash S'$ is exactly the algebraic curve $\Gamma\backslash H$ in question, and, moreover, the embedding is essentially birational over $\mathbf{C}$. Anyway, employing this embedding, I was able to find a number field over which $\Gamma'\backslash S'$ is defined for an arbitrary n. When I was asked to contribute a paper to the volume in honor of Siegel's 70th birthday, I naturally took this as the topic and sent the manuscript to the editor in the fall of 1965.

Around the same time, perhaps in early September that year, I finally had a definite idea of settling the original question in the one-dimensional case: to employ many different $\Delta\backslash S$ for a given $\Gamma\backslash H$. By means of this idea together with a finer theory of variety of moduli of polarized abelian varieties, by June 1966 I was able to finish the paper [9] in which I determined the zeta function of the curve $\Gamma\backslash H$ with any totally real F. At the same time I determined the class fields generated by the values of automorphic functions, not only in the one-dimensional case, but also in the case where B is totally indefinite, including the Hilbert modular case. By doing so I showed that similar theories could be developed in a parallel way in both Fricke's and Hecke's cases. In fact, those are the two extreme cases of a more general class of arithmetic quotients for which one can do number-theoretical investigations Hecke wished to do in his case, a fact Hecke never realized.

I dedicated the paper to Weil. At some point I said to him jokingly that he became sufficiently old that I could now dedicate a paper to him, to which he replied, "I can't stop it." Meanwhile my paper dedicated to Siegel appeared in the *Mathematische Annalen* [10]; I also sent a reprint of my *Annals* article to him, as I had been doing regularly with my earlier papers. Here is what he wrote me about these:

> Göttingen, 15 May 1967
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> Dear Professor Shimura:
>
> After a long trip around the world I returned to Göttingen and I found your last paper from the *Annals of Mathematics* together with the work which you kindly dedicated on the occasion of my 70$^{\text{th}}$ birthday.
>
> I am sending you my most cordial thanks for your kindness. I have now begun to study these two papers, and both of them seem to be of great interest, from the arithmetical and the analytical point of view.
>
> During many years I have regretted that Hecke's earlier work on Hilbert's modular function and class field theory had not been continued by later mathematicians. I am glad to see in your last paper how much you have already achieved in this direction.
>
> I was very pleased to see from your other paper that you have obtained decisive results concerning those groups which I introduced in my paper on symplectic geometry.
>
> Best congratulations for the success of your previous work, and best wishes for the future!
>
> Yours sincerely
>
> Carl Ludwig Siegel

I was naturally gratified and even moved, but frankly I was somewhat disappointed by his mentioning only the Hilbert modular case, which was far easier than the case of curves that was the main feature of my paper. Therefore, I was not sure whether he perceived the full scope of the work. Perhaps he thought what he said was enough, which is true, and so I should not complain. In fact, reading this letter after almost thirty years, I now think that the letter tells more about the sender than about the recipient.

To clarify this point, we have to know what kind of a man Siegel was. Of course, he established himself as one of the giants in the history of mathematics long ago. He was not known, however, for his good-naturedness. Around 1980 I sat next to Natasha Brunswick at a dinner table, when she proclaimed, "Siegel is mean!" I don't remember how our conversation led to that statement, but many of those who knew him would agree with her opinion. Hel Braun, one of his few students, apparently disliked him. He was indisputably original, and even original in his perverseness. Once at a party he played a piano piece and challenged the audience to tell who the composer was. Hearing no answer, he said it was a sonata by Mozart, Köchel number such and such, played backward. On the other hand, he had a certain sense of humor. When Weil asked him which work of his he thought best, he replied, "Oh, I think a few watercolors I made in Greece some years ago are pretty good."

In any case, it would be wrong to presume him to be a mathematician who did what he wanted to do, unconcerned about what other people might think of his work. I believe he was not that aloof. He must have known who he was, but at the same time he must have felt unappreciated by the younger generation. That was Eichler's opinion, and I am inclined to agree with him.

After his retirement Siegel took a long trip around the world, as he mentions in his letter. On coming back to Göttingen, one day he went into his office in the university and found on his desk a copy of the volume of the *Mathematische Annalen* dedicated to him, which pleased him greatly. And here was a man 34 years younger than he, completely outside of his German influence, who took up the topic on which he expended considerable effort many years ago, with genuine appreciation of his work.

Perhaps he was not so crabbed as many people had imagined, and it is possible that he wrote a few more letters like the above one. At any rate, when he wrote that letter, he knew that at least one of his papers was really understood, and at that moment he was capable of appreciating the progress made by the new generation, of which he had often been contemptuous. I am indeed glad to be the recipient of the letter which showed this great mathematician as a warm-hearted man with no trace of ill-temperedness, nor any cynicism.

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Biographical Sketch

Goro Shimura was born on February 23, 1930, in Hamamatsu, Japan. He received his B.S. (1952) and D.Sc. (1958) from the University of Tokyo. During his academic career, Professor Shimura served as lecturer at the University of Tokyo (1954), as professor at Osaka University (1961), and as professor at Princeton University (1964–). He was a Guggenheim Fellow (1970–71).