Armand Borel (1923–2003)

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Armand Borel

Institute for Advanced Study, 1999.

Citations within this article.
Borel's *Collected Papers* are [Œ], and his 17 books are referred to as [1] through [17]. These are all listed in a sidebar on page 501. An item like [Œ 23] is Borel paper number 23 in [Œ]. Citations of work by people other than Borel are by letter combinations such as [Che], and the details appear at the end of the article.

Jean-Pierre Serre

The Swiss mathematician Armand Borel died August 11, 2003, in Princeton from a rapidly evolving cancer. Few foreign mathematicians had as many connections with France. He was a student of Leray, he took part in the Cartan seminar, and he published more than twenty papers in collaboration with our colleagues Lichnerowicz and Tits, as well as with me. He was a member of Bourbaki for more than twenty years, and he became a foreign member of the Académie des Sciences in 1981. French mathematicians feel that it is one of their own who has died.

He was born in La Chaux-de-Fonds in 1923 and was an undergraduate at Eidgenössische Technische Hochschule of Zürich (the "Poly"). There he met H. Hopf, who gave him a taste for topology, and E. Stiefel, who introduced him to Lie groups and their root systems. He spent the year 1949–50 in Paris, with a grant from the CNRS[1]. A good choice (for us, as well as for him), Paris being the very spot where what Americans have called "French Topology" was being created, with the courses from Leray at the Collège de France and the Cartan seminar at the École Normale Supérieure. Borel was an active participant in the Cartan seminar while closely following Leray's courses. He managed to understand the famous "spectral sequence", not an easy task, and he explained it to me so well that I have not stopped using it since. He began to apply it to Lie groups, and to the determination of their cohomology with integer coefficients. That work would make a thesis, defended at the Sorbonne (with Leray as president) in 1952, and published immediately in the *Annals of Mathematics*. Meanwhile Borel returned to Switzerland. He did not stay long. He went for two years (1952–54) to the Institute for Advanced Study in Princeton and spent the year 1954–55 in Chicago, where he benefited from the presence of André Weil by learning algebraic geometry and number theory. He returned to Switzerland, this time to Zürich, and in 1957 the Institute for Advanced Study offered him a position as permanent professor, a post he occupied until his death (he became a professor emeritus in 1993).

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[1] *Centre National de la Recherche Scientifique.*

He was a member of the academies of sciences of the USA and of Finland. He received the Brouwer Medal in 1978, the AMS Steele Prize in 1991, and the Balzan Prize in 1992. His *Œuvres* have been collected in four volumes, published by Springer-Verlag in 1983 (volumes I, II, III) and in 2001 (volume IV).

Borel's works have a profound unity: they relate in almost every case to *group theory*, and more particularly to Lie groups and algebraic groups. The central nature of group theory has been known for a long time. In [Œ IV, p. 381] Borel quoted a sentence from Poincaré, dating from 1912, saying this: "Group theory is, so to speak, all of mathematics, stripped of its content and reduced to a pure form."[2]

We today would no longer employ terms so extreme: "all of mathematics" seems too much, and "stripped of its content" seems unfair. Nevertheless the importance of group theory is much more evident now than in Poincaré's day, and that is true in areas as different as geometry, number theory, and theoretical physics. It seems that, in his youth, Borel consciously made the decision to explore and go further into everything related to Lie theory; and this is what he did, during nearly sixty years.

I am not going to try to make an exhaustive list of the results he obtained. I shall confine myself to what I know best.

Topology of Lie Groups and of Their Classifying Spaces

As said above, this is the subject of his thesis ([Œ 23]). The objective is the determination of the cohomology with coefficients in the integers $\mathbb{Z}$ (torsion included) of compact Lie groups and of their classifying spaces. Borel uses Leray's theory, and sharpens it by proving a difficult result of the type:

"exterior algebra (fiber) $\Longrightarrow$
polynomial algebra (base)."

(The proof is so intricate that, according to Borel, "I do not know whether it has had any serious reader other than J. Leray and E. B. Dynkin.")

This led to the introduction of the "torsion primes" of a compact Lie group G (for example, 2, 3, and 5 for G of type E_8). He showed that these primes play a special role in the structure of the finite commutative subgroups of G ([Œ 24, 51, 53]). It has since been found that they also occur in the Galois cohomology of G, and in particular in the theory of the "essential dimension".

[2] *"La théorie des groupes est, pour ainsi dire, la mathématique entière, dépouillée de sa matière et réduite à une forme pure."*

Photograph by Dominique Borel.

Borel with Jean-Pierre Serre (left), Princeton, 1963. Over the years, a great many mathematicians enjoyed the Borels' hospitality in Princeton.

Linear Algebraic Groups

His article on this subject ([Œ 39]) played a fundamental role (it in particular served as the point of departure for the classification by Chevalley [Che] of semisimple groups in terms of root systems). In it Borel established the main properties of maximal connected solvable groups (now called "Borel subgroups") and of maximal tori. The proofs are astonishingly simple; they rest in great part on a lemma saying that every linear connected solvable group that acts algebraically on a nonempty projective variety has a fixed point.

The point of view of [Œ 39] is "geometric": the given group G is defined over a ground field k that is assumed algebraically closed. The same assumption occurs in [Che]. The case of a field that is not algebraically closed is however of great interest, as much for geometers (É. Cartan, for $k = \mathbb{R}$) as for number theorists (k = number field, or p-adic field). Borel (and, independently, Tits) constructed a "relative" theory, based on maximal split k-tori and the corresponding root systems. Borel and Tits published their results together ([Œ 66, 94]); the theory obtained in this way carries their name today; it is invaluable as long as the group, assumed simple, is isotropic, that is, contains nontrivial unipotent elements. (The anisotropic case is in the domain of "Galois cohomology", and is still not completely understood.) Borel and Tits completed their results by describing the homomorphisms that are not necessarily algebraic (called, curiously, "abstract") between groups of the form $G(k)$, cf. [Œ 82, 97].

Arithmetic Groups, Stability, Representations, …

It is to Borel and Harish-Chandra that we owe the basic results on *arithmetic subgroups* of reductive groups over number fields: finite generation, cocompactness in the anisotropic case, finite covolume in the semisimple case, cf. [Œ 54, 58]. These results have great importance for number theory.

Editor's Note:

The accompanying article shows some of the many sides of Armand Borel, who died August 11, 2003. The eight authors write about him in the order Serre, Chandrasekharan, Bombieri, Hirzebruch, Springer, Tits, Arthur, Prasad.

Borel's research in algebra and topology was good enough to get him appointed professor at the Institute for Advanced Study at age thirty-four. Serre gives an overview of the mathematics, and Hirzebruch describes that mathematics in a different way, with emphasis on the topology. Springer and Tits write about Borel's work in algebraic groups, and Arthur writes about arithmetic groups and about how Borel's work in this area laid the foundations of the modern theory of automorphic forms.

An Institute colleague said that Borel believed strongly in the unity of mathematics and in the importance of the written record. The means for acting on these beliefs included roles as editor, author, educator, and conference organizer, and some of the present authors have elaborated on these activities. Borel put a sizable effort into contributions to the volumes by Bourbaki, detailing his experiences with that group in an article "Twenty-five Years with Nicolas Bourbaki, 1949–1973" in the *Notices* in March 1998 [Œ 165]. He is widely regarded as having played a major role in the writing of the Bourbaki chapters on Lie groups and Lie algebras, which have been of particularly enduring value.

Borel was an editor of *Annals of Mathematics* for 1962–79 and of *Inventiones Mathematicae* for 1979–93, among other journals. For the interval 1998–2000 he served quietly as a kind of unofficial associate editor for the *Notices*, advising the editor on various matters, particularly memorial articles and the collaboration of the *Notices* with its counterparts in other countries. Borel played a large but anonymous role in planning the various articles about A. Weil and the memorial articles for J. Leray and A. Lichnerowicz.

Some of the authors of the present article describe some of the books that Borel authored or edited. The list of such books has seventeen entries, apart from his *Œuvres: Collected Papers* [Œ], and appears in a sidebar with this article. A number of these books are outgrowths of seminars, sometimes joint with other people and sometimes not. No matter what form the seminars took, one can be confident that Borel was the choreographer of each. Of special note are the proceedings from two AMS summer institutes, in Boulder [3] in 1965 and in Corvallis [10] in 1977. Each proceedings contains significant expositions by Borel and contributions by many other experts; each has become a basic reference in its field.

In his last few years Borel kept up an annual schedule at the Institute in the winter, the Far East in the spring, and Switzerland in the summer. For each year 1999–2001 he was the organizer of a program on Lie theory at Hong Kong University from March to July. He had planned a summer school at Zhejiang University in Hangzhou, China, with Lizhen Ji and S.-T. Yau for August 2003, with continuations in 2004 and 2005 and plans for publishing the proceedings. The summer school for 2003 took place as planned, but Borel was unable to attend.

Borel was a counselor to mathematicians young and old. He had a fearsome reputation, and making a first approach to him was not for the faint-hearted. Yet many stories have come out since his death about how he helped individual mathematicians in large and small ways. All a person had to do was ask, and suddenly the effect of Borel's personality completely changed. Prasad writes how responsive Borel was to inquiries about mathematics from anyone anywhere in the world.

How did this man maintain his creative spirit, his energy, and his enthusiasm for so long? His older daughter, Dominique, said of him that he approached each new thing in his life, mathematical or otherwise, with the attitude of wonder and excitement of a small child. The citation for his AMS Steele Prize for Lifetime Achievement, which he was awarded in 1991, concluded with the following comments on Borel's activities beyond research: "In the course of amassing these astounding achievements, he placed the facilities of the Institute for Advanced Study at the service of mathematics and mathematicians, using them to foster talent, share his ideas, and facilitate access to recent developments through seminars and lectures. It is just simply not possible to cite a career more accomplished or fruitful or one more meaningful to the contemporary mathematical community."

Borel completed them in a series of papers ([Œ 59, 61, 70, 74, 88, 99]), as well as in [2]. Several themes are intertwined:

- Compactification of quotients: that of Baily–Borel ([Œ 63, 69]) in the complex analytic case; that of [Œ 90, 98] in the real case, using manifolds with corners. In the two cases, it is the Tits building of the group that dictates what has to be added "at infinity".
- Generalization to S-arithmetic groups and to adelic groups ([Œ 60, 91, 105]); here the use of the Tits building must be completed by that of Bruhat–Tits buildings at nonarchimedean places.
- Infinite-dimensional representations, and the Langlands program: [8], [12], and [Œ 103, 106, 112].
- Relations between the cohomology of arithmetic groups and that of symmetric spaces.

This last theme leads Borel to one of his most beautiful results: a stability theorem ([Œ 93, 100, 118]) that gives the determination of the ranks of the *K-theory groups of* $\mathbb{Z}$ (and, more generally, of the ring of integers of any number field). This leads him to the definition of a *regulator*, which he shows is essentially a *value of a zeta function* at an integer point ([Œ 108]).

History of Mathematics

In the last ten years of his life, Borel published a series of articles of a kind at once historical and mathematical on the following topics:

- Topology: on D. Montgomery ([Œ 357]), J. Leray ([Œ 164]), and A. Weil ([Œ 168]).
- Group theory: on H. Weyl ([Œ 132]), C. Chevalley ([Œ 143]), and E. Kolchin ([Œ 171]).
- Special relativity ([Œ 173]).

Some of his papers have been reprinted, and completed, in the last book that he published, his *Essays in the History of Lie Groups and Algebraic Groups* ([15]). It is a fascinating book, which leads us through a century of group theory, from Sophus Lie to Chevalley, past É. Cartan and H. Weyl. A "guided tour", with such a guide, what a pleasure!

The work of Borel is not at all limited to the texts that I have just invoked. He was an enthusiastic organizer. We owe to him several particularly successful seminars and summer schools, notably on group actions ([2]), algebraic groups ([8], [3]), continuous cohomology ([11]), modular forms ([10]), and D-modules ([12]). I wish to mention also his contribution to Bourbaki (from 1950 until his retirement in 1973, cf. [Œ 165]), to which he brought both common sense and expertise. The chapters on Lie groups ([LIE]) owe a great deal to him.

Borel received the Balzan Prize in 1992, with the following citation: *"For his fundamental contributions to the theory of Lie groups, algebraic groups and arithmetic groups, and for his indefatigable action in favour of high quality in mathematical research and of the propagation of new ideas."*

One would not know how to say it better.

Komaravolu Chandrasekharan

Armand Borel was wont to sojourn in far-off lands, spreading his message of mathematics. He was supposed to be in rude health. And suddenly he is no more. In Auden's phrase, he has become his admirers.

He was nonpareil as an algebraist, with wide horizons. One has just to look at his article on André Weil's contributions to topology. His brilliance is in his refusal to distinguish between fun and learning. His visits to India and China provide instances. But his overriding enthusiasm for his subject swept aside all distractions.

He had homes in the U.S. and in Switzerland. But John Steinbeck's words apply to him: "I have no 'place' home, I have homes everywhere, and many I have not even seen yet."

It was Warren Ambrose who first alerted me to his work in 1954. An invitation to India followed, which he could not immediately accept, but in which he expressed continued interest. He became a full professor at the ETH Zürich, his alma mater, in 1955, whence he moved to the Institute in Princeton in 1957, following the trail once blazed by Albert Einstein and Hermann Weyl. True to his expressed intention, he came to Bombay in 1961. His lectures there fell on fertile ground, as his many subsequent visits testify. His love of jazz kindled in him an interest in Carnatic music, with its syncopated rhythms and melodic improvisations, which grew into a passion. I treasure the memory of many shared moments of joy.

On the initiative of Georges de Rham, the Swiss federal authorities tried for years without success to attract him home. Armand's response was: "If Barry Goldwater becomes president, then maybe." His attitude changed somewhat after Jürgen Moser moved to Zürich. The determined initiative of President H. Ursprung of the ETH Zürich resulted in his acceptance of a professorship (15 April 1983 to 1 July 1986). He made it clear that he was not used to supervising theses for the doctorate or the diploma. It was suggested to him that he might inaugurate a new series, called the Swiss Seminars in Mathematics, jointly with colleagues from the French-speaking part of the country, and hold them in a central place like Bern, which he did with conspicuous success. They were subsequently renamed "Borel Seminars"; they are Armand's legacy to Switzerland.

Photograph courtesy of Armand Borel's family.

Armand and Gaby Borel aboard the liner *Ile de France* on their way to the Institute for Advanced Study for the first time, September 1952.

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Bill Casselman assembled the photographs and prepared the captions and the figure for this article. He is the graphics editor of the Notices. *His email address is* `notices-covers@ams.org`.

The Notices *expresses its deep appreciation to Anthony W. Knapp for organizing this article and to Dominique Borel for help in assembling the photographs.*

His personality might have seemed dour to those who did not know him well; they could not sense the soft core underneath, nourished and sustained by his devoted wife, Gabrielle. He had a social conscience and human sympathy for the predicament of the poor and disadvantaged. On his first visit to India, he and Gaby sponsored the education of a "street urchin" into adulthood and self-sufficiency. He detested the display of self-importance or officiousness in any form and did not hide his displeasure even if a close colleague indulged in it. He had a highly developed sense of the absurd, which moved him to outright laughter when faced with people who spoke or wrote about things they did not know. In his early years as a referee or editor, he tended to be impatient, but his use of the word "inutile" became less frequent with the passage of time.

Unforgettable is the midnight session of jazz in Chicago (with saxophonist Sonny Stitt), which we together enjoyed *after* the conclusion of the symposium in honour of Marshall Stone (May 1968) and *all* the associated festivities. A tape of Armand's centennial lecture on "Hermann Weyl and Lie groups" (November 1985) remains a prized memento. As I listen to it again, I wonder if Armand's delight in the continual and indefatigable generation of knowledge did not exceed the short vehemence of any temporal pleasure.

Enrico Bombieri

Armand Borel, professor emeritus in the School of Mathematics at the Institute for Advanced Study since 1993, died at home in Princeton on August 11, 2003, only two months after the first symptoms of a terminal illness appeared. He had celebrated his eightieth birthday on May 21.

Borel was born in 1923 in the French-speaking city of La Chaux-de-Fonds in Switzerland. He soon distinguished himself as an exceptional student and graduated in 1947 from the Swiss Federal Institute of Technology[3] in Zürich, where he was introduced to the study of topology and Lie groups by the famous mathematicians Heinz Hopf and Eduard Stiefel. He immediately obtained a position as assistant at the same institution, which he held for two years, and then, with a research grant from the French CNRS,[4] he moved to Paris for the 1949–50 year. This was a turning point in his mathematical development. There he quickly got acquainted with senior members of the Bourbaki group—namely Henri Cartan, Jean Dieudonné, Laurent Schwartz—and with the younger members—notably Roger Godement, Pierre Samuel, Jacques Dixmier, and most importantly Jean-Pierre Serre, who became a close friend and collaborator of Borel. The discussions with these mathematicians had a lasting influence on Borel and completed his preparation. He joined the Bourbaki group in the same year.

Borel returned to Switzerland with a position as adjunct professor of algebra at the University of Geneva from 1950 to 1952. In these years he completed the write-up of his thesis for a Doctorat d'État and defended it at the Sorbonne in Paris. His thesis, of fundamental importance in the theory of Lie groups, was published without delay in the prestigious journal *Annals of Mathematics*.

The same year, with his thesis as his entry card, Borel arrived with his young bride, Gaby, at the Institute as a member of the School of Mathematics. His membership in the School was renewed for a second year (at that time renewal of membership was done almost automatically, Borel told me, adding that he thought it was a very good thing). Then he spent a year in Chicago, where he profited highly from the presence of André Weil, thus adding algebraic geometry and number theory to his already vast knowledge of algebra and topology.

In 1957 he joined the School of Mathematics at the Institute as a professor, remaining until his retirement in 1993. At the time of his death he had authored or edited 16 books and over 180 papers and was working on a major monograph [17] in collaboration with Lizhen Ji of the University of Michigan at Ann Arbor on the subject of compactifications of homogeneous spaces. He became a U.S. citizen on February 18, 1986.

He was a member of the National Academy of Sciences of the USA and of the American Academy of Arts and Sciences; a foreign member of the Finnish Academy of Sciences and Letters, of the American Philosophical Society, and of the Academia Europæa; a foreign associate of the French Academy of Sciences; an honorary fellow of the Tata Institute in Bombay, India; and the laureate of an honorary doctorate from the University of Geneva. He was a recipient of the Brouwer Medal of the Dutch Mathematical Society, of the Steele Prize of the American Mathematical Society, and of the Balzan Prize of the Italian-Swiss International Balzan Foundation.

His scientific activities, besides research, involved participation in the Consultative Committees of the International Congresses of Mathematicians in 1966 and 1978, participation in the editorial boards of the most prestigious mathematical journals in a span of over thirty years, and also teaching (I can mention various summer

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[3] *Eidgenössische Technische Hochschule.*

[4] *Centre National de la Recherche Scientifique.*

Books by Armand Borel

[Œ] *Œuvres: Collected Papers*, Springer-Verlag, Berlin; vol. I, II, III, 1983; vol. IV, 2001.

[1] *Cohomologie des Espaces Localement Compacts, d'après J. Leray*, École Polytechnique Fédérale, Zürich, 1953; 2nd edition, 1957; 3rd edition, Lecture Notes in Math., vol. 2, Springer-Verlag, Berlin, 1964.

[2] *Seminar on Transformation Groups*, Annals of Math. Stud., vol. 46, Princeton University Press, Princeton, NJ, 1960.

[3] (Edited with G. D. Mostow) *Algebraic Groups and Discontinuous Subgroups*, Proc. Sympos. Pure Math., vol. IX, American Mathematical Society, Providence, RI, 1966.

[4] (Written with S. Chowla, C. S. Herz, K. Iwasawa, J-P. Serre) *Seminar on Complex Multiplication*, Lecture Notes in Math., vol. 21, Springer-Verlag, Berlin, 1966.

[5] *Topics in the Homology Theory of Fibre Bundles*, Lecture Notes in Math., vol. 36, Springer-Verlag, Berlin, 1967.

[6] *Introduction aux groupes arithmétiques*, Actualités Sci. Indust., vol. 1341, Hermann, Paris, 1969.

[7] *Linear Algebraic Groups*, W. A. Benjamin, Inc., New York, 1969; 2nd edition, Grad. Texts Math., vol. 126, Springer-Verlag, New York, 1991.

[8] (Written with R. Carter, C. W. Curtis, N. Iwahori, T. A. Springer, R. Steinberg) *Seminar on Algebraic Groups and Related Finite Groups*, Lecture Notes in Math., vol. 131, Springer-Verlag, Berlin, 1970.

[9] *Représentations de Groupes Localement Compacts*, Lecture Notes in Math., vol. 276, Springer-Verlag, Berlin, 1972.

[10] (Edited with W. Casselman) *Automorphic Forms, Representations and L-Functions*, Proc. Sympos. Pure Math., vol. XXXIII, Parts I and II, American Mathematical Society, Providence, RI, 1979.

[11] (Written with N. R. Wallach) *Continuous Cohomology, Discrete Subgroups, and Representations of Reductive Groups*, Ann. of Math. Stud., vol. 94, Princeton University Press, Princeton, NJ, and University of Tokyo Press, Tokyo, 1980; 2nd edition, Math. Surveys Monogr., vol. 67, American Mathematical Society, Providence, RI, 2000.

[12] (Written with P.-P. Grivel, B. Kaup, A. Haefliger, B. Malgrange, F. Ehlers) *Algebraic D-Modules*, Perspect. Math., vol. 2, Academic Press, Inc., Boston, MA, 1987.

[13] *Automorphic Forms on* $SL_2(\mathbf{R})$, Cambridge Tracts Math., vol. 130, Cambridge University Press, Cambridge, 1997.

[14] *Semisimple Groups and Riemannian Symmetric Spaces*, Texts Read. Math., vol. 16, Hindustan Book Agency, New Delhi, 1998, distributed by American Mathematical Society, Providence, RI.

[15] *Essays in the History of Lie Groups and Algebraic Groups*, History Math., vol. 21, American Mathematical Society, Providence, RI, and London Mathematical Society, Cambridge, 2001.

[16] (Written with R. Friedman and J. W. Morgan) *Almost Commuting Elements in Compact Lie Groups*, Mem. Amer. Math. Soc., vol. 157, American Mathematical Society, Providence, RI, 2002.

[17] (Written with Lizhen Ji) *Compactifications of Symmetric and Locally Symmetric Spaces*, Birkhäuser, Boston, to appear.

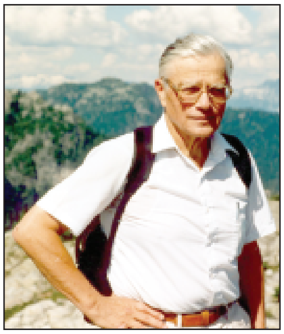
schools on mathematical topics at a high level and a three-year program in Hong Kong in his last years).

Less obvious, but not less important, was his presence in the Bourbaki group. The Bourbaki group was founded in 1934 by a small group of young French mathematicians, with the purpose of writing *ex novo* the foundations of modern mathematics in a correct and coherent fashion. Among these "young Turks" was André Weil, later mentor of Borel in Chicago and professor at the Institute. Their work was published anonymously under the pseudonym of "Bourbaki", a name borrowed from the French general with the Army of Napoleon III operating in Italy. The Italian school of algebraic geometry had produced a great body of fundamental work, but its foundations were indeed quite shaky and in need of drastic revision, so the comparison of the fictitious mathematician Bourbaki with the real general Bourbaki at war with Italy was not inappropriate. The influence of Bourbaki in the development of twentieth-century mathematics cannot be overestimated: its axiomatic approach, its quest for general statements, and its absolute mathematical rigor have been a model for many decades, and its texts are basic references. Borel was a member of this group from 1949 to 1973 (fifty is the mandatory retirement age for membership in Bourbaki).

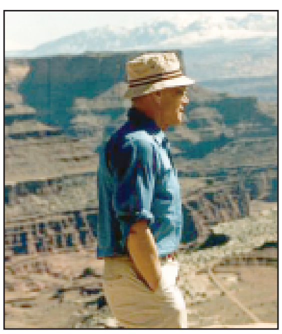
I have mentioned topology and Lie groups (the name is from the Norwegian mathematician Sophus Lie) as the main subjects of research by Borel. Topology at its simplest is the study of geometric shapes under continuous deformations, namely without jumps or breaks; a Lie group (and I hope not to raise the disapproval of my mathematical colleagues in my gross oversimplification) can be seen as formed by continuous transformations of a highly symmetric object. An example is formed by the rotations of a sphere. Topology and Lie theory are a big part of the backbone of mathematics (Borel would say they *are* the backbone of mathematics). The contributions of Borel in the field will remain in the history of the subject. The citation of the Balzan Prize could not be more appropriate: "For his fundamental contributions to the theory of Lie groups, algebraic groups and arithmetic groups, and for his indefatigable action in favour of high quality in mathematical research and of the propagation of new ideas."

Borel's view of mathematics is very interesting, and I will spend a few words by reading an excerpt from his response[5] in receiving the Balzan Prize.

"Mathematics is a gigantic intellectual construction, very difficult, if not impossible, to view in its entirety. Sometimes I like to compare it to an iceberg, because it has a small visible part and a large invisible part. By visible part I mean the mathematics useful in the external world, in technology, physics, natural sciences, astronomy, computers, and so on, whose usefulness and social justification cannot be doubted. Indeed, it is certain that practical problems in ancient times were at the very origin of mathematics. However, with the development of mathematics the subject acquired a life of its own and mathematicians became more and more interested in purely mathematical problems, not necessarily paying attention to applications outside mathematics itself. This forms the invisible part of the iceberg; I mean invisible or at least very difficult to grasp for the nonmathematician, the part that is pure mathematics.



At the top of a mountain near Vancouver, around 1993. This photo was used as the frontispiece to Volume IV of Borel's *Collected Papers.*



Hiking in southern Utah, spring 1987.

"This does not mean at all that these researches will never find applications, that the invisible will never become visible. Experience shows the opposite; even the most abstract parts of mathematics sooner or later can find practical applications, often in the most unlikely ways. However, this point usually has no importance for the pure mathematician, who works in a world of intellectual forms with its own laws and its internal motivations, and he is often guided by æsthetic considerations. In the present environment dominated by competition for funding, it is easy for the agencies in charge of financing research to ignore or pay little attention to this intellectual speculation that apparently has no motivation and that seems to be an intellectual luxury item, thereby giving priority only to the visible part, from which one may expect a concrete practical return in a short period of time."

He returned to this last point in other writings, and he lamented the shortsightedness of such an attitude and its potential danger for the future development of mathematics. He continues:

[5] *Translated here from the French [Œ], IV, 375–6.*

"Mathematics has been for me a profession but also my preferred hobby. The course my investigations have taken, the choice of arguments to study, have been influenced by both points of view, which often are not quite distinct. Again and again I have been guided by a sense of the architecture of this building to which we continue to add new wings and new floors, while renovating the parts already built, by the feeling that certain problems had priority over others, so to open new perspectives or to establish a new foundation for future constructions. This is the professional point of view, but happily these problems were those that attracted me the most. In other instances I was not guided by such motives, being attracted only by curiosity, by the need to know the answer to an enigma, without reference to its importance in a general context."

We see here Borel as architect and planner of mathematics, a builder of magnificent constructions and of foundations for other buildings. Like all great architects, his constructions are tempered with the touch of the artist, following what he calls "æsthetic considerations". However, æsthetic considerations were for him always an aid and did not take over his overall view and philosophy of unity in mathematics.

I recall a conversation I had with him last June when I asked him about the origin of his well-known paper with Jean-Pierre Serre on Grothendieck's sweeping generalization of the Riemann–Roch theorem. He smiled and explained to me that there were no written notes by Grothendieck and they felt that what he had achieved was so important that it had to be written up in absolutely perfect form to make it accessible to everyone. I asked him why Grothendieck was delaying publication of his work. He explained to me that Grothendieck wanted the whole thing to be kind of automatic, a consequence of his constructions in algebra and his view of geometry. Indeed this was so for the first half of the proof, which dealt with embeddings. However, the second half of the proof, dealing with projections, needed a trick, technically known as a blow-up along a subvariety, which did not fit with his philosophy. It was a trick, a special tool, and there had to be something else more intrinsic that would fit better with the rest. Borel and Serre were more pragmatic and certainly had no qualms about using a well-known tool in the course of a proof in order to complete an argument. The paper they wrote is a real gem, a model for clarity, and, to a mathematician, it is very beautiful indeed.

The Institute and the School of Mathematics were of primary importance to Borel. In the School of Mathematics he was always paying a lot of attention to the selection of visitors, and quite often I saw him in his office late at night reading carefully the material presented by applicants for membership; not limiting himself to a cursory reading of letters of recommendation, he read the research papers. Often we discussed candidly and openly the relative merits of the candidates, during long walks on the Institute lawns and, weather permitting, in the Institute woods.

There is one point that should be mentioned here in which his contribution to the Institute turned out to have lasting effects. In the mid 1970s a serious controversy started at the Institute about the appointment of a professor in the School of Social Science. The Director approved it, but the faculty was split, and there was strong opposition to this appointment. Things got ugly. Faculty members ended up by not talking to each other, by making statements to the newspapers, and matters eventually ended with the resignation of the Director. Clearly something had to be done by defining precisely the relative role of the trustees, the Director, and the faculty, and a special committee, chaired by trustee Marty Segal, was appointed to this task. Borel was the faculty representative, and he played a very big role in the formulation of the new Rules of Governance of the Institute, which have served us well since then.

He was also always very involved with our School, beyond the daily running of academic affairs. I will recall one amusing story, related to Simonyi Hall, the new mathematics building. The well-known architect Cesar Pelli had been selected for the task of designing the building and the auditorium, Wolfensohn Hall. Borel was very involved in the project. He was not at all intimidated by having to deal with a famous architect, and beyond the appearance Borel also wanted the building and the auditorium to be very functional. When the discussion came to the rather mundane topic of the heating and air-conditioning system, the architect proposed fixed windows and a forced-air system. Borel was adamant; he wanted windows that could be opened, at least in spring and the early autumn, when the weather in Princeton is really beautiful. The architect did not want such a change: it would change the visual aspect of the façade of the building. The administration did not want it: it could mean loss of heat in winter and loss of cooling in summer, with higher electricity bills. However, Borel persevered, and at last the architect decided to consult the Swedish firm that was going to supply the special windows, asking for a solution. The answer came as a surprise. Yes, it could be done by dividing the window into four horizontal sections, the lowest of which could be opened by pushing it forward. Pelli not only agreed to this but also found that the horizontal subdivision of the windows into four sections was visually much more appealing than the subdivision into two parts he had originally planned. There was one more problem: with an open window, screens are needed to keep out insects. The difficulty was the handle for opening the window. When the screen was mounted, a person could not reach the handle and had to remove the screen to open or close the window, hardly a practical solution. We all thought about how to solve the problem, but no satisfac-

Photographs on pages 504 and 505 by Bill Casselman.

Dragan Miličić and Borel hiking near the junction of the Yampa and Green Rivers in Dinosaur National Monument between Colorado and Utah, summer 2002.

tory solution was found by us mathematicians nor by the architects. The solution was instantly found by a clever employee, sent to measure the windows by the firm chosen to build the screens: split the screen with an additional movable small screen in the center. In this way one could slide the small screen sideways, creating an opening so as to reach the window handle, and then slide the small screen back in place. In the end, everyone was happy.

Mathematics and the Institute were not Borel's only interests. He loved music, especially jazz and Indian music, and he timed his professional trips to India with major music festivals, which he attended on a regular basis. He was instrumental in initiating a concert series at the Institute, which he directed until 1992 with a varied choice of performances ranging from early and baroque music, classical and contemporary, to jazz and Indian music. On a lighter side, he organized informal jazz concerts by members proficient in playing the piano or the saxophone, and he helped also in selecting good bands for playing in our traditional midwinter ball. He loved nature, and quite often I walked with him in the Institute woods, talking about the future of mathematics and of our School of Mathematics. He was very active and fit until his illness, and he loved hiking and swimming. He even took scuba diving certification when he was already over sixty. At some point scuba diving became too strenuous an exercise for him to do, but he continued to do snorkeling, the last time in Belize in winter 2003. He liked the Institute woods, and he was very relieved when eventually they did not fall to a developer and were preserved as a park. The last time I saw him I mentioned that the same afternoon I was planning to go in the Institute woods to visit my secret chanterelles patch, maybe I would find a few, would he like to have some too? He had a big smile and just said, "Oh yes!" I found quite a few chanterelles, and I suspect that they were much better for him than those one can buy in a store, not just because they were very fresh, but especially because they came from the Institute woods. He loved nature and was concerned with preserving the environment and with the future of our country, and he contributed generously to charities.

He always set very high standards for himself in his dedication to tasks, in honesty and integrity at work, in relationships with others, and he expected the same from other people too. He had a very reserved personality, and a first meeting with him was quite intimidating. However, people who knew him a little beyond a casual or purely business acquaintance soon found a good sense of humor, a warm human being, and a real friend under the surface.

He was a great scientist, a giant in the mathematical world, and a great colleague. We all miss him.

Friedrich Hirzebruch

When I learned of Armand Borel's death in August 2003, my thoughts went back to our long friendship, to our joint work, and to our last exchange of correspondence, which had occurred around Armand's eightieth birthday in May 2003. I had sent him congratulations on that occasion, and in his response he mentioned his appreciation for the introduction I had given him for his Euler Lecture on May 19, 1995.

The Euler Lecture takes places in Sanssouci in Potsdam. By now this is a rather well-known yearly lecture that attracts mathematicians from Berlin, Potsdam, and also farther away. The lecture is in a small rococo theater built by Friedrich the Great, who had invited Euler to Potsdam, where Euler worked for many years before he went to St. Petersburg. In 1995 Borel was the person invited to deliver the Euler Lecture, and it is a custom that a kind of extended introduction about the speaker is given. The nature of this introduction is best rendered in a single word by "laudatio" in Latin or "encomium" in English. I did this for Borel in 1995. He liked this "laudatio", and this is what he was referring to in his final email to me on June 6, 2003:

> Dear Fritz,
>
> How nice of you to remember an old friend and send me such a warm letter on the occasion of my becoming an octogenarian. Already ten years of retirement. As I wrote to you at your "Emeritierung", I found this a pleasant situation and I hope you feel the same, especially since, as I gather from your letter, Inge and you are in good health.
>
> There are still projects of publications and trips but also a tendency at this age to reminisce. You speak of the Euler lecture. In retrospect, I was very glad that you insisted so much for me to give it (I was rather reluctant at first), since this is indeed one of our fond memories. I really enjoyed giving a lecture in these unique surroundings, especially after your (too) nice "laudatio". And of course, our two years in Princeton, our joint work and so much else, remain very much on my mind.
>
> We both travel, but our paths do not seem to cross anymore. I hope they will sometime.
>
> With best regards from both of us to both of you.
>
> Armand

The facts and sentiments in that "laudatio" still apply today. Charles Thomas[6] has kindly translated the "laudatio" into English, so that I can include it now:

Armand Borel studied at the Eidgenössische Technische Hochschule (ETH) in Zürich and was an "assistant" there from 1947 to 1949. After this he worked in Paris while supported by a grant from the CNRS, served as a replacement for the professor of algebra in Geneva, and from 1952 to 1954 was a member of the renowned Institute for Advanced Study in Princeton, becoming a professor in 1957.

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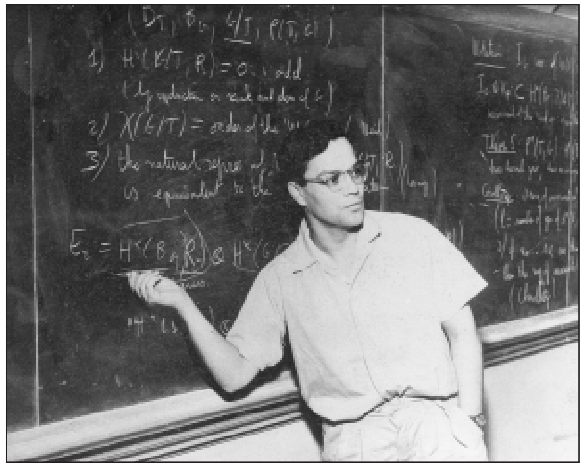
[6] *Charles Thomas is professor of algebraic topology at the University of Cambridge. His email address is* `c.b.thomas@pmms.cam.ac.uk`.

Borel was always a little ahead of me; already he was born a few years before me. When I studied in Zürich from 1949 to 1950, he was an assistant in Zürich or researcher in Paris. Forty-five years ago in Zürich was the first time we were able to discuss mathematics together. When in 1952 we both began two exciting years as members of the Institute for Advanced Study, he but not I was already married. My wife-to-be arrived somewhat later (November 1952) in Princeton, where we actually married, the Borels joining in the celebration. Hence today it is a great pleasure for me to welcome not only Armand to the "Neues Palais" but also Gaby Borel. When we began our joint work in 1953 in Princeton ("Characteristic classes and homogeneous spaces", first appearing 1958–1960), Borel's knowledge of Lie groups far outstripped mine, and it still does. Borel's impressive Paris thesis (in Germany it would be called his "Habilitationsschrift") with the title "Sur la cohomologie des espaces fibrés principaux et des espaces homogènes de groupes de Lie compacts"[7] appeared in 1953 in the *Annals of Mathematics* and was for me a kind of bible. The thesis examiners were J. Leray, H. Cartan, and A. Lichnerowicz.

In 1992 Borel was awarded the Balzan Prize for mathematics. In his acceptance speech[8] he described the development of the notion of a group from Galois through Felix Klein to Sophus Lie. Groups appeared first as symmetry groups, as self-maps of a mathematical configuration, such that each map has an inverse and the composition of two maps still belongs to the set. Galois groups operate on the roots of an equation. Felix Klein suggested that a geometry should be studied with the help of its symmetry group. The Norwegian mathematician Sophus Lie, who served as Klein's successor in Leipzig from 1886 to 1898, there published his great work *Theorie der Transformationsgruppen*.[9] In the 1870s Lie had already considered finite-dimensional transformation groups. Examples of such Lie group actions are the group of motions of Euclidean space (rotations and translations) and the Lorentz group of special relativity. The three fundamental theorems of the Lie theory of transformation groups include the generation of local groups by infinitesimal transformations. Lie groups and Lie algebras were born.[10] Today groups and algebras are considered



Armand Borel lecturing at the AMS Summer Institute on Lie algebras and Lie groups, Colby College, Waterville, Maine, 1953. This photo is the frontispiece to Volume I of Borel's *Collected Papers*.

abstractly, divorced from the mathematical objects on which they act. The whole area of Lie groups and algebras is central for mathematical research; here many continually developing branches of mathematics come together, and modern physics is inconceivable without this. Armand Borel's great work, building on that of Élie Cartan and Hermann Weyl, belongs to this center and its numerous ramifications, and in recent decades has influenced many important developments in mathematics.

After these few general remarks let me return to our joint time in Princeton (1952–54). There Borel lectured frequently on the results and further development of the thesis already mentioned. In volume one of the three volumes of collected papers, published by Springer-Verlag[11] in 1983, may be found many important works, reaching back to the Princeton period. These demonstrate how much today's knowledge of the cohomology and homotopy of Lie groups and their homogeneous spaces is due to him.

On this transparency[12] one sees Borel during lectures at the summer school of the American Mathematical Society in 1953 in Maine. What is the significance of the first line on the blackboard

$$(B_T, B_G, G/T, \rho(T,G))\ ?$$

Well, G is a connected compact Lie group, T a maximal torus in G. For example G might be the group $U(n)$ of all unitary matrices, i.e., the automorphisms of the n-dimensional Hermitian space

[7] *"On the cohomology of principal fiber spaces and homogeneous spaces of compact Lie groups".*

[8] *"Quelques réflexions sur les mathématiques en général et la théorie des groupes en particulier", lecture on the occasion of the award of the 1992 Balzan Prize in mathematics, published in French in a booklet* Orientamenti e Attività dei Premi Balzan 1992 *of the Fondazione Internazionale Balzan, Milan.*

[9] Theory of Transformation Groups.

[10] *This was the birth. The christening came in the 1930s with the introduction of the terms "Lie groups" and "Lie algebras".*

[11] *A fourth volume appeared in 2001, after this "laudatio" was given.*

[12] *This refers to the photograph that appeared as the frontispiece of Volume I of the collected papers and is reproduced at the top of this column.*

Photograph courtesy of Armand Borel's family.

Armand and Gaby Borel, probably at the AMS Summer Institute at Colby College, 1953.

$\mathbb{C}^n$, and T the group of diagonal matrices. But what is B_G? Shortly before I came to Princeton, I had learned the theory of fiber bundles from Steenrod's book. For Borel the theory of fiber bundles was already something self-evident, as it is today for many mathematicians and physicists. B_G is the classifying space for G, from which all fiber bundles with structure group G can be induced. Properties of G are fundamental for the study of G-bundles. For $G = U(n)$ the cohomology ring of B_G is generated by the Chern classes, which are thus defined for each $U(n)$-bundle. We have arrived at the theory of characteristic classes, which at that time played such a major role for me and which I was able to learn from Borel. More precisely I ought to say that the classifying space B_T of the maximal torus is fibered over B_G with the homogeneous space G/T as fiber, and that $\rho(T, G)$ is the projection map $B_T \to B_G$ of this fiber bundle. The Weyl group acts on this fibration. In the case $G = U(n)$ this is the symmetric group for n symbols. The fibers are flag manifolds, consisting of all pairwise mutually perpendicular n-tuples of 1-dimensional subspaces of an n-dimensional Hermitian vector space, permuted by the Weyl group. The cohomology comes from the fibration and the theory of symmetric functions: the symmetric group acts on the n variables in the polynomial ring over them (the cohomology ring of B_T), and the Chern classes appear as the elementary symmetric functions.

The manifold G/T is algebraic. To it one can apply the Riemann-Roch theorem that I was developing at that time. I did this for $G = U(n)$, needed first to determine the Chern classes of the tangent bundle of G/T, and showed this to Borel, who recognized the roots of the Lie algebra in the formula. This held more generally for arbitrary G and led to our joint work. The Riemann-Roch theorem applied to G/T suddenly delivered Hermann Weyl's formula for the degree of the irreducible representations of G, from which it was only a short step to the Borel–Weil theorem in the representation theory of G. This one sentence hides a long story. Today a mathematics graduate student would certainly require a one-semester course of four hours per week in order to understand it.

Since the two years in Princeton were so important for me, I have devoted much of this short lecture to this time. Everything else will have to be much shorter. Immediately after the Princeton period Borel went to Chicago and worked there on algebraic groups. His important work "Groupes linéaires algébriques"[13] appeared in 1956 in the *Annals of Mathematics*. This work is essential for the classification of semisimple groups over algebraically closed fields (achieved by Chevalley) and for many further developments. The work of W. Baily and A. Borel ("Compactification of arithmetic quotients of bounded symmetric domains", *Annals of Mathematics*, 1966) is famous. It deals with arithmetically defined, discontinuous groups of automorphisms of a bounded homogeneous symmetric domain, with the compactification of the quotient space as a normal analytic space, and with the embedding of the compactification in a projective space by means of automorphic forms.

Many other deep results for arithmetic groups and the links with number theory cannot be mentioned here. Areas to which Borel has also made decisive contributions in the past twenty years can be characterized under the following headings: cohomology of arithmetic groups, applications to K-theory, automorphic functions, and infinite-dimensional representations of real and p-adic Lie groups.

From March 1 until June 30, 1994, Borel was supported by a Humboldt Research Award at the Max Planck Institute. Every morning he was the first to arrive at the Institute, having beforehand swum for one hour in the Ennert–Gebirge open-air pool. His final report on his time at the MPI shows that he still stands in the middle of active research:

1. "Joint seminar with D. Zagier on the higher regulators for algebraic number fields, which I introduced about twenty years ago, polylogarithms, the Zagier conjectures relating them and Goncharov's proof in two special cases. I gave about six lectures in that seminar."
2. He reported the completion of a survey article on "Values of zeta-functions at integers, cohomology and polylogarithms". Here it seems to me we are close to his topic for today.
3. He reported on new results in the homology of general linear groups over a number field.

As has been already said, since 1957 Borel has been a professor at the Institute for Advanced

[13] *"Linear algebraic groups".*

Study. Here he has always played a conspicuous role. His seminars on topics of current research interest have enriched the scientific life of young members of the Institute. Here new theories were worked through, and his own new research discoveries were introduced. From these seminars have come these books, all with contributions by other authors:

Seminar on Transformation Groups (1960),
Seminar on Algebraic Groups and Related Finite Groups (1970),
Continuous Cohomology, Discrete Subgroups and Representation of Reductive Groups (1980).

Between 1983 and 1986 he worked part-time at the Institute for Advanced Study and also held a chair at the ETH in Zürich. He organized a Pan-Swiss research seminar, meeting over the railroad station in Bern. From it the books *Intersection Cohomology* and *Algebraic D-modules* emerged. The success of this seminar can be gleaned from the observation that it still continues under the label "Borel Seminar", without Borel's participation.

The Borels have looked after members at the Institute for Advanced Study in many ways. My wife and I have attended many parties, for which Gaby Borel is to thank. On September 23, 1959, he collected us, three children, and nineteen pieces of luggage, by car and trailer from Hoboken, New Jersey, and brought us to 39 Einstein Drive in the Institute's housing complex in Princeton. Let this service, admittedly reserved for only a few members, be acknowledged and applauded at this time. In any event it was a good start to my first sabbatical as professor at the University of Bonn.

Borel has given much thought to the place of mathematics in our culture. Many people were impressed by his lecture to the Siemens Foundation on "Mathematik, Kunst und Wissenschaft".[14] On the last page is the sentence:

> I hope that I have at least left you with the impression that mathematics is an extremely complex creation, which exhibits so many essentially common traits from Art and from both the experimental and theoretical Sciences. It reflects simultaneously all three of them and therefore must be distinguished from all three of them.

At the end of his acceptance lecture[15] upon being awarded the Balzan Prize, he warned us all of well-known dangers:

> More and more, people insist on usefulness. It is said that we have paid enough attention to fundamental research and that it is time to turn to its applications. If such a policy is adopted, people will doubtless realize at some stage that they have spoiled the very source of the practical applications that they are trying to favor. This may not happen immediately, because mathematical research possesses such verve that nothing can prevent it from continuing on its trajectory for a certain time. I could therefore console myself by saying that, if there is decline, I will not witness it. But that would be small consolation for someone who does not tire of admiring the wealth and beauty of mathematics in its present state and is convinced that what is to come will be in no way inferior.

Mathematicians and politicians can learn much from Borel.

Tonny A. Springer

The foundations of the modern theory of linear algebraic groups were laid in Armand Borel's paper "Groupes linéaires algébriques", published in 1956 [Œ 39]. Below I shall review, more or less chronologically, his publications on the theory of algebraic groups proper. These are relatively few in number. The more numerous publications about applications of the theory, for example to arithmetic groups and automorphic forms, fall outside the scope of this segment of the article. Some of the publications about applications are discussed in Arthur's segment below.

Linear Algebraic Groups

A *linear algebraic group* over the field of complex numbers $\mathbb{C}$ is a subgroup G of a group $GL_n(\mathbb{C})$ of invertible $n \times n$-matrices whose elements $g = (g_{ij})_{1 \le i,j \le n}$ are precisely the solutions of a set $P_a(g_{ij}) = 0$ $(1 \le a \le N)$ of polynomial equations in the matrix entries. A linear algebraic group G is an affine algebraic variety, and thus the machinery of algebraic geometry can be used. Topological notions in G will be relative to the Zariski topology.[16]

Examples of algebraic groups (the adjective "linear" will be dropped) are the group of diagonal matrices (which is abelian), the group of upper triangular matrices (which is solvable), and the complex orthogonal group, defined by quadratic equations.

[14] *"Mathematics, Art and Science", lecture of May 7, 1981, to the Carl Friedrich von Siemens Foundation, Munich, published in German as number 35 in the series of lectures to the foundation.*

[15] *See footnote 8.*

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[16] *A Zariski closed set is the zero set of a family of polynomial functions.*

Of course, $GL_n(\mathbb{C})$ is also an example. An algebraic group isomorphic to a group of diagonal matrices is called a *torus*.

If the P_a have coefficients in a subfield F of $\mathbb{C}$ (e.g., $F = \mathbb{Q}$), then G is said to be *defined over F*. In all this $\mathbb{C}$ may be replaced by an algebraically closed field k (of arbitrary characteristic) and F by a subfield of k. (If F is nonperfect,[17] some care has to be taken.) If G is defined over F, one has the group $G(F)$ of points of G with coordinates in F, known as the group of *F-rational points*.

When Borel took up the subject, the notion of algebraic group had already been around for some time, and a restricted assortment of results was known. Borel's book [15] contains an excellent review of the history of the theory of algebraic groups. I mention only a few points.

Algebraic groups appeared in the nineteenth century. In the 1880s É. Picard tried to develop a Galois theory for complex linear differential equations with polynomial coefficients. He had the insight that such an equation has a Galois group that is an algebraic group. Somewhat later L. Maurer established some general properties of linear algebraic groups. In the background of his work are Lie's theory of transformation groups and the theory of invariants.

The more general notion of algebraic group variety—an algebraic variety with a compatible group structure (similar to the notion of a Lie group)—also appeared in the nineteenth century. Examples are elliptic curves, which are projective curves with a structure of abelian group.

Around 1950 basic general facts about algebraic groups over arbitrary fields were being developed, for example by A. Weil, who required foundational material for his work on abelian varieties over an arbitrary field. He gave a construction of the quotient of an algebraic group variety by a closed subgroup, as an algebraic variety. (In contrast to the case of Lie groups, this construction is rather delicate). It was known also that the quotient of a (linear) algebraic group by a closed normal subgroup is again an algebraic group.

Pertaining to the theory of algebraic groups is work of E. R. Kolchin in 1948, motivated by Ritt's algebraic version of Picard's work on linear differential equations. Kolchin showed that a connected solvable algebraic group can be put in upper triangular form over any algebraically closed field. This is a global analog of an old theorem of Lie about solvable Lie algebras.

At about the same time Chevalley began a study of linear algebraic groups in characteristic zero, partly inspired by Maurer's work. Lie algebras played an important role.

[17] *The field F of characteristic p is perfect if either $p = 0$ or if every element of F is a p^{th} power.*

"Groupes Linéaires Algébriques"

Borel started work on this paper in Chicago in 1954–55. In the paper he gives a systematic exposition of the theory, using methods of algebraic geometry. Perhaps he was influenced by the work of his teacher Heinz Hopf, who had introduced global geometric methods in the theory of compact Lie groups, thus circumventing the use of Lie algebras.

The paper starts with a discussion of elementary matters. One elementary but important ingredient of the theory is the Jordan decomposition. Let g be an element of the algebraic group $G \subset GL_n(k)$. Then there exist unique elements $g_s, g_u \in G$ such that $g = g_s g_u = g_u g_s$, g_s is semisimple (diagonalizable), and g_u is unipotent (all its eigenvalues are 1). If $G = GL_n(k)$, this follows from the Jordan normal form theorem. Moreover, the decomposition is intrinsic, i.e., independent of the particular imbedding $G \subset GL_n(k)$. The result was more or less in the literature, but Borel gave it its final form.

The paper then discusses solvable groups. It is shown that a connected solvable algebraic group G is a semidirect product $G = T.G_u$, where G_u is the set of unipotent elements of G, which is a connected normal closed unipotent[18] subgroup, and where T is a maximal torus of G. Moreover, any two maximal tori of G are conjugate.

Borel Subgroups

The heart of the paper is the study of Borel subgroups, as they are called now. A *Borel subgroup* B of the connected algebraic group G is a connected solvable closed subgroup of G that is maximal relative to these properties. The main results about them are:

(a) G/B is a projective variety,

(b) any two Borel subgroups of G are conjugate,

(c) the Borel groups cover G.

Although similar results in the context of Lie groups were in the air, Borel's results in arbitrary characteristic were surprisingly elegant and the proofs were surprisingly simple. A crucial ingredient for the proof is the "orbit lemma": if the algebraic group G acts on an algebraic variety X (in the sense of algebraic geometry), there exists a point of X whose G-orbit is closed.

Borel once told me that at first he was doubtful about the lemma, because in complex analytic geometry—then more familiar—such a result is completely false. But a conversation with Weil removed the doubts.[19]

[18] *An algebraic group is unipotent if all its elements are unipotent.*

[19] *This must be the conversation alluded to in [Œ, vol. IV, p. 660].*

Here is a sketch of how the main results (a) and (b) follow from the orbit lemma. Assume that $G \subset GL_n(k)$. There is a natural projective variety X on which G acts, namely the flag variety of $V = k^n$. A *flag* in V is a sequence of subspaces $(V_1, V_2, \ldots, V_{n-1}, V_n)$ of V with $\dim V_i = i$ for all i and $V_i \subset V_{i+1}$ for $1 \leq i \leq n-1$. The set F_n of all flags has a structure of projective variety (this is a fact from classical algebraic geometry) on which the algebraic group $GL_n(k)$ acts. Hence G acts on F_n. The orbit lemma implies that there is a closed subgroup C of G such that G/C is a projective variety. Then C is a subgroup of $GL_n(k)$ that is triangular relative to some basis of k^n, hence is solvable. If G itself is solvable, such quotients are affine varieties, hence must be finite. If moreover G is connected, it follows that G fixes a flag, i.e., can be put in triangular form. This is the Lie–Kolchin theorem.

By a similar argument Borel proves a fixed point theorem, which now carries his name: a connected solvable algebraic group G acting on a projective (or complete) variety X fixes a point of X.

From the fixed point theorem it is not hard to see that the identity component B of C (the component containing the identity element) is a Borel subgroup. The conjugacy of Borel subgroups follows by another application of the fixed point theorem, proving (a) and (b).

Now let S be a subtorus of G (a closed subgroup that is a torus), and let B be a Borel subgroup. Applying the fixed point theorem to S and G/B, one sees that S is conjugate to a subgroup of B. From the fact that maximal tori in B are conjugate, it follows that two maximal tori of G are conjugate. (This is an analog of the conjugacy of Cartan subgroups of a compact Lie group.)

The proof of (c) proceeds as follows. Let B be a Borel subgroup of G. To establish (c), one has to show that the sets gBg^{-1}, for $g \in G$, cover G. To prove this, first a geometric analysis is made of the union of the conjugates of a given closed subgroup of G. In the case of a Borel group B this leads to the following construction (Borel formulates things a bit differently).

Let $\tilde{G}$ be the quotient of $G \times B$ by the B-action $b.(g, b') = (gb^{-1}, bb'b^{-1})$, and let $\pi : \tilde{G} \to G$ be the morphism induced by the map $(g, b) \mapsto gbg^{-1}$. The image of π is the union of all Borel subgroups. So property (c) says that π is surjective. As π is a proper morphism, surjectivity will follow if the image of π is dense. This is proved by showing that the conjugates of a subgroup of B (namely the connected centralizer of a maximal torus) fill up a dense subset.

The map π appears for the first time, implicitly, in Borel's work. Further study of π and of its fibers has led to interesting insights, discussed in [Slo]. For example, let G_u be the set of unipotent elements of G. This is an irreducible closed subvariety of G. Then (say over $\mathbb{C}$) the restriction of π to $(\pi)^{-1}(G_u)$ is a resolution of singularities of G_u. It has been much studied.

Chevalley's Work

What is not proved in [Œ 39] and what Borel did not know at the time of writing is the normalizer theorem: a Borel subgroup B of G coincides with its normalizer; i.e., if $g \in G$ is such that $gBg^{-1} = B$, then $g \in B$.

Chevalley proved this a little later and then developed a structure theory of semisimple groups.[20] He gave a complete classification of simple algebraic groups over any algebraically closed field k. It is the "same" as the Cartan–Killing classification of simple Lie algebras over $\mathbb{C}$.

Borel tells in [15, p. 158] that he gave Chevalley a copy of his paper in the summer of 1955. The next summer Chevalley told him that after reading the paper he had proved the normalizer theorem, after which "the rest followed by analytic continuation."

Chevalley also introduced the combinatorial ingredients from Lie theory, such as root system and Weyl group. His work was published in the Paris Seminar Notes [Che]; they were for several years the standard text about the theory of algebraic groups.

The Notes also contain (without naming it) the first published version of the "Borel–Weil theorem" in the context of algebraic groups [Che, exp. 15]. The theorem asserts that in characteristic 0 the irreducible representations of a semisimple algebraic group G can be realized in spaces of sections of suitable line bundles on G/B, where B is a Borel subgroup of G.

Borel and Weil in 1954 dealt with the representations of compact Lie groups, which was seemingly a somewhat different context. In the meantime it has become clear that the representation theory of compact Lie groups is equivalent to the representation theory of reductive algebraic groups over $\mathbb{C}$.

Borel's own notes on the Borel–Weil Theorem remained unpublished until their appearance in [Œ] as [Œ 30]. The insight that representations of a Lie group or algebraic group G may be constructed using sections (or, more generally, cohomology groups) of line bundles on suitable varieties with a G-action has turned out to be quite fruitful.

Reminiscences

I entered the area of algebraic groups by a back entrance, so to speak. In the 1950s I had been interested in questions about quadratic forms and

[20] *The radical (respectively, unipotent radical) of the algebraic group G is the maximal connected normal solvable closed subgroup of G. G is semisimple if its radical is trivial. Replacing "solvable" by "unipotent", one has the the definition of the unipotent radical of G and of G being reductive.*

Borel, Bill Casselman, Mark Goresky, and Robert MacPherson celebrating Borel's sixtieth birthday in Zürich, May 1983 (see sidebar next page).

special algebraic groups, working more or less on my own. It was clear to me that I was getting involved with algebraic groups, but I did not know well the general theory. I was not yet versed in the technicalities of Lie theory (what A. Weil calls "digging roots and lifting weights").

It was a great surprise when I received in the fall of 1960 a letter from Borel, inviting me to the Institute for Advanced Study. Of course, I accepted the invitation gratefully. I spent the academic year 1961–62 at the Institute.

In that year Borel had a weekly seminar on algebraic groups. As I recall it, the seminar was more in the nature of a course, as he was the only speaker. The seminar was oriented towards rationality questions, i.e., questions involving a ground field F. This was at the time largely new territory. Borel expounded his own as-yet-unpublished work. A little later he joined forces with J. Tits, who worked in the same direction. I shall return to their joint work below.

The seminar was very stimulating, and I learned a great deal. Moreover, Borel gave very good advice.

Soon during my stay at the Institute I got to know Robert Steinberg, who also attended the seminar. We discussed a problem of "fusion" of conjugacy classes over finite fields, which he was interested in. This is as follows. Let G be a connected algebraic group over a finite field F, and let k be an algebraic closure of F. Suppose $a, b \in G(F), g \in G$ are such that $gag^{-1} = b$. Is there $g \in G(F)$ with this property? A theorem of Lang of 1956 showed that this is the case if the centralizer of a in G is connected. So a general question arose: what can one say about connectedness of centralizers in a connected algebraic group G, assumed to be semisimple?

Borel, when consulted about this question, put us on the right track by pointing out that in his paper [Œ 53] on torsion in compact Lie groups, which had just appeared, connectedness of centralizers was proved for a compact semisimple Lie group under the assumption that the group is simply connected. In the proof the affine Weyl group makes its appearance, with a reference to E. Stiefel, who introduced Borel to these matters. In a footnote Borel remarks that he also proved that the centralizer of a semisimple element of a complex semisimple simply connected Lie group is connected. But he did not publish the proof. An analysis of the connectedness of centralizers of semisimple elements in semisimple groups over any algebraically closed field was given by Steinberg [St2, §§8–9] in 1968. In particular, he proved connectedness in a simply connected group.[21]

Borel's paper [Œ 53] on torsion is of interest also for other reasons. It introduces for the first time the "bad" primes for the simple types of Lie groups (for example 2, 3, 5 for type E_8). Subsequently these showed up in the theory of algebraic groups in several other places, for example in Galois cohomology and in the study of unipotent elements.

The paper is also unique among Borel's papers in having a literary quotation in the introduction,[22] an ironic comment on the technicalities of the paper.

Galois Cohomology

The last part of Borel's seminar of 1961–62 was devoted to Galois cohomology of linear algebraic groups, at the time a new topic. Let G be an algebraic group over the field F, and let E/F be a finite Galois extension with group Γ. A 1-cocycle of Γ with values in $G(E)$ is a $G(E)$-valued function z on Γ satisfying $z(st) = z(s).s(z(t))$. Two such cocycles z, z' are equivalent if there is $g \in G(E)$ with $z'(s) = g^{-1}z(s)(s.(g))$ for $s, t \in \Gamma$. The set of equivalence classes is denoted by $H^1(E/F, G)$. Taking a limit over all finite extensions E/F contained in a given separable closure of F, one obtains the Galois cohomology set $H^1(F, G)$. It has a distinguished element 1.

Several problems about algebraic groups involving a ground field have a convenient formulation in terms of Galois cohomology. An example is the problem of describing F-isomorphism classes of algebraic groups over a field F. The theorem of Lang, which was alluded to before, when formulated

[21] *An isogeny $G \to G'$ of semisimple groups is a surjective homomorphism of algebraic groups with finite kernel. G is simply connected (respectively, adjoint) if any isogeny $G' \to G$ (respectively, $G \to G'$) is an isomorphism of algebraic groups.*

[22] *Namely, a quotation from G. B. Shaw,* Arms and the Man*: "You have a low shopkeeping mind. You think of things that would never come into a gentleman's head." "That's the Swiss national character, dear lady."*

Borel and Gamesmanship

In his 1998 *Notices* article, Borel described the enormous effort and meticulous care that went into the writing of the Bourbaki works. He went on to express his pride in the fact that the authors of these works ultimately remained anonymous. But his favorite story of hidden authorship concerns what is now commonly referred to as "Chevalley's Theorem".

After Borel heard C. Chevalley first lecture on this theorem, he told Chevalley that he needed to have the same result for characteristic $p > 0$, and did Chevalley's proof work in this case? When Chevalley did not answer, Borel began to pester him at least to publish the proof so that it could be referred to and modified as necessary. Chevalley, according to Borel's story, felt the result was too trivial to merit publication. But Borel persisted, and eventually in a fit of exasperation Chevalley gave Borel his notes, saying, "Do whatever you want with these!"

Indeed, the notes did not cover the case of characteristic p, but only a few extra remarks were needed. So Borel, following Chevalley's notes, wrote up the theorem including the characteristic p case, added a footnote to say that A. Borel would be using the characteristic p case in a forthcoming paper, put Chevalley's name on the manuscript, and gave it to A. Weil, who was the editor of the *American Journal of Mathematics*, for publication.

There were a few tense moments when Borel and Weil feared the ruse might be discovered, particularly when the list of accepted papers was circulated among the associate editors, which included Chevalley. But they surmised correctly that Chevalley rarely looked at his mail, and it was not until the bound volume arrived on his desk that Chevalley found the paper with his name on it. His surprise and confusion turned to anger. Chevalley stormed into Borel's office. "What is the meaning of this?" he demanded. "You told me to do whatever I wanted with your notes, and that is what I did with them," Borel answered. Now recalling the conversation, Chevalley realized that he had lost the argument and left the office, closing the door behind him.

Borel could be terrifying, and it was rumored (especially among the younger mathematicians) that if you wanted to stay healthy, you would best keep out of his way. But I believe his sometimes gruff demeanor was simply an expression of his natural competitiveness and self-confidence.

I recall a fancy meal in a formal French restaurant in Zürich on the occasion of Borel's sixtieth birthday, when our whole party—Borel, W. Casselman, R. MacPherson, and I—was almost asked to leave by the manager after Borel had taken out his pen and corrected the French spelling and grammar on the huge glossy menu. We were saved, I believe, only by Borel's distinguished appearance and commanding presence. At the end of the meal, Borel removed from his vest pocket an envelope containing a new credit card, removed from his wallet the old credit card, put the new one into his wallet, and broke the old one into about ten pieces, making a little pile beside his empty wine glass. During a lull in the conversation, I picked up two of these pieces, studied them, and said to him, "Why, it appears that you have broken the wrong card." He looked at me defiantly without saying a word. After several moments of silence I ventured, "You don't seem to be very worried." This was the cue he had been waiting for. "Well, in the first place," he admonished, speaking slowly and deliberately, "I checked it three times before breaking it up. And in the second place, and even more important, I am becoming familiar with your peculiar sense of humor." With that, having beaten me at my own game, he broke into laughter, which quickly spread to the whole table.

—Mark Goresky

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in Galois cohomology terms, states that $H^1(F, G) = 1$ if F is finite and G is connected.

At the time of the seminar, Borel and Serre were preparing the paper [Œ 64] on Galois cohomology, which appeared in 1964. Borel explained part of the material of the paper, such as the formalism of exact sequences and the proof of finiteness of $H^1(F, G)$ when F is a p-adic field.

He also introduced us to Serre's Conjectures I and II. Conjecture I states that $H^1(F, G) = 1$ if F has cohomological dimension 1 and G is connected. During the seminar I proved Conjecture I for a particular class of fields. For perfect fields it was proved by Steinberg in 1963 in [St1, §10] as a consequence of work on conjugacy classes. The general case was proved in [Œ 80]. Conjecture II about fields of dimension 2 is still open as far as I know, although proved in many particular cases.

Borel did not further pursue Galois cohomology. The notes of Serre's lectures on this topic at the Collège de France in 1962–63 have been very influential. They have gone through several editions and were translated into English [Ser]. Galois cohomology is still an interesting and active subject. I owe my early introduction to it to Borel.

Grothendieck's Results

I mentioned already that in his seminar that Borel expounded his results on rationality questions. But he had to assume perfectness of the ground field F. This is an undesirable restriction, as it excludes interesting fields such as global fields of nonzero characteristic. In subsequent work with

Tits the restriction could be removed, thanks to work of Grothendieck (in 1964 in his Seminar on Group Schemes [SGA3, exp. XII, XIII, XIV]). Grothendieck's new results were:

(a) an algebraic group G defined over F contains a maximal torus defined over F;

(b) if moreover G is connected reductive and F is infinite, then $G(F)$ is Zariski-dense in G.

"Groupes Réductifs"

I now come to the work of Borel and Tits on algebraic groups over an arbitrary field F, published in 1965 in their paper "Groupes réductifs" [Œ 66]. I think that Borel was motivated by questions of an arithmetical nature, such as the construction of boundary pieces of the compactification of a quotient by an arithmetic group (cf. [Œ 59]). Tits's interest had its origin in geometry.

Thanks to Grothendieck's results, Borel and Tits could deal with an arbitrary base field F. The paper of Borel and Tits contains a wealth of interesting material. I mention only a few of its results.

Let G be a connected algebraic group defined over $F \subset k$, as before. A subgroup P of G containing a Borel subgroup is called a *parabolic subgroup*. Then $P \subset G$ is parabolic if and only if G/P is a complete variety, as follows via an application of Borel's fixed point theorem. The parabolic subgroups containing a given Borel subgroup can be described by combinatorial data extracted from the root system of G.

Now let G be reductive. In the theory over F the role of Borel subgroups over k is taken over by the minimal parabolic subgroups defined over F. It is shown that two of these are conjugate by an element of $G(F)$. If there are no proper parabolic subgroups over F, the group G is called *anisotropic*. For example, such is the case with the orthogonal group defined by an anisotropic quadratic form over F, whence the name.

The role of maximal tori over k is taken over by subtori of G that are defined over F and F-split, i.e., F-isomorphic to a group of diagonal matrices, and maximal for these properties. Let S be such a torus. Then G is anisotropic if and only if S lies in the center of G. This implies that the centralizer M of S, which is a connected group over F, is anisotropic. Out of M and a half-space in the character group of S, which is a free abelian group of rank $\dim S$, one can construct a minimal parabolic subgroup over F. The split torus S defines a "small" Weyl group, an ingredient of a "Tits system" on $G(F)$.

It is also shown that two maximal split F-tori are $G(F)$-conjugate. In fact, this is true for any F-group G, not necessarily reductive. The proof is sketched in the later paper [Œ 110] by Borel and Tits.

The Boulder Conference

Borel and G. D. Mostow were the organizers of a Summer Institute of the American Mathematical Society "Algebraic Groups and Discontinuous Subgroups", held in Boulder (Colorado) in the summer of 1965. Looking today at the proceedings of the Institute [3], one is struck by the taste and foresight shown in the choice of the subjects. Several of them have had impressive developments in the intervening years. Such are: Hasse principles, Tamagawa numbers, Eisenstein series, Shimura varieties, Bruhat-Tits theory (of algebraic groups over local fields, the theory having been conceived in Boulder), rigidity of arithmetic groups.

I took part in the Boulder Institute. Shortly before, I had tried to understand Grothendieck's new results, mentioned above. An interesting feature was his use of the Lie algebra $\mathfrak{g}$ of the algebraic group G over F. So far, there had been a feeling that it was not of much use.

I noticed that there is an additive Jordan decomposition for elements of $\mathfrak{g}$ and that in characteristic 0 the fixed point group in G (acting on $\mathfrak{g}$ by the adjoint representation) of a well-chosen semisimple element of $\mathfrak{g}(F)$ contains a maximal torus of G that is defined over F (if F is infinite). I wondered if an argument of this kind could work for arbitrary F to prove Grothendieck's result (a). (The argument cannot be used in G, since for a nonperfect field F one does not know a priori that nontrivial semisimple elements of $G(F)$ exist; for $\mathfrak{g}$ there is no problem.) A difficulty was that the fixed point group might be all of G.

In Boulder I discussed the matter with Borel. Very soon he saw how the difficulty could be removed: by exploiting a result of Serre (used by him in the context of abelian varieties), passing to a quotient not by a subgroup but by a subalgebra of $\mathfrak{g}$. This led to the short paper [Œ 76] in [3]. A further outgrowth was [Œ 80], in which Grothendieck's result (b) is also dealt with.

Further Work with Tits

Subsequently Borel continued his collaboration with Tits. In [Œ 92] it is shown how to associate canonically to a unipotent subgroup U of a reductive group G a parabolic subgroup whose unipotent radical contains U (in [Œ 92] fields of definition are also taken into account). A consequence is the following nice result, which was known already in characteristic 0: a maximal proper closed subgroup of G is either parabolic or reductive.

The starting point of the long joint paper [Œ 97] is the problem of determining the automorphisms of the abstract group $G(F)$, where G is a connected semisimple group over F. More generally, the homomorphisms are studied of $G(F)$ into a similar group $G'(F')$. The problem had been around since the 1920s and had been solved in many particular cases, under restrictions on G or F ([15, pp. 134–140]).

G is assumed to be semisimple. The important standing hypothesis is: G is isotropic over F, so contains proper parabolic subgroups over F. Let G^+ be the subgroup of $G(F)$ generated by the groups $U(F)$, where U runs through the unipotent radicals of the parabolic subgroups over F of G (G^+ is a "large" subgroup of $G(F)$ but does not always coincide with it). If $\phi : F \to F'$ is a homomorphism of fields, one can transport G via ϕ to a group ${}^{\phi}G$ over F' and there is a canonical homomorphism $\phi^0 : G(F) \to {}^{\phi}G(F')$.

I shall not try to fully describe the results of [Œ 97]. Here is a typical example. Assume that G is simple (and isotropic over F). Let G' be a simple (nontrivial) algebraic group over F', and let $\alpha : G(F) \to G'(F')$ be a homomorphism such that $\alpha(G^+)$ is Zariski-dense in G'. Then there exist a homomorphism $\phi : F \to F'$ and an isomorphism of algebraic groups $\beta : {}^{\phi}G \to G'$ over F' such that $\alpha(g) = \beta(\phi^0(g))$ for $g \in G^+$.

Another topic of the paper is the analysis of an irreducible representation (in the algebraic sense) $\rho : G(F) \to PGL_n(k')$, k' being algebraically closed. It is shown that ρ can be built up from irreducible projective representations of the algebraic group G.

The paper exploits the properties of parabolic subgroups established in [Œ 66]. There are many technicalities, in particular in characteristic 2. The restriction to isotropic groups made in [Œ 97] is very essential. For the case of anisotropic groups, there is, as far as I know, as yet no general theory.

The short note [Œ 110] of Borel and Tits announces a number of results about a connected algebraic group G over a nonperfect field F that is "reductive relative to F", i.e., that has no connected normal unipotent closed subgroup defined over F (the assumptions in [Œ 110] are a bit more general). Analogs of the results of [Œ 97] are announced, such as a theory of "pseudo-parabolic" subgroups and the existence of a Tits system on $G(F)$. Subsequently, Tits has extended the work to obtain classification results. He has lectured about these matters at the Collège de France, but full proofs of his results and of those of [Œ 110] have not appeared. (I have treated some of the results of Borel and Tits in [Sp, Ch. 15].)

Epimorphic Groups

Borel's last contributions to the general theory of linear algebraic groups are in joint work with Bien and Kollár in the 1990s [Œ 147, 148, 158]. Let G be an algebraic group over k, and let H be a closed subgroup. In the cited papers the situation is studied where the variety G/H is quasi-complete; i.e., its regular functions are constant. The papers [Œ 147, 148] study the groups H with this property (called "epimorphic"). It is conjectured that if H is epimorphic and if E is a finite-dimensional rational H-module, the induced G-module is also finite dimensional. In [Œ 158], which studies questions of rational connectedness of homogeneous spaces, it is shown that the conjectured property holds if G/H contains sufficiently many one-dimensional images of $\mathbb{P}^1$. This last paper is methodologically interesting: it applies recent work on rational curves on algebraic varieties.

Borel was an excellent expositor. This does not mean that his research papers, often about difficult topics, are easy to read. But his presentation of the topics is excellent, striving for maximal clarity in the exposition. The same was true for his talks on his own work or on the work of others.

I knew Borel for more than forty years. During some periods we had quite frequent contacts, and not only about mathematics. In later years the contacts were less frequent, but they never ceased. The last time he wrote me was in May of 2003. This was about mathematics, but he also wrote that he was reasonably well. Hence it was a shock to hear in August about his demise.

I remember him with admiration and gratitude.

Jacques Tits

Armand Borel As I Knew Him

I do not remember exactly when I first met Armand Borel. It may have been in Paris in 1949 or in Zürich in 1950. After that we often met, and we soon became good friends. He liked to recall jokingly that he was the one from whom I learned that there existed five exceptional simple Lie groups, the study of which became for a while, shortly afterward, my special trade. According to accepted French usage between students and young scholars at the time, we always called each other by our family names, and I shall continue doing so here. A few years later, our friendship was extended to our wives, Gaby Borel and Marie-Jeanne Tits. Almost immediately, the four of us used the second person ("tu") to address each other, a quite unusual familiarity at the time between two French people of opposite sexes; there is no mystery, however: the Borels were Swiss and the Titses were still Belgian.

In the course of years, I came to know Borel very well and to appreciate his great qualities even more. All who have been in contact with him will agree, I believe, that among these qualities the dominant one was his exceptional earnestness, or

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maybe I should rather say conscientiousness. In German I would perhaps use the word "Gründlichkeit". This trait was always prominent when one worked with him; I shall give examples shortly when going over our joint work, but I had many other occasions to observe it: here are a few instances of diverse nature:

- when opinions were solicited for an appointment or an award, his reports were always highly appreciated for being the most informative and reliable, while being at the same time clear and concise;
- participants to meetings that were organized by him knew how seriously they were meant to work, but also how great was the reward; and, by the way, I suppose they had the same experience that I did, namely, that the proceedings were always published rapidly;
- in a completely different register (not totally disconnected from the previous one though), hiking was a serious matter for Borel; at the Oberwolfach meetings, he always belonged, of course, to the group of those who made the long walk (at high speed); I believe he felt that strong physical exertion was necessary to his intellectual activity;
- I believe that his knowledge of jazz and of a certain variety of Indian music happened at a professional level; I was unable to follow him there, but concerning classical music, my wife and I had with him very enriching discussions about the quality of performances we had heard together; he also introduced us to the art of his friend, the pianist Friedrich Gulda. The term "professional" used here could certainly be applied to most activities of Borel in any domain.

The tension that normally goes together with the extreme seriousness I have been talking about was probably responsible for a certain gruffness that some people resented but that was, in my experience, a purely superficial reflex. It was not rare that, to a question I had asked him, Borel first reacted as if I was offending him by asking something so elementary (or so stupid!). But immediately afterward came the answer to the question, perfectly clear and calm, exactly all I could hope for. A mutual friend of ours, who at first was very intimidated by Borel, had made the same observation. He told me that Borel on one occasion had asked him a question to which, for once, this friend knew the answer. To see what would happen, he then replied exactly as Borel would have done, and Borel accepted the scenario as the most natural thing in the world. But all this concerns relationships with equals and friends. The fact of the matter is that I never saw Borel react nastily to an expression of honest ignorance: he was always willing to explain things patiently to someone who did not know (I was often in that situation). On the other hand, he had very little tolerance for pretentious or arrogant attitudes.

It was not in Borel's habit to make inflated compliments; I already said that his professional evaluations were very much appreciated for that reason. But he was well capable of enthusiasm for beautiful achievements, in mathematics as well as in the arts, and he then expressed it with force.

His natural seriousness and his dedication to hard work should not overshadow the fact that he was—at least as I see him—of optimistic disposition, and that he liked to laugh (including about himself, as is illustrated by the quotation[23] of Bernard Shaw ending the introduction of [Œ 53]). In all our working sessions or during the many meals we had together, there were always long moments full of joy and good humor.

I have especially in mind our meetings in the late 1970s. Some of them occurred in Lausanne (or, more precisely, at La Conversion), where Marie-Jeanne and I enjoyed the wonderful hospitality of Gaby Borel. Alternatively, Armand could be visiting his mother in Paris and took advantage of this opportunity to come and see his friends, in particular my wife and me. The scheme was invariable: there suddenly came unexpectedly a ring of the phone; I answered and heard the unmistakable beautiful deep voice merely saying, "Allôô." This always unleashed mixed feelings: "What a nice surprise!" yet also, "How shall I manage to complete my (always urgent) program for the next few days?" Of course, the second problem got solved some way or other, and there remained only the pleasure of working and talking with him. The session of two or three days always ended with the ritual quotation: "When shall we three meet again?"

If I remember well, the last time we met was neither in Lausanne nor in Paris, but in Heidelberg after an editorial meeting. Shakespeare was not quoted that time, because, at the last moment, there arose a great confusion because Borel's hotel key had gotten lost. We separated in a hurry and we never met again.

Working with Armand Borel

It was great good fortune and a wonderful experience for me to collaborate with Armand Borel for many years: between 1965 and 1978. We wrote six papers together [Œ 66, 82, 92, 94, 97, 110]. Since their content is at least partially reviewed in the segment by T. A. Springer in the present article, I shall restrict the technical part of my description to a few points that, in my opinion, deserve to be emphasized.

Our first and main joint paper, entitled "Groupes réductifs" [Œ 66], is also nicknamed "(le) Borel–Tits" by many of its users. Its main purpose is to set up

[23] *See footnote 22.*

the foundations of the relative theory of reductive groups; here "relative" refers to the fact that the ground field is not assumed to be algebraically closed. Major objects of that study include: tori, parabolic subgroups, the relative root system, the relative Weyl group, and the notion of split reductive group (a group having maximal tori that are *split*, that is, are direct products of multiplicative groups). The origin and circumstances of this collaboration are perhaps instructive: both Borel and I had already worked and published on the subject, which we approached with quite different backgrounds and aims; he was mainly influenced by Lie theory and algebraic geometry, and I by "synthetic" and projective geometry; he was primarily thinking in terms of tori and root systems, and I in terms of parabolic subgroups. However, our results were closely related and intertwined. In the winter of 1962–63, I was visiting the Institute for Advanced Study in Princeton, where he had been appointed permanent professor a few years before. We both knew of course that we had a wealth of common knowledge between us, but it took us quite a while to realize that the only sensible thing to do with it was to publish it jointly. The decision was made shortly before I left Princeton for Chicago in the spring of 1963, so that much of the work had to be done at later meetings, which we organized in Chicago and various other places, or by mail. I think that both of us learned much in the process: the resulting paper contained a lot more than what each of us knew before. Personally, besides great mathematics, I learned a considerable amount of writing technique. (I had a similar experience later on when I collaborated with François Bruhat; he and Borel, each one in his own way but both strongly influenced by the strict principles of Bourbaki's rigor, taught me how to write mathematics.)

Most of the results presented in "Groupes réductifs" have become widely known, and there is no need here to spend many words on them beyond the indications given in T. A. Springer's report. I wish however to make special mention of the main theorem of §7 of that paper, which went largely unnoticed, although Borel and I liked it and often referred to it in subsequent work: it states that any connected reductive k-group G contains a split reductive subgroup H, the root system of which is the system of "long" relative roots[24] of G.

One delicate point concerning the paper "Groupes réductifs" was the fact that its main results, while easily shown to hold over arbitrary perfect fields, could at first be extended to arbitrary fields only by using fairly deep scheme-theoretic techniques of Grothendieck. Borel disliked what he considered, I believe, a lack of proportion between means and aims, and was happy when he managed,

[24] *That is, when α and 2α are roots, α is to be dropped.*

Photograph by C. J. Mozzochi.

At the Langlands sixtieth birthday conference, Princeton, 1996. Left to right in the group of four: Gregg Zuckerman, Borel, Marie Zuckerman (back to camera), and Laurent Clozel.

in a joint paper [Œ 80] with T. A. Springer, to get rid of that disharmony (see the section "The Boulder Conference" in Springer's contribution).

When discussing the joint work by Borel and me, our paper [Œ 97] naturally comes second, considering both its length and the fact that its announcement [Œ 82] was our second publication chronologically. The paper [Œ 97] had its origin in a question that M. Goto asked us during the Boulder conference on algebraic groups in 1965. Roughly speaking, he asked: to what extent are the isomorphisms between the groups of rational points of two algebraic simple groups over two fields a combination of an isomorphism between the two fields and an algebraic ("morphic") isomorphism of the algebraic groups? We considered mainly the case of isotropic—that is, nonanisotropic—groups over infinite fields (although our paper also handles, but with different methods, the case of compact groups over nondiscrete local fields, where we slightly improve earlier results of É. Cartan and B. L. van der Waerden). The answer we gave to the problem is rather typical of what happened when one worked with Borel: no corner of the question was left in the dark. Instead of merely considering isomorphisms, we treated the case of homomorphisms with Zariski dense image, and the final result was characteristic free (to be sure, some special cases, especially in characteristic 2, were a bit of a headache, but under Borel's moral code nothing short of a complete solution could be satisfactory). Our main theorem included most earlier contributions to the subject and also answered open questions of R. Steinberg, among others. Talking about this paper gives an opportunity to emphasize how valuable Borel's encyclopaedic knowledge of the literature always was to ensure complete and accurate reference to earlier work and to avoid duplications.

Our collaboration on [Œ 92] was brought about by a fortuitous (and happy) circumstance. Here again, Borel's extensive knowledge of the literature played a role. Inspired by a recent paper of V. P. Platonov, he had written a note proving the analog for simple groups over algebraically closed fields of a classical result of V. V. Morozov: a maximal proper subalgebra of a semisimple complex Lie algebra either is semisimple or contains a maximal nilpotent subalgebra. Borel sent a copy of the manuscript of this note to a mutual friend of ours, who showed it to me. I observed that the argument of Borel's proof could be adapted to a considerably more general setting and provided results over arbitrary fields, for instance the following fact, which turned out to be of great importance in finite group theory: if G is a reductive k-group (k being any field) and U is a split[25] unipotent subgroup, then there exists a parabolic k-subgroup P of G whose unipotent radical contains U such that every k-automorphism of G preserving U also preserves P; then, in particular, P contains the normalizer of U. (Much earlier, I had conjectured this fact and given a case-by-case proof "in most cases", but the new approach, using Borel's argument, gave a uniform and much simpler proof.)

I mention *en passant* the complements [Œ 94] to "Groupes réductifs", containing results concerning, among other things, the closure of Bruhat cells in topological reductive groups and the fundamental group of real algebraic simple groups.

The last joint paper by Borel and me is a *Comptes Rendus* note [Œ 97] that in personal discussions we nicknamed "Nonreductive groups". Indeed, we had discovered that many of our results on reductive groups could, after suitable reformulation, be generalized to arbitrary connected algebraic groups. Examples are the conjugacy by elements rational over the ground field, of maximal split tori, of maximal split unipotent subgroups, and of minimal pseudoparabolic subgroups,[26] or the existence of a BN-pair (hence of a Bruhat decomposition). Complete proofs of those results never got published, but both Borel and I, independently, lectured about them (he at Yale University, and I at the Collège de France).

[25] *A unipotent group over a field k is said to be split if it has a composition series over k, all quotients of which are additive groups.*

[26] *Pseudoparabolic subgroups are a substitute for parabolic subgroups that one must use when dealing with nonreductive groups over nonperfect fields.*

James Arthur

My topic is Armand Borel and the theory of automorphic forms. Borel's most important contributions to the area are undoubtedly those established in collaboration with Harish-Chandra [Œ 54, 58]. They include the construction and properties of approximate fundamental domains, the proof of finite volume of arithmetic quotients, and the characterization in terms of algebraic groups of those arithmetic subgroups that give compact quotients. These results created the opportunity for working in the context of general algebraic groups. They laid the foundations of the modern theory of automorphic forms that has flourished for the past forty years.

The classical theory of modular forms concerns holomorphic functions on the upper half plane $\mathcal{H}$ that transform in a certain way under the action of a discrete subgroup Γ of $SL(2,\mathbb{R})$. The multiplicative group $SL(2,\mathbb{R})$ consists of the 2×2 real matrices of determinant 1, and each element $\gamma = \left(\begin{smallmatrix} a & b \\ c & d \end{smallmatrix}\right)$ of $SL(2,\mathbb{R})$ acts on $\mathcal{H}$ by the linear fractional transformation $z \to \frac{az+b}{cz+d}$. For example, one can take Γ to be the subgroup $SL(2,\mathbb{Z})$ of integral matrices or, more generally, the congruence subgroup

$$\Gamma(N) = \left\{\gamma \in SL(2,\mathbb{Z}) : \ \gamma \equiv I \bmod N\right\}$$

attached to a positive integer N. The theory began as a branch of complex analysis. However, with the work of E. Hecke, it acquired a distinctive number theoretic character. Hecke introduced a commuting family of linear operators on any space of automorphic forms for $\Gamma(N)$, one for each prime not dividing N, with interesting arithmetic properties. We now know that eigenvalues of the Hecke operators govern how prime numbers p split in certain nonabelian Galois extensions of the field $\mathbb{Q}$ of rational numbers [Sh], [D]. Results of this nature are known as reciprocity laws and are in some sense the ultimate goal of algebraic number theory. They can be interpreted as a classification for the number fields in question. The Langlands program concerns the generalization of the theory of modular forms from the group of 2×2 matrices of determinant 1 to an arbitrary reductive group G. It is believed to provide reciprocity laws for all finite algebraic extensions of $\mathbb{Q}$.

Let us use the results of Borel and Harish-Chandra as a pretext for making a very brief excursion into the general theory of automorphic forms. In so doing, we can follow a path illuminated by Borel himself. The expository articles and monographs of Borel encouraged a whole generation of

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mathematicians to pursue the study of automorphic forms for general algebraic groups. Together with the mathematical conferences he organized, they have had extraordinary influence.

The general theory entails two reformulations of the classical theory of modular forms. The first is in terms of the unitary representation theory of the group $SL(2,\mathbb{R})$.

The action of $SL(2,\mathbb{R})$ on $\mathcal{H}$ is transitive. Since the stabilizer of the point $i=\sqrt{-1}$ is the special orthogonal group

$$K_{\mathbb{R}} = SO(2,\mathbb{R}) = \left\{k_\theta = \begin{pmatrix} \cos\theta & \sin\theta \\ -\sin\theta & \cos\theta \end{pmatrix}\right\},$$

one can identify $\mathcal{H}$ with the space of cosets $SL(2,\mathbb{R})/K_{\mathbb{R}}$. The space of orbits in $\mathcal{H}$ under a discrete group $\Gamma \subset SL(2,\mathbb{R})$ becomes the space of double cosets $\Gamma\backslash SL(2,\mathbb{R})/K_{\mathbb{R}}$. A *modular form* of weight $2k$ is a holomorphic function f on $\mathcal{H}$ such that[27]

$$f(\gamma z) = (cz+d)^{2k} f(z)$$

whenever $\gamma = \begin{pmatrix} a & b \\ c & d \end{pmatrix}$ is in Γ. A modular form of weight 2, for instance, amounts to a holomorphic 1-form $f(z)dz$ on the Riemann surface $\Gamma\backslash\mathcal{H}$, since $d(\gamma z) = (cz+d)^{-2}dz$. For a given f, the function F on $SL(2,\mathbb{R})$ defined by $F(g) = (ci+d)^{2k} f(z)$ when $g = \begin{pmatrix} a & b \\ c & d \end{pmatrix}$ and $z = gi$ is easily seen to satisfy

$$F(\gamma g k_\theta) = F(g) e^{-2ki\theta},$$

for $\gamma \in \Gamma$. The requirement that f be holomorphic translates to the condition that F be an eigenfunction of a canonical biinvariant differential operator Δ on $SL(2,\mathbb{R})$ of degree 2, with eigenvalue a simple function of k. The theory of modular forms of any weight becomes part of the following more general problem:

> *Decompose the unitary representation of $SL(2,\mathbb{R})$ by right translation on $L^2\left(\Gamma\backslash SL(2,\mathbb{R})\right)$ into irreducible representations.*

That the problem is in fact more general is due to a variant of Schur's lemma. Namely, as an operator that commutes with $SL(2,\mathbb{R})$, Δ acts as a scalar on the space of any irreducible representation. To recover the modular forms of weight $2k$, one would collect the irreducible subspaces of $L^2\left(\Gamma\backslash SL(2,\mathbb{R})\right)$ with the appropriate Δ-eigenvalue, and from each of these, extract the smaller subspace on which the restriction to $K_{\mathbb{R}}$ of the corresponding $SL(2,\mathbb{R})$-representation equals the character $k_\theta \to e^{-2ki\theta}$.

This is all explained clearly in Borel's survey article [Œ 75] in the proceedings of the 1965 AMS conference at Boulder [3]. The Boulder conference was organized jointly by Borel and G. D. Mostow. It was a systematic attempt to make the emerging theory of automorphic forms accessible to a wider audience. Borel himself wrote four articles [Œ 73, 74, 75, 76] for the proceedings, each elucidating a different aspect of the theory.

The second reformulation is in terms of adeles. Though harder to justify at first, the language of adeles ultimately streamlines many fundamental operations on automorphic forms. The relevant Boulder articles are [T] and [K]. These were not written by Borel, but were undoubtedly commissioned by him as part of a vision for presenting a coherent background from the theory of algebraic groups.

The adele ring

$$\mathbb{A} = \mathbb{R} \times \mathbb{A}_{\text{fin}} = \mathbb{R} \times \Big(\widetilde{\prod_{p \text{ prime}}} \mathbb{Q}_p \Big)$$

of $\mathbb{Q}$ is a locally compact ring that contains the real field $\mathbb{R}$, as well as completions $\mathbb{Q}_p$ of $\mathbb{Q}$ with respect to the p-adic absolute values

$$|x|_p = p^{-r}, \qquad x = p^r(ab^{-1}),\ (a,p) = (b,p) = 1,$$

on $\mathbb{Q}$. One constructs $\mathbb{Q}_p$ by a process identical to the completion $\mathbb{R}$ of $\mathbb{Q}$ with respect to the usual absolute value. In fact, one has an enhanced form of the triangle inequality,

$$|x_1 + x_2|_p \le \max\left\{|x_1|_p, |x_2|_p\right\}, \qquad x_1, x_2 \in \mathbb{Q},$$

which has the effect of giving the compact "unit ball" $\mathbb{Z}_p = \left\{x_p \in \mathbb{Q}_p :\ |x_p|_p \le 1\right\}$ the structure of a subring of $\mathbb{Q}_p$. The complementary factor $\mathbb{A}_{\text{fin}}$ of $\mathbb{R}$ in $\mathbb{A}$ is defined as the "restricted" direct product

$$\widetilde{\prod_p} \mathbb{Q}_p = \Big\{x = (x_p) :\ x_p \in \mathbb{Q}_p,\ x_p \in \mathbb{Z}_p \text{ for almost all } p\Big\},$$

which becomes a locally compact (totally disconnected) ring under the appropriate direct limit topology. The diagonal image of $\mathbb{Q}$ in $\mathbb{A}$ is a discrete subring. This implies that the group $SL(2,\mathbb{Q})$ of rational matrices embeds into the locally compact group $SL(2,\mathbb{A})$ of unimodular adelic matrices as a discrete subgroup. The theory of automorphic forms on $\Gamma\backslash SL(2,\mathbb{R})$, for any congruence subgroup $\Gamma = \Gamma(N)$, becomes part of the following more general problem:

> *Decompose the unitary representation of $SL(2,\mathbb{A})$ by right translation on $L^2\left(SL(2,\mathbb{Q})\backslash SL(2,\mathbb{A})\right)$ into irreducible representations.*

The reason that the last problem is more general than the previous one is provided by the theorem of strong approximation, which applies to the

[27] *There is also a mild growth condition that need not concern us here.*

Photograph by Lyn Howe, provided courtesy of Roger Howe.

"Armand Borel Math Camp" at the AMS Summer Institute on automorphic forms, representations, and L-functions at Corvallis, Oregon, 1977. Front: Nick Howe. Back row, left to right: Roger Howe, Armand Borel, Robert Langlands, Bill Casselman, Marie-France Vigneras, Kenneth Ribet. Borel has the special designation "Coach" on his T-shirt and Casselman has the designation "Assistant Coach".

simply connected[28] group $SL(2)$. The theorem asserts that if K is any open compact subgroup of $SL(2,\mathbb{A}_{\text{fin}})$, then

$$SL(2,\mathbb{A}) = SL(2,\mathbb{Q}) \cdot (K \cdot SL(2,\mathbb{R})).$$

This implies that if $\Gamma = SL(2,\mathbb{R}) \cap SL(2,\mathbb{Q})K$, then there is a unitary isomorphism

$$L^2\Big(SL(2,\mathbb{Q})\backslash SL(2,\mathbb{A})/K\Big) \xrightarrow{\sim} L^2\Big(\Gamma\backslash SL(2,\mathbb{R})\Big)$$

that commutes with right translation by $SL(2,\mathbb{R})$. For example, if we take K to be the group

$$K(N) = \Big\{x = (x_p):\ x_p \in SL(2,\mathbb{Z}_p),$$
$$x_p \equiv I \bmod (M_2(N\mathbb{Z}_p))\Big\},$$

then Γ equals $\Gamma(N)$. To recover the decomposition of $L^2\Big(\Gamma(N)\backslash SL(2,\mathbb{R})\Big)$, one would collect the irreducible subspaces of $L^2\Big(SL(2,\mathbb{Q})\backslash SL(2,\mathbb{A})\Big)$, and from each of these, extract the smaller subspace for which the restriction to K of the corresponding $SL(2,\mathbb{A})$-representation is trivial.

Given that the decomposition of $L^2\Big(SL(2,\mathbb{Q})\backslash SL(2,\mathbb{A})\Big)$ includes the classical theory of modular forms, we can see reasons why the adelic formulation is preferable. It treats the theory simultaneously for all weights and all congruence subgroups. It is based on a discrete group $SL(2,\mathbb{Q})$ of rational matrices rather than a group $\Gamma(N)$ of integral matrices. Most significantly, perhaps, it clearly displays the supplementary structure given by right translation under the group

[28] *"Simply connected" in this instance means that $SL(2,\mathbb{C})$ is simply connected as a topological space.*

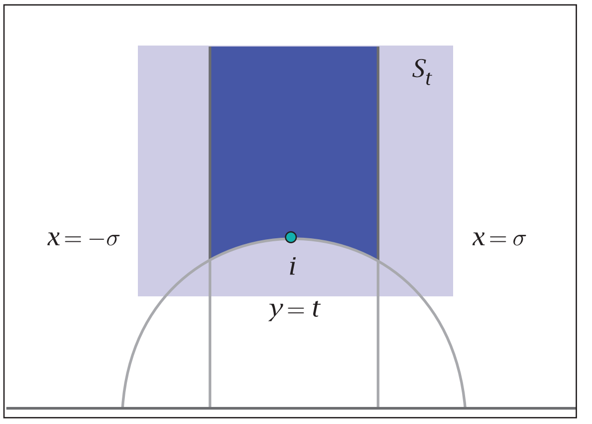

Figure 1. Standard fundamental domain for the action of $SL(2,\mathbb{Z})$ on the upper half plane, together with a more tractable approximate fundamental domain. The standard fundamental domain, darkly shaded, is the semi-infinite region bounded by the unit circle and the vertical lines at $x = \pm 1/2$. The approximate fundamental domain S_t generalized by Borel and Harish-Chandra is the total shaded region.

$SL(2,\mathbb{A}_{\text{fin}})$. The unitary representation theory of the p-adic groups $SL(2,\mathbb{Q}_p)$ thus plays an essential role in the theory of modular forms. This is the source of the operators discovered by Hecke. Eigenvalues of Hecke operators are easily seen to parametrize irreducible representations of the group $SL(2,\mathbb{Q}_p)$ that are *unramified* in the sense that their restrictions to the maximal compact subgroup $SL(2,\mathbb{Z}_p)$ contain the trivial representation. It turns out that in fact *any* irreducible representation of $SL(2,\mathbb{Q}_p)$ that occurs in the decomposition of $L^2\Big(SL(2,\mathbb{Q})\backslash SL(2,\mathbb{A})\Big)$ carries fundamental arithmetic information.

It is now straightforward to set up higher-dimensional analogs of the theory of modular forms. One replaces[29] the group $SL(2)$ by an arbitrary connected reductive algebraic group G defined over $\mathbb{Q}$. As in the special case of $SL(2)$, $G(\mathbb{Q})$ embeds as a discrete subgroup of the locally compact group $G(\mathbb{A})$. The Langlands program has to do with the irreducible constituents (known as *automorphic representations*) of the unitary representation of $G(\mathbb{A})$ by right translation on $L^2\Big(G(\mathbb{Q})\backslash G(\mathbb{A})\Big)$. A series of conjectures of Langlands, dating from the mid-1960s through the 1970s, characterizes the internal structure of automorphic representations. The conjectures provide a precise description of the arithmetic data in automorphic representations, together with a formulation of deep and unexpected relationships among these data as G varies (known as the "principle of functoriality").

[29] *Even in the classical case, one has to replace $SL(2)$ by the slightly larger group $GL(2)$ to obtain all the operators defined by Hecke.*

General automorphic representations are thus firmly grounded in the theory of algebraic groups. It seems safe to say that the many contributions of Borel to algebraic groups described by Springer and Tits in this article are all likely to have some role to play in the theory of automorphic forms. Borel did much to make the Langlands program more accessible. For example, his Bourbaki talk [Œ 103] in 1976 was one of the first comprehensive lectures on the Langlands conjectures to a general mathematical audience.

In 1977 Borel and W. Casselman organized the AMS conference in Corvallis on automorphic forms and L-functions, as a successor to the Boulder conference. It was a meticulously planned effort to present the increasingly formidable background material needed for the Langlands program. The Corvallis proceedings [10] are considerably more challenging than those of Boulder. However, they remain the best comprehensive introduction to the field. They also show evidence of Borel's firm hand. Speakers were not left to their own devices. On the contrary, they were given specific advice on exactly what aspect of the subject they were being asked to present. Conference participants actually had to share facilities with a somewhat unsympathetic football camp, led by Coach Craig Fertig of the Oregon State University Beavers. At the end of the four weeks, survivors were rewarded with orange T-shirts, bearing the inscription ARMAND BOREL MATH CAMP. Borel himself sported[30] a T-shirt with the further designation COACH.

Let us go back to the topic we left off earlier, Borel's work with Harish-Chandra. The action of $SL(2,\mathbb{Z})$ on $\mathcal{H}$ has a well-known fundamental domain, given by the darker shaded region in Figure 1. This region is difficult to characterize in terms of the transitive action of $SL(2,\mathbb{R})$ on $\mathcal{H}$. The total shaded rectangular region S_t in the diagram is more tractable, for there is a topological decomposition $SL(2,\mathbb{R}) = P(\mathbb{R})K_{\mathbb{R}} = N(\mathbb{R})M(\mathbb{R})K_{\mathbb{R}}$, where P, N, and M are the subgroups of matrices in $SL(2)$ that are respectively upper triangular, upper triangular unipotent, and diagonal. The group $N(\mathbb{R})$ acts by horizontal translation on $\mathcal{H}$, while $M(\mathbb{R})$ acts by vertical dilation. We have already noted that $K_{\mathbb{R}}$ stabilizes the point i. We can therefore write

$$S_t = \omega A_t \cdot i,$$

where ω is the compact subset $\left\{\begin{pmatrix} 1 & x \\ 0 & 1 \end{pmatrix} : |x| \le \sigma\right\}$ of $N(\mathbb{R})$, and A_t is the one-dimensional cone $\left\{\begin{pmatrix} a & 0 \\ 0 & a^{-1} \end{pmatrix} : a > 0,\ a^2 \ge t\right\}$. The set S_t is an approximate fundamental domain for the action of $SL(2,\mathbb{Z})$ on $\mathcal{H}$, in the sense that it contains a set of representatives of the orbits, while there are only finitely many $\gamma \in SL(2,\mathbb{Z})$ such that S_t and γS_t intersect.

For a general group G, the results of Borel and Harish-Chandra provide an approximate fundamental domain for the action of $G(\mathbb{Q})$ by left translation on $G(\mathbb{A})$. To describe it, I have to rely on a few notions from the theory of algebraic groups. Let me write P for a minimal parabolic subgroup of G over $\mathbb{Q}$, with unipotent radical N and Levi component M. The adelic group $M(\mathbb{A})$ can be written as a direct product $M(\mathbb{A})^1 A_M(\mathbb{R})^0$, where A_M is the $\mathbb{Q}$-split part of the center of M, $A_M(\mathbb{R})^0$ is the connected component of 1 in $A_M(\mathbb{R})$, and $M(\mathbb{A})^1$ is a canonical complement of $A_M(\mathbb{R})^0$ in $M(\mathbb{A})$ that contains $M(\mathbb{Q})$. The *roots* of (P, A_M) are characters $a \longrightarrow a^\alpha$ on A_M that determine a cone

$$A_t = \left\{a \in A_M(\mathbb{R})^0 :\ a^\alpha \ge t,\ \text{for every } \alpha\right\}$$

in $A_M(\mathbb{R})^0$ for any $t > 0$. Suppose that $K_{\mathbb{A}} = K_R K_{\text{fin}}$ is a maximal compact subgroup of $G(\mathbb{A})$. If Ω is a compact subset of $N(\mathbb{A})M(\mathbb{A})^1$, the product

$$S_t = \Omega A_t K_{\mathbb{A}}$$

is called a *Siegel set* in $G(\mathbb{A})$, following special cases introduced by C. L. Siegel. One of the principal results of [Œ 58] implies that for suitable choices of $K_{\mathbb{A}}$, Ω, and t, the set S_t is an approximate fundamental domain for $G(\mathbb{Q})$ in $G(\mathbb{A})$.

The obstruction to $G(\mathbb{Q})\backslash G(\mathbb{A})$ being compact is thus governed by the cone A_t in the group $A_M(\mathbb{R})^0 \cong \mathbb{R}^{\dim A_M}$. It follows that $G(\mathbb{Q})\backslash G(\mathbb{A})$ is compact if and only if A_M is trivial, which is to say that G has no proper parabolic subgroup over $\mathbb{Q}$ and no $\mathbb{Q}$-split central subgroup. This is essentially the criterion of [Œ 58].[31] Borel and Harish-Chandra obtained other important results from their characterization of approximate fundamental domains. For example, in the case of semisimple G, they proved that the quotient $G(\mathbb{Q})\backslash G(\mathbb{A})$ has finite volume with respect to the Haar measure of dx on $G(\mathbb{A})$. This is a consequence of a decomposition formula for Haar measures $dx = a^{-2\rho}\, d\omega\, da\, dk$, where ω is in Ω, a is in A_t, and k is in $K_{\mathbb{A}}$ and where 2ρ denotes the sum of the roots of (P, A_M).

The papers of Borel and Harish-Chandra were actually written for arithmetic quotients $\Gamma\backslash G(\mathbb{R})$ of real groups, as were the supplementary articles [Œ 59, 61] of Borel. Prodded by A. Weil [W, p. 25], Borel wrote two parallel papers [Œ 55, 60] that formulated the results in adelic terms and established many basic properties of adele groups.[32] His

[30] *See the photograph on the previous page.*

[31] *A similar result was established independently by Mostow and Tamagawa [MT].*

[32] *In his 1963 Bourbaki lecture [G], R. Godement presented an alternative argument, which he also formulated in adelic terms.*

later lecture notes [Œ 79], written again in the setting of real groups, immediately became a standard reference.

Borel and Harish-Chandra were probably motivated by the 1956 paper [Sel] of A. Selberg. Selberg brought many new ideas to the study of the spectral decomposition of $L^2\left(\Gamma\backslash SL(2,\mathbb{R})\right)$, including a construction of the continuous spectrum by means of Eisenstein series and a trace formula for analyzing the discrete spectrum. A familiarity with the results of Siegel no doubt gave Borel and Harish-Chandra encouragement for working with general groups. Their papers were followed in the mid-1960s by Langlands's manuscript on general Eisenstein series (published only later in 1976 [L]). In the context of adele groups, Langlands's results give a complete description of the continuous spectrum of $L^2\left(G(\mathbb{Q})\backslash G(\mathbb{A})\right)$. A starting point was the work of Borel and Harish-Chandra and, in particular, the properties of approximate fundamental domains. In recent years Borel lectured widely on the theory of Eisenstein series: in the three-year Hong Kong program mentioned by Bombieri, for example, and the 2002 summer school in Park City. One of his ambitions, alas unrealized, was to write an introductory volume on the general theory of Eisenstein series.

Photograph by C. J. Mozzochi.

Werner Mueller, Leslie Saper, and Borel (left to right) at the Park City Summer Institute, 2002.

In attempting to give a sense of both the scope of the field and Borel's substantial influence, I have emphasized Borel's foundational work with Harish-Chandra and his leading role in making the subject more accessible. Borel made many other important contributions. These were often at the interface of automorphic forms with geometry, especially as it pertains to the locally symmetric spaces

$$X_\Gamma = \Gamma\backslash G(\mathbb{R})/K_{\mathbb{R}}.$$

Elements in the deRham cohomology group $H^*(X_\Gamma,\mathbb{C})$ are closely related to automorphic forms for G, as we have already noted in the special case of modular forms of weight 2. This topic was fully explored in Borel's monograph [Œ 115, 172] with N. Wallach. Borel collaborated in the creation of two very distinct compactifications of spaces X_Γ: one with W. Baily [Œ 63, 69], the other with J-P. Serre [Œ 90, 98]. The Baily-Borel compactification became the setting for the famous correspondence between intersection cohomology (discovered by Goresky and MacPherson in the 1970s) and L^2-cohomology (applied to square integrable differential forms on X_Γ), a relationship first conjectured by S. Zucker.[33] The L^2-cohomology of X_Γ is the appropriate analog of deRham cohomology in case X_Γ is noncompact. Its relations with automorphic forms were investigated by Borel and Casselman [Œ 126, 131]. In general, the cohomology groups of spaces X_Γ are very interesting objects, which retain many of the deepest properties of the corresponding automorphic representations. They bear witness to the continuing vitality of mathematics that originated with Borel.

Gopal Prasad

Borel first visited India in 1961, when he gave a series of lectures on compact and noncompact semisimple Lie groups and symmetric spaces at the Tata Institute of Fundamental Research (TIFR) in Bombay. His course introduced the theory of Lie groups and geometry of symmetric spaces to the first generation of mathematicians at TIFR, who in turn trained the next generations in these areas. Having joined TIFR in 1966, I belong to the second generation, and so I owe a considerable debt to Armand Borel for my education in the theory of Lie groups and even more directly in the theory of algebraic and arithmetic groups, which I learned from his excellent books and numerous papers on these two topics.

Subsequently, Borel made many visits to TIFR and other mathematical centers in India. He was deeply interested in the development of mathematical institutions in India and helped several of them with his advice and frequent visits. For his contributions to Indian mathematics, he was made an honorary fellow of TIFR in 1990.

During his numerous trips to India, Armand and his wife, Gaby, visited many sites of historical and architectural interest and attended concerts of Indian classical dance and music. He became fond of many forms of Indian classical dances such as Bharat-Natyam, Odissi, and Kathakkali. Even more so, he came to love both Carnatic (south Indian) and Hindustani (north Indian) classical music. He became quite an expert on the subject and developed friendships with many musicians and dancers from India. He invited several of them to perform in the concert series he initiated and directed at the

[33] *The conjecture was later proved by L. Saper and M. Stern, and E. Looijenga.*

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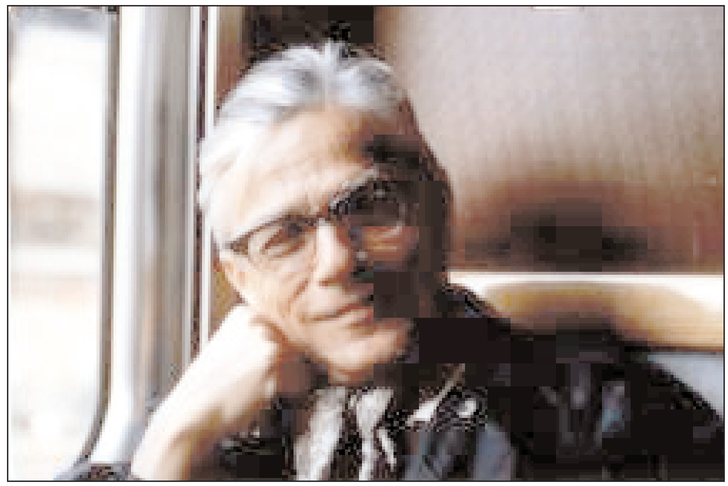
Institute for Advanced Study, and with Gaby hosted some of them in his house during their tours of the United States.

Indian classical music concerts differ from their western counterparts in many ways. The ones in India often start in the evening and continue until late into the night, with a single vocalist or instrumentalist playing for hours, sometimes solo and sometimes accompanied by a percussion player. This is always an endurance feat, not just for the artist, but for the audience too. Borel attended numerous such concerts and timed his trips to India so that he could attend a major Carnatic music festival held every December/January in Madras. Notable elements of Indian music, as with jazz, are improvisation, melody, and rhythm: the artist never uses a written score during the performance. The artist is simultaneously a composer and a performer. The rhythms involved are intricate and evolving. Borel was an equally enthusiastic jazz and blues aficionado; he frequently went to New York for such performances. It would appear that improvisation and complex rhythms in Indian classical music appealed to him quite naturally.

Borel was an astute observer: he had an uncanny eye for artistic detail and would reflect on the influence of literature and culture on human outlook. Once, at first sight, he pointed to a statue of Ganesha (the Hindu god with an elephant head and human body) in my apartment in Bombay and observed that the trunk was curled to the right and not, as he had usually seen, to the left—a seemingly obscure fact, most would think. He was delighted to learn, however, that it actually has important symbolic significance. In another, more recent, episode Borel discovered the Harry Potter children's book series by J. K. Rowling. He was greatly enthused by what her books had done to boost the literacy rate of youngsters and captivate their imagination in a time when books face such fierce competition from other media. On July 28, 2003, while already gravely ill, he wrote to Rowling to express this sentiment and to convey his admiration. In his letter he also speculated that the "quickness, cleverness" of even ordinary Indians, and not just his mathematical colleagues, "may be due in part to the fact that they are familiar with stories from great Indian epics *Ramayana* and *Mahabharata* from childhood on, in comparison with which our own fairy tales are no match."

Borel told me on more than one occasion that if he had a choice for a second life, he would like to be born a Hindu in India. This would work out well, since Hinduism, like most other religions originating in the subcontinent, propounds the doctrine of *Karma* and rebirth.

It is well known that Borel was meticulous. (And his stern demand for excellence in all things was exacting. This led to considerable anxiety in many of his colleagues.) His lectures, writing, seminars, summer schools, and trips were minutely planned. For example, before his first visit to Ann Arbor, he consulted his secretary, Elly Gustafsson, who had lived in Michigan for some time. On her suggestion, we went together to see the sculptures of the famous Swedish artist Carl Milles in Cranbrook Academy near Ann Arbor. Despite knowing of his penchant for detail, I was surprised to learn that Borel had not only closely studied the work of the artist before coming to Ann Arbor, but knew exactly how many of his sculptures there were in Cranbrook, and made sure that we saw each one of them before we left.



Photograph by Bill Casselman.

On the train, returning from a Sunday hike near Zürich, 1983.

My First Meetings

In 1972–73, I visited Yale at the invitation of Dan Mostow. This was my first visit abroad, and during the year I went to the Institute for Advanced Study (IAS), Princeton, to spend a weekend with my colleagues V. K. Patodi and R. Parthasarathy. Since Borel had already been an important influence in my mathematical development, I wanted to meet him. Patodi took me to Borel's office and introduced me to him. This happened on a Saturday. I later learned that he reserved Saturdays for writing manuscripts. However, Borel was quite warm and friendly, and we spent about two hours talking. During the discussion at that time, and on numerous subsequent occasions, I got the impression that he expected precision and showed unease in his own characteristic way if the discussion became imprecise or the arguments vague. I spent the next year at the IAS. In large measure it is because of him that I had a very fruitful stay at the Institute then and in my three subsequent year-long visits to the Institute.

During my stay at the Institute, whenever I had a question about Lie or algebraic groups, I asked Borel, and if he did not have an answer right away, he came back within a few days to provide me with a detailed answer and relevant references. Harish-Chandra once told me that whenever he needed a result about algebraic group or needed a reference, he turned to the encyclopaedic knowledge of Borel.

Borel as a Collaborator

Borel was an exceptional collaborator. He was very conscientious, and despite his many commitments, he diligently worked on questions that came up during our discussions and actively took part in writing and revising joint works.

Even before our collaboration began, I knew that he was an extremely good correspondent who promptly replied to every letter addressed to him. During my stay at the Institute in 1980–81, he told me that he considered it a duty to seriously look into and promptly respond to all mathematical letters he received. The reason for this, as he explained to me, was that S. Ramanujan, the mathematical prodigy, had written about his discoveries to two mathematicians, Henry F. Baker and E. W. Hobson (both of Cambridge University), but neither responded. Had G. H. Hardy, to whom Ramanujan wrote later, also ignored his results and not replied, Ramanujan and his work might have been lost to mathematics.

My Last Meeting

Borel kept himself fit by swimming a mile a day, weight-training, and bicycling. So the news of his illness came as a rude shock, and I immediately determined to see him. For this purpose, having arranged with Gaby through Lily Harish-Chandra to meet with Borel, I went to his house in Princeton on August 10, 2003, in the evening. Gaby and Borel's daughters, Dominique and Anne, were there. I talked with Borel for some time. He felt feverish and, at his request, I measured his temperature, which turned out to be normal. I asked him if he listened to music, his chief passion besides mathematics. To try to cheer him up, I told him that once the debilitating effects of the radiation wore off, he would be able to resume his normal routine, and I would come back to see him in a month. He commented that I was being too optimistic. Unfortunately, he was proved right, and the very next day I learned from Lily that he had passed away early that morning.

It is a great privilege to have known Armand for thirty years and to have collaborated with him. His passing away is a deep personal loss for me.

References

[LIE] N. Bourbaki, *Éléments de Mathématique: Groupes et Algèbres de Lie*, Chaps. I to IX, Hermann, Paris, and Masson, Paris, 1960 to 1982.

[Che] C. Chevalley, *Classification des groupes algébriques*, Séminaire École Normale Supérieure 1956–1958, Secrétariat de Mathématiques, Institut Henri Poincaré, Paris, 1958; 2nd edition, Springer-Verlag, to appear.

[D] P. Deligne, Formes modulaires et représentations ℓ-adiques, *Séminaire Bourbaki, Vol. 1968/69*, Lecture Notes in Math., vol. 179, Springer-Verlag, Berlin, 1971, pp. 139–172.

[SGA3] M. Demazure and A. Grothendieck, *Schémas en groupes I–III*, Lecture Notes in Math., vols. 151–153, Springer-Verlag, Berlin, 1970.

[G] R. Godement, Domaines fondamentaux des groupes arithmétiques, *Séminaire Bourbaki* **257** (1963).

[K] M. Kneser, Strong approximation, pp. 187–196 of (3) in list of Borel's books on page 501.

[L] R. Langlands, *On the Functional Equations Satisfied by Eisenstein Series*, Lecture Notes in Math., vol. 544, Springer-Verlag, Berlin, 1976.

[MT] G. D. Mostow and T. Tamagawa, On the compactness of arithmetically defined homogeneous spaces, *Ann. of Math.* **76** (1962), 446–463.

[Sel] A. Selberg, Harmonic analysis and discontinuous groups in weakly symmetric Riemannian spaces with applications to Dirichlet series, *J. Indian Math. Soc.* **20** (1956), 47–87.

[Ser] J-P. Serre, *Galois Cohomology*, Springer-Verlag, Berlin, 1997.

[Sh] G. Shimura, A reciprocity law in non-solvable extensions, *J. Reine Angew. Math.* **221** (1966), 209–220.

[Slo] P. Slodowy, *Simple Singularities and Simple Algebraic Groups*, Lecture Notes in Math., vol. 815, Springer-Verlag, Berlin, 1980.

[Sp] T. A. Springer, *Linear Algebraic Groups*, 2nd edition, Birkhäuser, Boston, 1998.

[St1] R. Steinberg, Regular elements of semisimple algebraic groups, *Inst. Hautes Études Sci. Publ. Math.* **25** (1965), 49–80; *Collected Papers*, no. 20.

[St2] ____ , Endomorphisms of linear algebraic groups, *Mem. Amer. Math. Soc.*, no. 80, 1968; *Collected Papers*, no. 23.

[T] T. Tamagawa, Adèles, pp. 187–196 of (3) in list of Borel's books on page 501.

[W] A. Weil, *Adeles and Algebraic Groups*, bound notes, Institute for Advanced Study, Princeton, NJ, 1961.