Daniel Quillen

Eric Friedlander and Daniel Grayson, Coordinating Editors

Daniel Quillen, 1940–2011, Fields Medalist, transformed many aspects of algebra, geometry, and topology. Especially in a succession of remarkable papers during the ten-year period of 1967–1977, Quillen created astonishing mathematics which continues to inspire current research in many fields. Quillen's mathematical exposition serves as the ultimate model of clarity. Despite his brilliance, those who knew Quillen were regularly impressed by his generosity and modesty. It has been our privilege to have been mentored by Quillen and to study his remarkable achievements. We feel a deep personal loss at his passing.

In this memorial article we assemble twelve contributions from Quillen's colleagues, collaborators, students, and family. Graeme Segal's contribution is a broad mathematical biography of Quillen which emphasizes the scope and breadth of his work. Hyman Bass surveys Quillen's stunning contributions to algebraic K-theory. Quillen's collaborators Joachim Cuntz and Jean-Louis Loday discuss their work with Quillen on cyclic homology. Michael Atiyah and Ulrike Tillmann, colleagues at Oxford, and Barry Mazur, of Harvard, offer their remembrances. Dennis Sullivan and Andrew Ranicki recall their early mathematical interactions with Quillen. Ken Brown and Jeanne Duflot reflect upon their experiences as students of Quillen. The final contribution, from Quillen's wife and the mother of their six children, Jean Quillen, gives a glimpse of the shy family man who created such beautiful mathematics.

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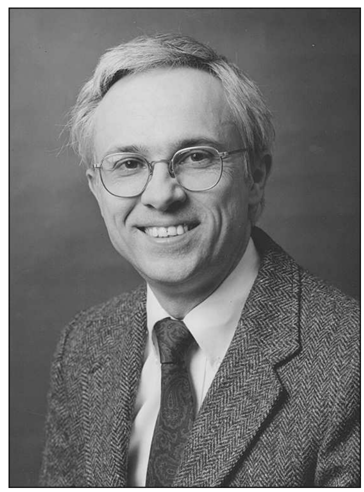

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Daniel Quillen.

Graeme Segal

Daniel Quillen, who died on April 30, 2011, at the age of seventy, was among the most creative and influential mathematicians of his time, transforming whole areas of mathematics. He solved a number of famous and important problems, but his most valuable contribution came less from that than from finding new ways of looking at the central questions of mathematics and opening paths into previously inaccessible terrain.

He was born in Orange, New Jersey, the elder of two brothers. His father, Charles Quillen, was a chemical engineer who became a teacher in a vocational high school, and his mother, Emma (née Gray), was a secretary. His mother, in particular, was very ambitious for her sons and sought out

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scholarships for Dan which carried him first to Newark Academy, an excellent private secondary school, and then (a year before finishing high school) to Harvard, where after his undergraduate degree he became a graduate student working under Raoul Bott. His thesis was on overdetermined systems of linear partial differential equations. Immediately on completing his Ph.D. in 1964, he obtained a post at MIT, where he stayed until he moved to Oxford (though with a number of years away on leave, at the IHES, and in Princeton, Bonn, and Oxford).

He said that Bott—a large, outgoing man universally beloved for his warmth and personal magnetism, outwardly quite the opposite of his shy and reticent student—was a crucial model for him, showing him that one did not have to be quick to be an outstanding mathematician. Unlike Bott, who made a performance of having everything explained to him many times over, Quillen did not seem at all slow to others, yet he saw himself as someone who had to think things out very slowly and carefully from first principles and had to work hard for every scrap of progress he made. He was truly modest about his abilities—very charmingly so—though at the same time ambitious and driven. Bott was a universal mathematician, who made contributions to many different areas of the subject while always preserving the perspective of a geometer, and Quillen too never confined himself to a "field". His most famous achievements were in algebra, but he somehow came at algebra from the outside. He was interested in almost all of mathematics and in a lot of physics too: when his eldest daughter was studying physics at Harvard, he carefully worked through the problem sheets she was given, and twenty years later he was doing the same when his youngest daughter was studying electrical engineering at Imperial College. It was a characteristic of his mathematics that he drew in ideas from very diverse areas to use for his own purposes. Throughout his life he kept a beautifully written record of the mathematical thoughts he had each day,[1] and they form an extraordinary archive, covering a huge range of topics, often his own reworkings of papers he had read or lectures he had attended. One finds, for instance, that in 1972, in the middle of the section where he was working out his treatment of algebraic K-theory for categories with exact sequences, there is a long digression entitled "Education in statistical mechanics", which begins with a conventional account of ideal gases and Carnot cycles that one might find in an undergraduate physics course, and then moves through a more mathematical discussion of entropy in statistical mechanics into considering how one can perturb the Hamiltonian or the symplectic structure on the product of a large number of copies of a symplectic manifold. It ends, mystifyingly, "Possible idea to use: entropy and how it arises from the gamma replacement for factorials."

The second great mathematical influence on Quillen—as on many others of his generation—was the towering figure of Alexander Grothendieck. Grothendieck is famous for his mystical conviction that a mathematical problem will solve itself when, by sufficient humble attentiveness, one has found exactly its right context and formulation. However that may be, he opened up one of the most magical panoramas of modern mathematics, connecting number theory, algebra, and geometry. Grothendieck's influence is most evident in Quillen's first lastingly famous work, his Springer Lecture Notes volume *Homotopical Algebra*, published in 1967, on a completely different subject from his thesis.

Its historical context was the development over the previous few decades of the new field of "homological algebra": the art of assigning homotopy types—or, initially, homology groups—to many algebraic and combinatorial structures such as groups and algebras which at first sight have nothing space-like about them. Grothendieck's special contribution to this field had been the invention (with his student Verdier) of the *derived category* in which any given abelian category—such as the modules for a given ring—can be embedded. The derived category is to the abelian category what the homotopy category is to the category of topological spaces. More strikingly, Grothendieck had shown how to associate a homotopy type to an arbitrary commutative ring, and to an algebraic variety over any field, in a way which promised to prove Weil's conjectures (made in 1949) relating the number of points of algebraic varieties defined over finite fields to the topology of the corresponding varieties over the complex numbers. Quillen had made himself a master of the ideas of the Grothendieck school, but at the same time he had immersed himself in a different mathematical tradition, that of the MIT algebraic topologists, especially Daniel Kan, who was his third great influence. (They shared a love of early rising, and were often talking at MIT long before the rest of the world was awake.) Kan was the apostle of simplicial methods: he had proved that the homotopy theory of topological spaces can be studied by entirely combinatorial means. The homotopy category is obtained from the category of topological spaces by formally

[1] *Contradicting what he often said about his own slowness, he said that he needed to write these long careful accounts to slow himself down, as otherwise his thoughts rushed headlong onwards and ended in chaos and confusion.*

Photo from George Lusztig, used with permission.

From left: George Lusztig, Daniel Quillen, Graeme Segal, and Michael Atiyah at the Institute for Advanced Study in Princeton, 1970.

inverting maps which are homotopy equivalences, and Quillen realized that Kan had proved that the *same* category is obtained by inverting a class of maps in the category of simplicial sets. He asked himself when it makes sense to invert a class of morphisms in an arbitrary category and call the result a homotopy category. He saw that the key lay in the concepts of fibration and cofibration, the traditional tools of algebraic topology, and that these were the right context for the projective and injective resolutions of homological algebra—an injective module, for example, is the analogue of a simplicial set obeying the Kan condition. His book went on to develop a very complete abstract theory of homotopy. At the time it attracted little attention except from a small band of enthusiasts, but it proved very prescient; thirty years later the theory was being widely used, and it is central on the mathematical stage today. The book was severely abstract, with hardly any examples and no applications, but Quillen immediately went on to apply the ideas to develop a cohomology theory for commutative rings—now called "André-Quillen cohomology"—and the associated theory of the cotangent complex and, after that, to show that the rational homotopy category can be modeled by differential graded Lie algebras, or, equivalently, by commutative differential graded algebras.

None of his subsequent works have the same unmistakable Grothendieck flavor of this first book. Both Grothendieck and Quillen sought for what was absolutely fundamental in a problem, but where Grothendieck found the essence in generality, Quillen's guiding conviction was that to understand a mathematical phenomenon one must seek out its very simplest concrete manifestation. He felt he was not good with words, but his mathematical writings, produced by long agonized struggles to devise an account that others would understand, are models of lucid, accurate, concise prose, which, as Michael Atiyah has pointed out, are more reminiscent of Serre than of Grothendieck.

He spent the year 1968–1969 as a Sloan Fellow at the IHES near Paris, where Grothendieck was based. The following year, spent at the Institute for Advanced Study in Princeton, was the most fertile of his life, and he produced a torrent of new work. Perhaps the most exciting development at the time was a proof of the Adams conjecture, which identifies—in terms of K-theory and its Adams operations—the direct summand in the stable homotopy groups of spheres which comes from the orthogonal groups. Quillen had already given an outline proof of this three years earlier, showing how it follows from the expected properties of Grothendieck's étale homotopy theory for algebraic varieties in characteristic p.[2] Meanwhile, however, he had been carefully studying the work of the algebraic topologists centered in Chicago, who had used ideas of infinite loop space theory to calculate the homology of many important classifying spaces. He now realized that the crucial idea of his first proof amounted to saying that the classifying spaces of the discrete group $GL_n(\bar{\mathbb{F}}_p)$ and of the Lie group $GL_n(\mathbb{C})$ have the same homology away from the prime p, and that this could be proved directly. (Here $\bar{\mathbb{F}}_p$ denotes the algebraic closure of the field with p elements.) This led straight to his development of algebraic K-theory, which is the achievement he is now most remembered for; but before coming to that I shall mention a few other things.

First, the Adams conjecture was almost simultaneously proved by Dennis Sullivan, also using Grothendieck's theory, but in a different way. While Quillen's proof led to algebraic K-theory, Sullivan's was part of a quite different program, his determination of the structure of piecewise linear and topological manifolds. This was just one of several places where Quillen's work intersected with Sullivan's though they were proceeding in different directions. Another was their independent development of rational homotopy theory, where Sullivan was motivated by explicit questions about the groups of homotopy equivalences of manifolds. Ib Madsen has remarked on the strange quirk of mathematical history that, a few years later, Becker and Gottlieb found a very much more elementary proof of the Adams conjecture which did not use Grothendieck's theory: if this had happened earlier, one can wonder when some active areas of current mathematics would have been invented.

[2] *This sketch proof was made complete a few years later in Friedlander's MIT thesis.*

At the ICM in Nice in 1970 Quillen described the theme of his previous year's work as the cohomology of finite groups. Besides the Adams conjecture and algebraic K-theory, another fertile line of development came out of this. Quillen had shown that the mod p cohomology of any compact group is controlled by the lattice of its elementary p-subgroups, proving, among other things, the Atiyah-Swan conjecture that the Krull dimension of the mod p cohomology ring is the maximal rank of an elementary p-subgroup and calculating for the first time the cohomology rings of the spin groups. He was interested in using these ideas to obtain significant results in finite group theory, but quite soon he left the field to others.

Another achievement of this golden period concerned the complex cobordism ring and its relation to the theory of formal groups. This idea is the basis of most recent work in stable homotopy theory, beginning with the determination by Hopkins of the primes of the stable homotopy category and the "chromatic" picture of the homotopy groups of spheres. Milnor's calculation of the complex cobordism ring in 1960 by means of the Adams spectral sequence had been one of the triumphs of algebraic topology. Quillen had been thinking about Grothendieck's theory of "motives" as a universal cohomology theory in algebraic geometry and also about the use Grothendieck had made of bundles of projective spaces in his earlier work on Chern classes and the Riemann-Roch theorem. He saw that complex cobordism had a similar universal role among those cohomology theories for smooth manifolds in which vector bundles have Chern classes, and that the fundamental invariant of such a theory is the formal group law which describes how the first Chern class of a line bundle behaves under the tensor product. He made the brilliant observation that the complex cobordism ring is the base of the universal formal group, and he succeeded in devising a completely new calculation of it, not using the Adams spectral sequence, but appealing instead to the fundamental properties of the geometric power operations on manifolds. This work is yet another mélange of Grothendieck-style ideas with more concrete and traditional algebraic topology. After his one amazing paper on this subject he seems never to have returned to the area.

I shall not say much about Quillen's refoundation of algebraic K-theory here, as so much has been written about it elsewhere. As he explained it in 1969–1970, one key starting point was the calculation of the homology of $BGL_\infty(\overline{\mathbb{F}}_p)$, and another was when he noticed that the known Pontrjagin ring of the union of the classifying spaces of the symmetric groups essentially coincided with the also-known Pontrjagin ring of $\Omega^\infty S^\infty$, the infinite loop space of the infinite sphere. This led him to the idea that from a category with a suitable operation of "sum"—such as the category of finite sets under disjoint union, or of modules over a ring under the direct sum—one can obtain a cohomology theory if, instead of forming the Grothendieck group from the semigroup of isomorphism classes, one constructs in the homotopy category the group completion of the topological semigroup which is the *space* of the category. The famous "plus construction", which he used in his 1970 ICM talk, is a nice way to realize the group completion concretely; it came from a suggestion of Sullivan, but I do not think it was the basic idea. Throughout his year in Princeton, Quillen was making lightning progress understanding the homotopy theory of categories, which he had not much thought about before. He realized that he must find a homotopy version of the more general construction of Grothendieck groups in which the relations come from exact sequences rather than just from direct sums, and eventually he settled on the "Q-construction" as his preferred method of defining the space. The culmination of this work was the definitive treatment he wrote for the 1972 Seattle conference on algebraic K-theory. He published only one paper on algebraic K-theory after that: his proof in 1976 of Serre's conjecture that projective modules over polynomial rings are free. This came from reflecting deeply on what was already known about the question—especially the work of Horrocks—and seeing that, when brewed lovingly in the way Grothendieck advocated for opening nuts, the result fell out.

By 1978, when he was awarded a Fields Medal, Quillen's interests had shifted back towards global geometry and analysis. His notebooks of the years 1976–1977 are mainly concerned with analysis: Sturm-Liouville theory for ordinary differential equations, scattering and inverse scattering theory in one dimension, statistical mechanics, the theory of electric transmission lines, quantum and quantum field theoretical aspects of the same questions, and also orthogonal polynomials, Jacobi matrices, and the de Branges theory of Hilbert spaces of entire functions. He gave a wonderful graduate course on these topics at MIT in 1977. He published nothing of this, however. He felt, I suppose, that he hadn't obtained any decisively new results. Nevertheless, I think one can say that a single circle of ideas connected with global analysis and index theory—an area extending to quantum field theory at one end, and at the other end to algebraic K-theory through Connes's treatment of index theory by cyclic homology—held his interest in many different guises ever after. The very last graduate course he gave in Oxford (in the year 2000, I think) was on scattering theory for the discretized Dirac equation in two dimensions.

Early in 1982 he decided that Oxford was the place he wanted to be, attracted to it especially by the presence of Michael Atiyah. He spent the year 1982–1983 on leave there, and in 1985 he moved permanently from MIT to Oxford as Waynflete Professor. (The joke surged irresistibly around the mathematical world of a dean at MIT rushing to Dan with an offer to halve his salary.)

In the 1980s he made at the very least three outstanding contributions which will shape mathematics for a long time: the invention of the "determinant line" of an elliptic partial differential operator as a tool in index theory, the concept of a "superconnection" in differential geometry and analysis, and the Loday-Quillen theorem relating cyclic homology to algebraic K-theory.

The first of these came from thinking about the relation of index theory to anomalies in quantum field theory. Determinant lines were a familiar idea in algebraic geometry, and defining regularized determinants by means of zeta functions was standard in quantum field theory and had been studied by mathematicians such as Ray and Singer. Nevertheless, the simple idea that any Fredholm operator has a determinant line in which its determinant lies and that the role of the zeta function is to "trivialize" the determinant line (i.e., identify it with the complex numbers) brought a new perspective to the subject.

"Superconnections" came from thinking about the index theorem for families of elliptic operators and also about Witten's ideas on supersymmetry in quantum theory. When one has a bundle whose fibers are compact Riemannian manifolds, there is a virtual vector bundle on the base which is the fiberwise index of the Dirac operators on each fiber. The index theorem for families gives a formula for the Chern character of this virtual vector bundle. Quillen's idea was to combine the formula expressing the index of a single Dirac operator D as the supertrace of the heat kernel $\exp D^2$ with the identical-looking definition of the Chern character form of a connection in a finite-dimensional vector bundle as the fiberwise supertrace of $\exp D^2$, where now D denotes the covariant derivative of the connection, whose curvature D^2 is a matrix-valued 2-form. He aimed to prove the index theorem for families by applying this to the infinite-dimensional vector bundle formed by the spinor fields along the fibers, defining a superconnection D, with $\exp D^2$ of trace class, by adding the fiberwise Dirac operator to the natural horizontal transport of spinor fields. Superconnections are now very widely used but, after the first short paper in which he gave the

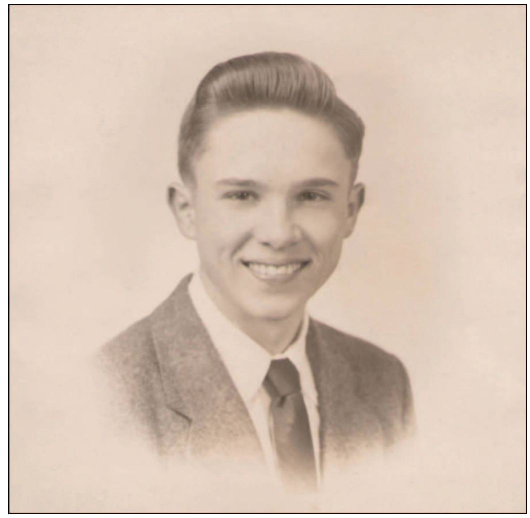
Photo courtesy of Cypora Cohen.

Quillen's Harvard college application photo.

definition and announced his project, Quillen himself did not return to the index theorem for families, as Bismut published a proof of it the following year along Quillen's lines. Only two of his subsequent papers involved superconnections. One of them (joint with his student Mathai) was extremely influential, though it dealt only with finite-dimensional bundles. It gave a beautiful account of the Thom class of a vector bundle in the language of supersymmetric quantum theory and has provided a basic tool in geometrical treatments of supersymmetric gauge theories.

The last phase of Quillen's work was mostly concerned with cyclic homology. He was attracted to this from several directions. On one side, cyclic cocycles had been invented as a tool in index theory, and the Connes "S-operator" is undoubtedly but mysteriously connected with Bott periodicity, whose role in general algebraic K-theory Quillen had constantly tried to understand. More straightforwardly, cyclic homology is the natural home of the Chern character for the algebraic K-theory of a general ring. Yet again, it seemed that cyclic theory ought somehow to fit into the framework of homotopical algebra of Quillen's first book. Connes was a virtuoso in developing cyclic cohomology by means of explicit cochain formulae, but to someone of Quillen's background it was axiomatic that these formulae should not be the basis of the theory. In trying to find the "right" account of the subject, he employed a variety of techniques, pursuing especially the algebraic behavior of the differential forms on Grassmannians when pulled back by the Bott map. One notable success has already been mentioned, his proof of a conjecture of Loday which, roughly, asserts that cyclic homology is to the Lie algebras of the general linear groups exactly what algebraic K-theory is to the general linear groups themselves.[3] In a paper written in 1989, dedicated to Grothendieck on his sixtieth birthday, he succeeded in giving a conceptual definition of cyclic homology but still wrote that "a true Grothendieck understanding of cyclic homology remains a goal for the future." He continued to make important contributions to the subject throughout the 1990s, mostly jointly with Cuntz, but I am far from expert on this phase of his work, and refer the reader to Cuntz's account. Nevertheless, on the whole I think he felt that, in T. S. Eliot's words, the end of all his exploring of Connes's work had been to arrive at where he started and know the place for the first time.

Outside mathematics his great love was music, especially the music of Bach. He always said that he met his wife, Jean, whom he married before he was twenty-one, when he was playing the triangle—and she the viola—in the Harvard orchestra. (She, however, says that he was the orchestra's librarian and occasional reserve trumpeter.) The triangle seems just the right instrument to go with his minimalist approach to mathematics. He delighted in "figuring out" things about how music worked and in devising tiny compositions of twenty or thirty bars, but he was far too driven mathematically to let himself spend much time on music. He and Jean had two children before he completed his Ph.D. and went on to have six altogether. His family was his whole life apart from mathematics, and, tongue-tied as he was, he never needed much encouragement from those he knew well to talk about his children's adventures and misadventures. Although his hair turned white in his twenties, he never lost the look or the manner of a teenager.

The last decade of his life was tragically blighted by steadily encroaching dementia. He is survived by his wife, his six children, twenty grandchildren, and one great-grandchild.

Michael Atiyah

I first met Dan during my visits to Harvard, when he was a student of Raoul Bott. I remember an excitable young man bubbling with ideas and enthusiasm which Raoul was happy to encourage. Many years later Dan became a senior colleague of mine at Oxford. By this time he was a mature mathematician with his own very individual style. He was a great admirer of Serre and later Grothendieck, and his research reflected the influence of both. Clarity and elegance were derived from Serre, but his universalist functorial approach was that of Grothendieck.

Dan was a solitary and deep thinker who spent years trying to get to the roots of a problem, and in this he was remarkably, if not invariably, successful. His interests were broad and his important contributions were characterized by their essential simplicity and inevitability. I am still impressed by his beautiful use of formal groups in cobordism theory, where elegant algebra is brought to bear so fruitfully on geometry.

His style did not lend itself to collaboration, but his influence was extensive. As a person he was quiet and modest, with none of the brashness that sometimes accompanies mathematical brilliance. But beneath the quiet exterior there was still the sparkle that I saw in the young student. Although he was dedicated to mathematics, this was balanced by his commitment to his family and to music.

[3] *This theorem was proved independently and roughly simultaneously by Tsygan.*

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Hyman Bass

Dan Quillen and I worked in the same physical space on only two occasions, both of them in settings that were pleasantly garden-like, but filled with intellectual ferment. One was the 1968–1969 year we both spent at the Institut des Hautes Études Scientifiques, attending Grothendieck's seminars, as well as Serre's course at the Collège de France. The other was a two-week conference on algebraic K-theory at the Battelle Memorial Institute in Seattle, in the summer of 1972.

During the year in Paris, Quillen presented his typical personal characteristics: a gentle good nature, modesty, a casual and boyish appearance unaltered by his prematurely graying hair, and his already ample family life. In that brilliant, and often flamboyant, mathematical milieu, Quillen seemed to listen more than he spoke, and he spoke only when he had something substantial to say. His later work showed him to be a deep listener.

At the Battelle Conference, in contrast, Quillen was center stage. This conference was a watershed event in the history of algebraic K-theory, owing primarily to Quillen's performance. In what follows I recall the context and atmosphere of that event and the paradigm-changing results and ways of thinking that Quillen brought to it, work for which he was awarded a Fields Medal in 1978.

The seed of K-theory was Grothendieck's introduction of his group $K(X)$ to formulate his generalized Riemann-Roch theorem for algebraic

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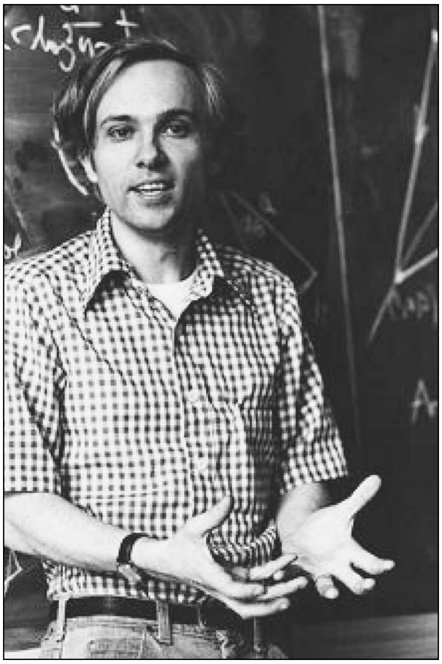

Photo courtesy of Cypora Cohen.

Quillen lecturing.

varieties X [4]. This inspired Atiyah and Hirzebruch to create topological K-theory, taking X to be a topological space and making $K(X)$ the degree zero term, $K^0(X)$, of a generalized cohomology theory with groups $K^n(X)$ $(n \geq 0)$ [1].

When X is an affine scheme, $X = \mathrm{Spec}(A)$, the algebraic vector bundles from which Grothendieck formed $K(X)$ correspond to finitely generated projective A-modules (Serre [11]), and the same applies to topological $K(X)$ when X is compact Hausdorff and $A = C(X)$ is the ring of continuous functions (Swan [12]). This suggested introducing the Grothendieck group[4] $K_0(A)$ of finitely generated projective A-modules, a definition sensible for *any* ring A (not necessarily commutative). This extra algebraic (and nongeometric) generality was not frivolous, since topologists had identified obstructions to problems in homotopy theory that reside in groups K_0 of the integral group ring, $\mathbb{Z}\pi$ of some fundamental group π (see Wall [13]).

It was natural then to seek some algebraic analogue of topological K-theory, composed of groups $K_n(A)$ $(n \geq 0)$. There was no obvious way to do this, but somewhat ad hoc methods succeeded in making the first two steps. First came the definition (Bass and Schanuel [3]) of $K_1(A) = GL(A)/E(A)$, where $GL(A)$ is the infinite general linear group and $E(A)$ is its commutator subgroup, known to be generated by the elementary matrices in $GL(A)$ (Whitehead [14]). Considerations recommending this definition of K_1 included natural functorial relations with K_0, and connections again with topology, where Whitehead torsion invariants in simple homotopy theory reside in groups of the form $Wh(\pi) = K_1(Z\pi)/(\pm\pi)$ [14].

When A is the ring of integers in a number field, K_0 is related to the ideal class group, K_1 to the group of units, and relative K_1 groups hold the answer to the classical congruence subgroup problem for $SL_n(A)$ $(n \geq 3)$ (see [2]).

[4] *The switch to subscript is because of the contravariance between X and A.*

The second step was the definition (Milnor [6]) of $K_2(A) = H_2(E(A), \mathbb{Z})$, the kernel of the universal central extension $St(A) \twoheadrightarrow E(A)$, where the "Steinberg group" $St(A)$ is presented by elementary generators and relations. Again, this had good functorial properties, and calculations for number fields showed deep relations with explicit reciprocity laws. Also, Hatcher ([5]) found topological connections of K_2 to problems in pseudoisotopy.

So while these algebraic K_0, K_1, and K_2 were only the first steps of a still unknown general theory, they already exhibited sufficiently interesting connections with algebraic topology, algebraic geometry, and number theory, as well as unmentioned connections with operator algebras, so that the quest for a full-blown algebraic K-theory seemed like a promising investment. In fact several people (Gersten, Karoubi-Villamayor, Swan, Volodin) produced candidates for higher algebraic K-functors. However their nature and the relations among them were not completely understood, and there were no extensive calculations of them for any ring A.

So this was the state of algebraic K-theory around 1970: a theory still in a fragmentary state of hypothetical development but already yielding several interesting applications that drew potential clients from diverse parts of mathematics. This led me to assemble this motley group of developers and consumers to seek some possible convergence. A two-week conference was convened on the pleasant campus of the Battelle Memorial Institute in Seattle. The seventy participants included Spencer Bloch, Armand Borel, Steve Gersten, Alex Heller, Max Karoubi, Steve Lichtenbaum, Jean-Louis Loday, Pavaman Murthy, Dan Quillen, Andrew Ranicki, Graeme Segal, Jim Stasheff, Dick Swan, John Tate, Friedhelm Waldhausen, and Terry Wall. As I wrote in the Introduction to the conference proceedings [BC],

> "…a large number of mathematicians with quite different motivations and technical backgrounds had become interested in aspects of algebraic K-theory. It was not altogether apparent whether the assembling of these efforts under one rubric was little more than an accident of nomenclature. In any case it seemed desirable to gather these mathematicians, some of whom had no other occasion for serious technical contact, in a congenial and relaxed setting and to leave much of what would ensue to mathematical and human chemistry."

The experiment was, I think, a dramatic success, beyond all expectations. It would not be unreasonable to call it the "Woodstock" of algebraic K-theory, and the superstar performer was undeniably Daniel Quillen. He came with two successful constructions of higher algebraic K-theory, the "+-construction", achieved prior to the Battelle Conference, and the "Q-construction", unveiled at the conference itself.

The +-construction defined

$$K_n^+(A) = \pi_n(BGL(A)^+) \qquad n \geq 1,$$

where $BGL(A)^+$ is a modification of the classifying space $BGL(A)$, sharing the same homology and having fundamental group

$$K_1(A) = GL(A)/E(A).$$

Quillen checked further that

$$\pi_2(BGL(A)^+) = K_2(A),$$

thus providing one main motivation for this definition. The other motivation was that, in the case of finite fields, Quillen had shown that the homotopy of $BGL(Fq)^+$ coincides with that of the homotopy fiber of

$$\Psi^q - Id : BU \to BU,$$

which arose in Quillen's proof of the Adams conjecture [7]. The latter thus provided a complete calculation of the K-theory of finite fields.

While the +-construction provided a major advance, it suffered from two related limitations. First, it did not directly account for $K_0(A)$. Second, and more importantly, it did not bring with it the basic computational tools that had proved effective with the lower K_n's ($n = 0, 1, 2$). To overcome this, one needed a definition of $K_n^Q(C)$ ($n \geq 0$) for additive categories C with exact sequences, in the spirit of Grothendieck's original definition. (For a ring A, to get $K_n^Q(A)$, one would take C to be the category of finitely generated projective A-modules.) This was accomplished by the "Q-construction", which defined $K_n^Q(C) = \pi_{n+1}(BQC)$, where BQC is the classifying space (defined for any category) of a new category QC (the Q-construction) invented by Quillen. This definition was validated by a spectacular cascade of theorems and foundational methods:

- *Consistency*: $K_n^Q(A) = K_n(A)$ for $n = 0, 1, 2$, and $K_n^Q(A) = K_n^+(A)$ for $n \geq 1$. Hence one defines $K_n(A)$ to be $K_n^Q(A)$ for all $n \geq 0$.
- *Resolution*: If $C' \subseteq C$ and every object has a finite C'-resolution, then $K_n(C') \to K_n(C)$ is an isomorphism.
- *Dévissage*: If $C' \subseteq C$ and every object has a finite filtration with subquotients in C', then $K_n(C') \to K_n(C)$ is an isomorphism.
- *Localization*: If C is an abelian category and C' is a Serre subcategory, then there is a localization exact sequence relating $K_n(C)$, $K_n(C')$, and $K_n(C/C')$.
- *Homotopy invariance*: If A is noetherian, then $K'_n(A[t]) = K'_n(A)$, where $K'_n(A)$ is the K-theory of the category of finitely generated A-modules (which agrees with $K_n(A)$ for A regular, by the Resolution Theorem).
- *Fundamental Theorem*: There is a natural exact sequence, $0 \to K_n(A) \to K_n(A[t]) \oplus K_n(A[t^{-1}]) \to K_n(A[t;t^{-1}]) \to K_{n-1}(A) \to 0$.
- *Algebraic geometry*: A host of theorems applying to the K-theory of schemes, including calculations, and relations to the Chow ring.

All of this, and more, was accomplished essentially from scratch, in sixty-three double-spaced pages [8]. It is a stunning composition of concepts, techniques, and applications that one would normally expect from the work of many mathematicians over a decade or more. It brought algebraic K-theory from gestation to young adulthood in one awesome leap, and there was more. In one lecture Quillen provided a complete proof, with elegant new methods, of the finite generation of the K-groups of rings of algebraic integers [9]. Later Quillen, in a display of technical virtuosity, proved Serre's so-called "Conjecture" that projective modules over polynomial algebras are free [10].

Individual mathematicians are often characterized as either theory builders or problem solvers. Quillen was a virtuoso in both modes. Like Grothendieck, he was disposed to solve concrete problems not head on, but by finding and mobilizing just the right general concepts, to make arguments flow with almost a mathematical inevitability. But if Grothendieck's style was perhaps Wagnerian, Quillen's was closer to Mozart. He was personally modest, but amiable, and he was a magnificent expositor, kind and edifying to his audience, leaving nothing superfluous, nothing one would want to change, but much from which one continues to learn. It was a personal pleasure and privilege to witness him in action.

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Ken Brown

I had the privilege of being Dan's first official Ph.D. student, although Eric Friedlander preceded me as Dan's unofficial student. In the late summer of 1970, Eric and I were talking in the MIT lounge when Dan walked in, having recently returned from two years abroad. Eric called him over and said, "Hey, Dan, I've got a student for you." Dan asked me what I was interested in, and I nervously told him some things I had been thinking about and what I hoped to prove. He let me down as gently as he could, saying, "That's a good idea; unfortunately it's been done already. But I've got a problem that you might like."

He then gave me a spontaneous one-hour lecture on the higher algebraic *K*-theory that he had been developing. By that point he had defined the *K*-groups via the "plus construction", and he had computed them for finite fields. But he hadn't been able to prove the basic theorems about them that he was sure should be true (various isomorphisms, long exact sequences, etc.). His idea was that there should be an alternative definition of the *K*-groups as a very fancy kind of sheaf cohomology, and the theorems would follow easily. My task, if I chose to accept it, was to develop that sheaf cohomology theory.

I worked on this for a couple of months and then went to Dan's office to tell him I had an idea. I started by telling him I had been reading his Springer Lecture Notes *Homotopical Algebra*. He smiled and said, "Why are you reading *that*? I should never have written it. I was trying to be like Grothendieck, and I couldn't pull it off." [I think history has proven him wrong.] But he listened carefully as I told him how I thought homotopical

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Three Oxford Fields Medalists: Michael Atiyah, Simon Donaldson, and Quillen.

algebra might lead to a solution to the problem he had proposed. He was very encouraging in spite of his initial skepticism, and he gave me a wealth of ideas as to how I could continue my work.

Those first two meetings with Dan were typical. He was always generous with his time, and he always freely shared his ideas, even if not fully developed, about his work in progress. He also very openly talked about his perceived weaknesses. In my talks with him I got many spontaneous lectures on a variety of subjects that he thought I should know about.

I only saw Dan a handful of times after getting my degree in 1971 and leaving MIT. But, whenever I did get back, he would invite me to spend a day at his house, where he would tell me about his current work and show me his private handwritten notes. He would also feed me lunch and openly wonder how I could possibly eat a sandwich without a glass of milk. His devotion to his family was always evident during these lunches.

Dan Quillen was everything I could possibly have hoped for in a thesis advisor and mentor. It is impossible to express in a few words how much he did for me. Although we lost touch with one another in later years, I will always look back with fondness on the time I spent with him early in my career.

Joachim Cuntz

I first met Dan at a conference (I believe it was in 1988). At that time, following his first paper with Jean-Louis Loday, he had already written several papers exploring different approaches to cyclic homology and different descriptions of cyclic cocycles. On the other hand, in a paper with Alain Connes, I had at that time studied

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a description of cyclic cocycles on the basis of (even or odd) traces on certain universal algebras constructed from a given algebra A. It became clear that we were working on similar questions. I also immediately found Dan congenial and I like to think that this feeling was reciprocal. Dan was a very nice person and definitely not spoiled by his fame. In the beginning of 1989 I wrote a letter to him describing some considerations about connections between our work and possible ways to proceed from there. He wrote back saying that he had made progress in the same direction and suggested pooling our efforts. I felt of course honored by that proposal. Nevertheless, some time passed before we really started making progress and exchanging more letters (email of course already existed and we used it too, but still we adopted to some extent the old-fashioned method—also partly because at that time, unlike Dan, I was not fluent with TeX). Then we also exchanged visits. Dan came to Heidelberg several times and I went to Oxford several times (on which occasions Dan, together with his wife Jean, proved to be an excellent host). At that time, Dan was also strongly interested in C^*-algebras. From time to time he would ask me a technical question about C^*-algebras.

On one occasion when Dan visited Heidelberg, we went on a bicycle tour through the Neckar Valley. Originally we had planned to go for a distance of forty-five kilometers along the river and to take a train back. But when we arrived at the train station in Zwingenberg, it was still relatively early, and when I asked Dan, he agreed that we could still continue by bike a little bit. The same procedure repeated itself at the following station and so on, so that, in the end, we had gone back by bike the entire way to Heidelberg. At that point, we had done nearly 100 kilometers by bike. For an inexperienced cyclist this was quite a feat. But I remember that Dan was really tired and could hardly move the next day. Another time, he impressed me by playing (very well!) on my piano pieces in the style of Haydn and Mozart or other composers, which he had created himself and which really conveyed the spirit of those composers. It seems to me that this was another example for his wonderful sense of structures (this time within music).

In the beginning of our collaboration, I contributed mainly computations and some more pedestrian considerations. I was surprised how, in his hands, these developed into a big and powerful machinery. For instance, I had in my computations come across and used a natural projection operator on the cyclic bicomplex. Not much later, Dan sent me several chapters containing a striking and fundamental interpretation of this operator as the projection onto the generalized eigenspace of the Karoubi operator for the eigenvalue 1. Dan had an amazing gift in recognizing structures in formulas and computations. Thus he also embedded our computations into the powerful formalism of using quasifree extensions of a given algebra. I remember that he was quite modest about this achievement. He told me, "This result is only due to my training which made this way of thinking about the situation unavoidable for me." Finally, after nearly five years, in the paper "Cyclic homology and nonsingularity" (*J. Amer. Math. Soc.*, 1995) we had reached the culmination of the first phase of our collaboration. This paper contains a new description of cyclic homology, of cohomology, and of the bivariant theory (which has since become a basis for cyclic theories for algebras with additional structures, such as the entire and local theories or the equivariant theory) and gives a satisfactory unifying treatment for all the ideas which had started our collaboration. Also, we had two other long papers that developed the general framework underlying the construction. I think that, at that point, we both considered this as being the successful end of our collaboration (and in fact I think we were both, but especially Dan, a little bit relieved, because the project had developed into something much bigger and more time consuming than we had originally planned). However, not very much later, we realized that the universal extension algebra JA, associated with an algebra A, which plays a basic role in our approach, has another important feature. While not being H-unital in the sense of Wodzicki, it has a property (we called it approximately H-unital) which makes it amenable to an argument in the spirit of Wodzicki to show that it satisfies excision in periodic cyclic cohomology. The excision problem in periodic cyclic theory had been on my mind for many years. Shortly after, we realized that this property is in fact shared by any ideal in a quasifree algebra. This observation then led directly to a proof of excision in periodic cyclic cohomology in the general case. When these ideas came up, Dan immediately came to Heidelberg to discuss the details. Our collaboration was thus revived and continued until we had worked out the complete proof for excision in periodic cyclic theory (homology, cohomology, and bivariant); cf. "Excision in bivariant periodic cyclic cohomology", *Invent. Math.* **127** (1997). Altogether we have four joint papers and two joint announcements. I feel strongly in Dan Quillen's debt.

Photo by Olli Lehto, used with permission.

Quillen at the Fields Medal award ceremony, Helsinki, 1978, with Pierre Deligne, Charles Fefferman, and Rolf Nevanlinna, who awarded the medals to the three.

Jeanne Duflot

Entering MIT as a new graduate student, and indeed entering the high-powered world of East Coast academics, was daunting to me, a diffident Texan to whom someone with a heavy Boston accent not only seemed to be speaking a foreign language but who also seemed to think I was. In a happy change of fortune, one of my professors in my first year at MIT was Daniel Quillen. That year, he was teaching the first-year graduate course in algebra, and at that particular moment in time, with that particular professor, this meant the first semester was an illuminating series of lectures on homological algebra and sheaf theory; the second semester was a complete course on commutative algebra. I was also taking a course on algebraic topology, and the resulting juxtaposition of inspirations, as well as my starry-eyed appreciation of the unparalleled lucidity of Professor Quillen's lectures, emboldened me to ask him to be my dissertation advisor, to which he kindly consented, after I had explained my naive hope of doing research in algebraic topology and commutative algebra simultaneously. I was, of course, completely unaware that he had done groundbreaking work uniting these fields; cf. the series "The spectrum of an equivariant cohomology ring, I, II", *Ann. of Math.* **94** (1971), no. 3.

Having him as an advisor was wonderful, mostly because of the remarkable clarity of his explanations and reasonings when he talked to me about mathematics. By clarity, I don't mean that I understood everything he told me immediately, far from it, but I intend rather a quality of clearing obscurities and lighting up new ways of thinking. After leaving MIT with my Ph.D., I continued to work in the application of concepts from commutative algebra applied to equivariant cohomology for a few years, then moved on to other diversions. However, I've recently come back to thinking about that topic, even passing on my comparatively meager expertise therein while directing the recent Ph.D. of one of my students. Quillen's mathematical lessons and remarks of thirty years ago at MIT, as well as the remembrance of the quality of his mentorship as an advisor, were then with me.

He was devoted to his family; one child was born during my time at MIT, and I remember him apologizing for grogginess more than once, due to late nights up with the baby—an affliction that at the time I could not even imagine, but now I laugh at the recollection, having had first-hand experience. He was a teetotaler, and he won the Fields Medal while I was his student; I was awestruck and could barely speak to him at our first meeting after I found out about this and was floored when he offered me a bottle of champagne that had been given to him by a congratulatory colleague, explaining that he did not drink alcoholic beverages. I gratefully accepted it and drank it with some fellow students. I think it was quite good champagne, but I was not an expert. He did seem to favor wearing a particular plaid shirt a lot, and indeed, when I read over Graeme Segal's obituary for Professor Quillen in *The Guardian*, I noticed with both sadness and a smile that in the accompanying picture he was wearing exactly that plaid shirt and looked exactly the way I remember him.

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Jean-Louis Loday

I was fortunate to begin my mathematical career in the early seventies when Quillen opened up the book of algebraic *K*-theory. As a result, I could participate in the development of higher *K*-theory and meet Quillen on several occasions. Then, later on, I had the opportunity to lecture at Oxford (UK) with Quillen present in the audience. A few weeks later I received a friendly letter from him telling me how to finish the work that I had begun. It started a fruitful collaboration on cyclic homology, and thus Quillen played a major role in my mathematical life. But there is more. When I wrote my master's thesis in Paris under the supervision of Jean-Louis Verdier, I had to deal with the van Kampen theorem. It turns out that the question was strongly related to the first published paper of Quillen [1]. This concise three-page paper is a jewel. It has the characteristics of a French theater piece by Corneille or Racine: one notion, one result, one argument. Here, the notion

The late Jean-Louis Loday was professor of mathematics at the Institut de Recherche Mathématique Avancée.

is *simplicial group*; the result is *any bisimplicial group gives rise to a homotopy spectral sequence abutting to the same object*; the argument is *look at the diagonal simplicial group*.

Since I mentioned the first paper of Quillen, I will also comment on one of his last ones [2], published thirty years later. Since our collaboration on cyclic homology, Quillen had the idea that nonunital associative algebras should have specific homology and homotopy invariants. The classical way to handle nonunital algebras was to add a unit formally, a trick that he found too naive to be useful. We did get some results for Hochschild and cyclic homology, but his aim was to produce K-theoretic invariants. To understand the task at hand, it suffices to recall that K_1 is a generalization of the determinant and that the determinant of an invertible matrix lives in the group of units of the ring. But without a unital element (i.e., 1) there are no units. In fact Quillen did not begin with K_1, but with K_0, and this is precisely the subject of this last paper [2]. The subject is now an orphan.

During the period between these two papers Quillen produced an immense body of work, about which you may read in other contributions, as well as in the small tribute that you can find on my homepage: `http://www-irma.u-strasbg.fr/~loday/DanQuillen-par-JLL.pdf`

Quillen influenced my mathematical life deeply. It has been a tremendous opportunity to read his papers, to hear him speak, and to collaborate with him.

Thanks, Dan!

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Barry Mazur

I feel grateful to Dan for this gift of his: in each of the areas of mathematics in which he worked, his vision always had the marvelous consequence of "opening" the subject if it were brand new and of "opening up" the subject if it had a previous tradition; the mathematics became all the fresher, all the larger, all the more vibrant, and yet all the more unified, once he got to it. This is true, for example, of algebraic K-theory, of course, of his work on the "Quillen determinant", and of his striking "harnessing" of the power of complex cobordism theory by making that celebrated connection to formal groups. I'm, of course, not alone in feeling this gratitude.

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Andrew Ranicki

The name of Quillen featured already in the very first topology seminar I attended as a graduate student in Cambridge in 1970. It was given by Frank Adams, who talked about the then-recent work of Quillen and Dennis Sullivan on the solution of the Adams conjecture. Frank spoke about both Dan and Dennis with an unusual amount of respect!

I actually met both Quillen and Sullivan at the same time, when I spent a year at IHES, 1973–1974. Sullivan's interest in surgery theory was naturally greater than Quillen's. Both Dan and his wife, Jean, were kind to me, and I was a frequent visitor at Pavillon 8 of the Residence de l'Ormaille. Although I did not talk to Dan all that much about mathematics, there were plenty of other topics, and I was always impressed by his seriousness of purpose and independence of mind, allied with a winning personal modesty. Soon after Dan moved to Oxford in 1984 I invited him and his family to visit us in Edinburgh. I asked him if MIT had proposed to match his Oxford offer. He answered that to do this they would have had to cut his MIT salary by two-thirds!

On my occasional visits to Oxford I would always call on the Quillens, who were as kind to me as they had been at IHES. There was in fact one occasion when Dan and I did talk about mathematics, Over dinner I mentioned that I had worked out a formula for the projective class of a finitely dominated chain complex. He asked me to come to his office the next day and explain it to him in detail—it turned out that he needed just such a formula for his work on K_0 of nonunital rings. I was most flattered! But I should have spent much more time talking to Dan about his mathematics. Too late now.

Dennis Sullivan

My interactions with Dan Quillen were concentrated in the late sixties and early seventies in Princeton, Cambridge, and Paris.

In our mathematical encounters the more naive geometric tradition of Princeton topology and the more sophisticated algebraic tradition of Cambridge topology and geometry informed one another. Here are three examples.

(i) Together we pondered the conflict between Steenrod's cellular approximations to the main

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diagonals in products of a cell complex with itself leading to cohomology operations beyond cup product and the Cartan-de Rham differential algebra of smooth forms which is graded commutative and associative.

(ii) Quillen explained to me one day at MIT how the structure of a nilpotent group is built from the abelianization as extensions of quotients of functors applied to it. Quillen's insight was of great utility to me in understanding the algebraic groups that arise in rational homotopy theory. The clarity of his insight was remarkable.

(iii) One day in Princeton I showed Dan an elementary pictorial argument that attaches two and three cells to a space with zero first homology to go on to kill the fundamental group without changing the homology. The discussion arose when he showed me his beautiful computations of the cohomology of $GL\,[n$, finite field]. Quillen made use of this device in the early stages of his constructions of algebraic K-theory.

Our later interactions near Paris at IHES were more about kids and family in the Residence d'Ormaille.

One memory that seems to fit with everything was of a large smooth wooden table situated without chairs in the middle of the main room of Pavillon 8 in the Residence d'Ormaille. On the table hundreds of little shapes were deployed into a dozen or so neat little battalions surrounding a coherent structure emerging in the middle. Dan and a couple of kids and anyone else who might be around were hovering around the table, peering intently at these patterns, muttering softly and hoping to experience the exquisite pleasure of fitting in new parts to the emerging structure. It was serious business with good karma.

Ulrike Tillmann

In 1988, as a visiting graduate student at Oxford, I attended a course on cyclic cohomology. It seemed lectures had never been as clear before: new mathematics was created in front of our eyes, and even to a novice like me it all seemed logical and natural. The lecturer, white haired, in jeans and hand-knitted jumpers frayed at the edges, was Dan Quillen, Waynflete Professor of Pure Mathematics.

One of the first research papers that I had read was the one for which Dan had received the Fields Medal in 1978 as the chief architect of algebraic K-theory. I had also studied his papers on group cohomology though I was still unaware of his landmark contributions in homotopy theory. Most of this work had been done at MIT. But having visited Michael Atiyah and Graeme Segal in Oxford for a year, in 1985 Dan accepted the Waynflete chair that had been vacated a year before by Graham Higman. At first Dan worked on questions motivated by quantum physics, superconnections in particular. Later on he concentrated on the development of cyclic cohomology, and the lectures that I attended were followed by many more on the topic.

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Photo by Richard Schori, used with permission.

Quillen lecturing at the University of Florida, April 2000.

I was told that Dan's work in the subject started when Jean-Louis Loday gave a seminar in Oxford in the early 1980s. Dan was intrigued by the questions it left unanswered, and as a result he and Loday wrote a paper interpreting cyclic cohomology as an infinitesimal version of K-theory. While working on my thesis problem to prove a version of the Novikov conjecture, this was one of a handful of papers that I kept referring back to for information and inspiration.

In the late 1980s and 1990s, together with Joachim Cuntz from the University of Münster, Dan, in a series of nearly a dozen papers, laid out a purely algebraic, noncommutative theory of differential forms and established their homological properties. These papers were first written as lectures, many of which I attended, then back in Oxford as a young member of the faculty. My own work had moved on to different topics, but it was always fascinating and educational to watch a master.

For many years until his retirement in 2006, Dan had been an editor of the Oxford-based journal *Topology*, a leading journal in the field since its foundation by Michael Atiyah and Ioan James in 1962 and until the resignation of its editors and the founding of the new *Journal of Topology* in 2007. Dan also had been part of the LMS-supported regional K-Theory Days for nearly ten years. These had been initiated by his only Oxford DPhil student, Jacek Brodzki. In 2001, there was a short conference in honor of Dan's sixtieth birthday, and in 2006, a special *K*-Theory Day marked his sixty-fifth birthday and retirement. Two years later, he and his wife Jean moved to Florida.

Jean was of course the one who had knitted Dan's jumpers. They had met as undergraduates majoring in mathematics at Harvard and have six children with many more grandchildren. The move across the Atlantic from MIT to Oxford was no doubt eased by the fact that as a professional violist and violin teacher Jean found Oxford very amenable and full of opportunities.

Jean Quillen

Dan was born on June 22, 1940, in Orange, New Jersey. From an early age his intellectual abilities and particular approach to the world were apparent. His mother used to enjoy telling how Dan didn't talk as a baby until he surprised everyone by being able to speak full sentences. He spent his high school years at Newark Academy, a private school in New Jersey where he had a full scholarship. His mother was a driving force in his young life, pushing others to recognize his abilities. She discovered that Harvard had a program where promising incoming students were able to start their studies a year early by skipping their last year of high school. She had him apply and I suspect rewrote all his essays (Dan struggled throughout his life with words and writing). At any rate Harvard accepted him to begin in September 1957 when he was only seventeen, whereupon Newark Academy had a problem: whether to give him his diploma or let him go to Harvard without the high school diploma. They awarded him his diploma.

I met Dan the following year when I was a first-year student at Harvard/Radcliffe. For some reason Dan decided to take the first-year chemistry course in his second year. I apparently smiled at him. We were married three years later on June 3, 1961.

We have six children, twenty grandchildren at last count, and one great-grandchild.

I remember Dan as an incredibly motivated and bright young man. Mathematics was his first love from the age of twelve or thirteen when his father gave him a calculus book. He briefly toyed with chess but found it too intense (!) and thereafter a career in mathematics was the only option for him.

Dan and I had many study dates during our undergraduate years; he seemed to absorb his undergraduate mathematics courses like a sponge. He learned and understood all of every course he took. In fact I noticed that he could reproduce by memory nearly every theorem and proof. He also had the talent of being able to identify what was important in a subject. Once when I got behind in a course, he managed to teach me all the important points in only three days. Not only did I get an A on the exam, but I also noticed a misprint in the examination paper!

Although I have little experience of Dan's classroom teaching, I remember what a wonderful teacher he was one-to-one. He never made me feel stupid; he just accepted what I knew and built upon it. This kindness in teaching also extended into other areas of my life. Dan taught me to cook by happily eating the mistakes and by being delighted by dishes that came out well. Once I managed to turn a cake over into the oven. He fended off my tears by cheerfully scooping it up, putting it on a plate, and announcing to the children that we were having an "upside-down-in-the-oven cake".

Dan had three degrees from Harvard: BA (magna cum laude), 1961; MA, 1962; Ph.D., 1964. Dan was awarded his BA with only (!) a magna cum laude because he nearly failed one of his required distribution courses. I remember typing his Ph.D. thesis: Dan in one room producing pages, me in the next room with a hired electric typewriter. What we did with the two babies I cannot imagine! Naturally the thesis was produced about three minutes after it was due. Shortly thereafter he was offered a position at the Massachusetts Institute of Technology.

Dan also spent a number of years at other institutions, mainly because he was interested in working with different mathematicians. He twice took a year sabbatical in France at the IHES in Bures-sur-Yvette. We spent a year at the Institute for Advanced Study in Princeton, a year at the Max Plank Institute in Bonn in Germany, and a year at Oxford University, England. I enjoyed the times abroad. It was a challenge and I learned to speak French, a little German, and British English.

Dan spoke of the year at Princeton as a particularly productive time for him mathematically. It was during our time there that Dan worked on and solved the Adams conjecture. It was also in Princeton where he first came under the influence of Sir Michael Atiyah.

After the year in Princeton we returned to MIT where Dan had been made a full professor. With Dan's support I was able to study music part-time at the New England Conservatory of Music. He arranged his teaching schedule to make it possible. I am so grateful that he gave me that support. That year Dan also started to do most of his research at home, partly to help with the children and partly, I suspect, because nobody could interrupt him at home. We used to close five

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Photo courtesy of Cypora Cohen.

Quillen with daughter, Cypora.

doors between him and my music practice, and woe to any child who left a door open.

It was Dan's relationship with Atiyah that first brought us to Oxford. During the year in Bonn Dan said to me, "I'm in the wrong country." I said, "What country should you be in?" He said he wanted to be in Oxford, partly because he was interested in something that Michael Atiyah was working on. After a year in Oxford we returned to Boston. About six months later Dan was in Oxford giving a talk when Atiyah mentioned that the Waynflete Chair at Magdalen College was opening up and asked if he would consider moving to Oxford permanently. He phoned me. I said, "Yes, please," and that's how we came to Oxford to stay.

Dan worked with and was influenced by many people throughout the years: Grothendieck and Deligne in France in the 1970s and later Loday and Connes. In the early years there were the mathematicians at Harvard and in later years colleagues at MIT and in Germany and England. He often talked to me about those whose work he admired. There was never any jealousy, just admiration for work well done.

Alzheimer's is a cruel disease. The first sign of the disease was Dan's inability to understand mathematics. He was aware of this, and you can imagine his agony. He was such a private person that he never spoke about this. Because of his suffering, in some ways we were prepared to lose him. Although Dan's first love was mathematics, he was also a kind and devoted husband. I will miss him very much and so, I think, will many others.