George Mackey 1916–2006

Robert S. Doran and Arlan Ramsay

Robert S. Doran

Introduction

George Whitelaw Mackey died of complications from pneumonia on March 15, 2006, in Belmont, Massachusetts, at the age of ninety. He was a remarkable individual and mathematician who made a lasting impact not only on the theory of infinite dimensional group representations, ergodic theory, and mathematical physics but also on those individuals with whom he personally came into contact. The purpose of this article is to celebrate and reflect on George's life by providing a glimpse into his mathematics and his personality through the eyes of several colleagues, former students, family, and close friends. Each of the contributors to the article has his or her own story to tell regarding how he influenced their lives and their mathematics.

I first became aware of George Mackey forty-five years ago through his now famous 1955 Chicago lecture notes on group representations and also through his early fundamental work on the duality theory of locally convex topological vector spaces. I had taken a substantial course in functional analysis, and Mackey's Chicago notes were a rather natural next step resulting from my interest in C*-algebras and von Neumann algebras—an interest instilled in me by Henry Dye and Sterling Berberian, both inspiring teachers who, at the time, were themselves fairly young Chicago Ph.D.'s. I purchased a copy of Mackey's notes through the University of Chicago mathematics department and had them carefully bound in hard cover. They became my constant companion as I endeavored to understand this beautiful and difficult subject. In this regard it is certainly not an exaggeration to say that Mackey's notes were often the catalyst that led many mathematicians to study representation theory. As one important example I mention J. M. G. (Michael) Fell, who, with the late D. B. Lowdenslager, wrote up the original 1955 Chicago lecture notes. Fell makes it clear in the preface of our 1988 two-volume work [1, 2] on representations of locally compact groups and Banach *-algebraic bundles that the basic direction of his own research was permanently altered by his experience with Mackey's Chicago lectures. The final chapter of the second volume treats generalized versions of Mackey's beautiful normal subgroup analysis and is, in many respects, the apex of the entire work. It is perhaps safe to say that these volumes stand largely as a tribute to George Mackey's remarkable pioneering work on induced representations, the imprimitivity theorem, and what is known today as the "Mackey Machine".

In 1970 I contacted George to see if he would be interested in coming to TCU (Texas Christian University) to present a series of lectures on a topic in which he was currently interested. In spite of being very busy (at the time, as I recall, he was in Montecatini, Italy, giving lectures) he graciously accepted my invitation, and I enthusiastically submitted a grant to the NSF for a CBMS (Conference Board of the Mathematical Sciences) conference to be held at TCU with George as the principal

Robert S. Doran is Potter Professor of Mathematics at Texas Christian University. His email address is `r.doran@tcu.edu`.

Arlan Ramsay is emeritus professor of mathematics at the University of Colorado at Boulder. His email address is `ramsay@colorado.edu`.

lecturer. Although the grant was carefully prepared, including a detailed description provided by George of his ten lectures titled "Ergodic theory and its significance in statistical mechanics and probability theory", the grant was not funded. Given George's high mathematical standing and reputation, I was of course surprised when we were turned down. Fortunately, I was able to secure funding for the conference from another source, and in 1972 George came to TCU to take part as originally planned. True to form he showed up in Fort Worth with sport coat and tie (even though it was quite hot) and with his signature clipboard in hand. Many of the leading mathematicians and mathematical physicists of the day, both young and old, attended, and the result was a tremendously exciting and meaningful conference. To put the cherry on top, Mackey's ten conference lectures were published in [3], and he received a coveted AMS Steele Prize in 1975 for them.

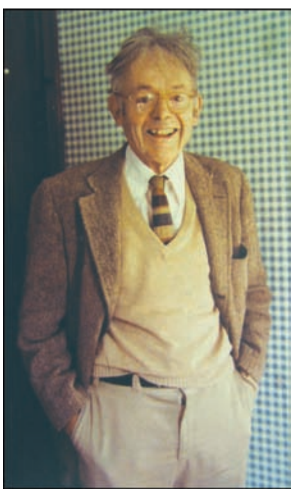

Photo courtesy of Ann Mackey.

George Mackey, on vacation in 1981.

From these early days in the 1970s George and I became good friends, and we would meet for lunch on those occasions that we could get together at meetings and conferences (lunches are a pattern with George, as you will note from other writers). It seemed that we agreed on most things mathematical—he of course was the master, I was the student. On the other hand, he particularly liked to debate philosophical and spiritual matters, and we held rather different points of view on some things. These differences in no way affected our relationship. If anything, they enhanced it. Indeed, George honestly enjoyed pursuing ideas to the very end, and he doggedly, but gently, pushed hard to see if one could defend a particular point of view. I enjoyed this too, so it usually became a kind of sparring match with no real winner. The next time we would meet (or talk on the phone) it would start all over again. Although at times he could be somewhat brusque, he was always kind, compassionate, and highly respectful, and, I believe, he genuinely wanted to understand the other person's viewpoint. It seemed to me that he simply had a great deal of difficulty accepting statements that he personally could not establish through careful logical reasoning or through some kind of mathematical argument. Perhaps this, at least in part, is why he had a particularly strong affinity for mathematics, a discipline where proof, not faith, is the order of the day.

Following the biography below, Calvin Moore provides an overview of George's main mathematical contributions together with some personal recollections. Then each of the remaining writers shares personal and mathematical insights resulting from his or her (often close) association with George. Arlan Ramsay and I, as organizers, are deeply grateful to all of the writers for their contributions, and we sincerely thank them for honoring an esteemed colleague, mentor, family member, and friend who will be greatly missed.

Biography of George W. Mackey

George Whitelaw Mackey was born February 1, 1916, in St. Louis, Missouri, and died on March 15, 2006, in Belmont, Massachusetts. He received a bachelor's degree from the Rice Institute (now Rice University) in 1938. As an undergraduate he had interests in chemistry, physics, and mathematics. These interests were developed during his early high school years. After briefly considering chemical engineering as a major his freshman year in college, he decided instead that he wanted to be a mathematical physicist, so he ended up majoring in physics. However, he was increasingly drawn to pure mathematics because of what he perceived as a lack of mathematical rigor in his physics classes. His extraordinary gift in mathematics became particularly clear when he was recognized nationally as one of the top five William Lowell Putnam winners during his senior year at Rice. His reward for this accomplishment was an offer of a full scholarship to Harvard for graduate work, an offer that he accepted.

Photo courtesy of Ann Mackey.

A young George Mackey.

He earned a master's degree in mathematics in 1939 and a Ph.D. in 1942 under the direction of famed mathematician Marshall H. Stone, whose 1932 book *Linear Transformations in Hilbert Space* had a substantial influence on his mathematical point of view. Through Stone's influence, Mackey was able to obtain a Sheldon Traveling Fellowship allowing him to split the year in 1941 between Cal Tech and the Institute for Advanced Study before completing his doctorate. While at the Institute he met many legendary figures, among them Albert Einstein, Oswald Veblen, and John von Neumann, as well as a host of young Ph.D.'s such as Paul Halmos, Warren Ambrose, Valentine Bargmann, Paul Erdős, and Shizuo Kakutani.

After receiving his Ph.D. he spent a year on the faculty at the Illinois Institute of Technology and then returned to Harvard in 1943 as an instructor in the mathematics department and remained there until he retired in 1985. He became a full professor in 1956 and was appointed Landon T. Clay Professor of Mathematics and Theoretical Science in 1969, a position he retained until he retired.

His main areas of research were in representation theory, group actions, ergodic theory, functional analysis, and mathematical physics. Much of his work involved the interaction between infinite-dimensional group representations, the theory of operator algebras, and the use of quantum logic in the mathematical foundations of quantum mechanics. His notion of a system of imprimitivity led naturally to an analysis of the representation theory of semidirect products in terms of ergodic actions of groups.

He served as visiting professor at many institutions, including the George Eastman professor at Oxford University; the University of Chicago; the University of California, Los Angeles; the University of California, Berkeley; the Walker Ames professor at the University of Washington; and the International Center for Theoretical Physics in Trieste, Italy. He received the distinguished alumnus award from Rice University in 1982 and in 1985 received a Humboldt Foundation Research Award, which he used at the Max Planck Institute in Bonn, Germany.

Mackey was a member of the the National Academy of Sciences, the American Academy of Arts and Sciences, and the American Philosophical Society. He was vice president of the American Mathematical Society in 1964–65 and again a member of the Institute for Advanced Study in 1977.

His published works include *Mathematical Foundations of Quantum Mechanics* (1963), *Mathematical Problems of Relativistic Physics* (1967), *Induced Representations of Groups and Quantum Mechanics* (1968), *Theory of Unitary Group Representations* (1976), *Lectures on the Theory of Functions of a Complex Variable* (1977), *Unitary Group Representations in Physics, Probability, and Number Theory* (1978), and numerous scholarly articles.

George's final article [4], published in December of 2004, contains in-depth descriptions of some of the items mentioned in this biography as well as his interactions with Marshall Stone and others while he was a Harvard graduate student. He was not in good health at the time, and his devoted wife, Alice, typed some of his handwritten notes and helped get the article completed and in print. Dick Kadison and I, as editors of the volume in which the paper appeared, are extremely grateful for her kindness and help.

Ph.D. Students of George Mackey, Harvard University

Lawrence Brown, 1968
Paul Chernoff, 1968
Lawrence Corwin, 1968
Edward Effros, 1962
Peter Forrest, 1972
Andrew Gleason, 1950
Robert Graves, 1952
Peter Hahn, 1975
Christopher Henrich, 1968
John Kalman, 1955
Adam Kleppner, 1960
M. Donald MacLaren, 1962
Calvin Moore, 1960
Judith Packer, 1982
Richard Palais, 1956
Arlan Ramsay, 1962
Caroline Series, 1976
Francisco Thayer, 1972
Seth Warner, 1955
John Wermer, 1951
Thomas Wieting, 1973
Neal Zierler, 1959
Robert Zimmer, 1975

References

[1] J. M. G. Fell and R. S. Doran, *Representations of *-algebras, locally compact groups, and Banach *-algebraic bundles, Vol. 1 (General representation theory of groups and algebras)*, Pure and Applied Mathematics 125, Academic Press, New York, 1988.

[2] ———, *Representations of *-algebras, locally compact groups, and Banach *-algebraic bundles, Vol. 2 (Banach *-algebraic bundles, induced representations, and the generalized Mackey analysis)*, Pure and Applied Mathematics 126, Academic Press, New York, 1988.

[3] G. W. Mackey, Ergodic theory and its significance in statistical mechanics and probability theory, *Advances in Mathematics* **12** (1974), 178–268.

[4] ———, Marshall H. Stone: Mathematician, statesman, advisor and friend, *Operator Algebras, Quantization, and Noncommutative Geometry*, Contemporary Mathematics 365 (eds., R. S. Doran and R. V. Kadison), Amer. Math. Soc., Providence, RI, 2004, pp. 15–25.

Calvin C. Moore

George Mackey's Mathematical Work

After graduating from the Rice Institute (as Rice University was known at the time) in 1938 with a major in physics and a top five finish in the Putnam Exam, George Mackey entered Harvard University for doctoral study in mathematics. He soon came under the influence of Marshall Stone and in 1942 completed a dissertation under Stone entitled "The Subspaces of the Conjugate of an Abstract Linear Space". In this work he explored the different locally convex topologies that a vector space can carry. The most significant result to emerge can be stated as follows. Consider two (say real) vector spaces V and W that are in perfect duality by a pairing

$$V \times W \to R$$

so that each may be viewed as linear functionals on the other. It was obvious that there is a weakest (smallest) locally convex topology on V (or W) such that the linear functionals coming from W (or V) are exactly the continuous ones, called the weak topology. What Mackey proved was that there is a unique strongest (largest) locally convex topology such that the linear functionals coming from W are the continuous ones. This is the topology of convergence of elements of V, now viewed as linear functionals on W uniformly on weakly compact convex subsets of W. All locally convex topologies on V for which the linear functionals from W are exactly the continuous ones lie between these two [Ma1, Ma2]. This topology became known generally as the Mackey topology.

Mackey retained a life-long interest in theoretical physics, no doubt inspired initially by his undergraduate work, and soon after his initial work, he turned his attention to the theorem of Stone [S] and von Neumann [vN] that asserts that a family of operators $p(i)$ and $q(i)$ on a Hilbert space satisfying the quantum mechanical commutation relations $[p(k), q(j)] = i\delta(k, j)I$ is essentially unique. Mackey realized that it was really a theorem about a pair of continuous unitary representations, one U of an abelian locally group A, and the other V of its dual group $\hat{A}$ which satisfied

$$U(s)V(t) = (s, t)V(t)U(s)$$

where (s, t) is the usual pairing of the group and its dual. He showed [Ma3] that such a pair is unique if they jointly leave no closed subspace invariant and in general any pair is isomorphic to a direct sum of copies of the unique irreducible pair. When A is Euclidean n-space, this result becomes the classic theorem about the quantum mechanical commutation relations. He then also saw immediately that there was a version for nonabelian locally compact groups, which appeared in [Ma4], but this is really part of the next phase in Mackey's work, to which we turn now. Mackey initiated a systematic study of unitary representations of general locally compact second countable (and all groups will be assumed to be second countable without further mention) groups, work for which he is most famous. Von Neumann had developed a theory of direct integral decompositions of operator algebras in the 1930s as an analog of direct sum decompositions for finite-dimensional algebras, but he did not publish it until F. I. Mautner persuaded him to do so in 1948.

Adapted to representation theory, direct integral theory became a crucial tool that Mackey used and developed. For representations of finite groups, induced representations whereby one induces a representation of a subgroup H of G up to the group G are an absolutely essential tool. Mackey in a series of papers [Ma5, Ma6, Ma7] defined and studied the process of induction of a unitary representation of a closed subgroup H of a locally compact group G to form the induced representation of G. When the coset space G/H has a G-invariant measure, the definition is straightforward, but when it has only a quasi-invariant measure, some extra work is needed. Mackey developed analogs of many of the main theorems about induced representations of finite groups. In particular he established his fundamental imprimitivity theorem, which characterizes when a representation is induced. The process of induction had appeared in some special cases a year or two earlier in the work of Gelfand and his collaborators on unitary representations of the classical Lie groups.

This imprimitivity theorem states that a unitary representation U of a group G is induced by a unitary representation V of a closed subgroup H of G if and only if there is a normal representation of the von Neumann algebra $L^\infty(G/H, \mu)$ on the same Hilbert space as U (or equivalently that there is a projection-valued measure on G/H absolutely continuous with respect to μ) which is covariant with respect to U in the natural sense. Here μ is a quasi-invariant measure on G/H. Moreover, U and V uniquely determined each other up to unitary equivalence. Mackey's theorem above on unicity of pairs of representations of an abelian group A and its dual group $\hat{A}$ is a special case of applying the imprimitivity theorem to a generalized Heisenberg group built from A and $\hat{A}$.

These results laid the basis for what was to become known as the Mackey little group method, or as some have called it, the Mackey machine, for calculating irreducible unitary representations

Calvin C. Moore is emeritus professor of mathematics at the University of California at Berkeley. His email address is ccmoore@math.berkeley.edu.

of a group knowing information about subgroups. But before this program could get under way, Mackey had to put in place some building blocks or preliminaries. First some basic facts about Borel structures needed to be laid out, which Mackey did in [Ma9]. A Borel structure is a set together with a σ-field of subsets. He identified two kinds of very well-behaved types of Borel structures that he called standard and analytic based on some deep theorems in descriptive set theory of the Polish school. An equivalence relation on a Borel space leads to a quotient space with its own Borel structure. If the original space is well behaved, the quotient can be very nice—one of the two well-behaved types above—or if not, it is quite pathological. If the former holds, then the equivalence relation is said to be smooth. The set of concrete irreducible unitary representations of a group G can be given a natural well-behaved Borel structure, and then the equivalence relation of unitary equivalence yields the quotient space—that is, the set of equivalence classes of irreducible unitary representations, which he termed the dual space $\hat{G}$ of G. If the equivalence relation is well-behaved, $\hat{G}$ is a well-behaved space, and Mackey said then that G had smooth dual. This was a crucial concept in the program.

Mackey developed some additional facts about actions of locally compact groups on Borel spaces. A group action ergodic with respect to a quasi-invariant measure—and ergodicity is a concept that played such a central role in Mackey's work over decades—is one whose only fixed points in the measure algebra are 0 and 1. An important observation is that if the equivalence relation induced on X by the action of G is smooth, then any ergodic measure is concentrated on an orbit of G, and so up to null sets the action is transitive. Another related issue concerned point realizations of actions of a group. Suppose that G acts as a continuous transformation group on the measure algebra $M(X, \mu)$ of a measure space, where X is a well-behaved Borel space. Then, can one modify X by μ-null sets if necessary and show that this action comes from a Borel action of G on the underlying space X that leaves the measure quasi-invariant? In [Ma12] Mackey showed that the answer is affirmative, extending an earlier result of von Neumann for actions of the real line.

Also, by adapting von Neumann's type theory for operator algebras, Mackey introduced the notion of a type I group, by which he meant that all its representations were type I or equivalently all of its primary representations were multiples of an irreducible representation. On the basis of his work in classifying irreducible representations of a group—e.g., calculating $\hat{G}$—Mackey observed that the property of a group G having a smooth dual seemed to be related to and correlated with the absence of non-type I representations of G. Mackey then made a bold conjecture that a locally compact group G had a smooth dual if and only if it is type I. It was not too long before James Glimm provided a proof of this conjecture [Gl].

Another element was to deal with what one would call projective unitary representations of a group G, which are continuous homomorphisms from G to the projective unitary group of a Hilbert space. Such projective representations not only arise naturally in the foundations of quantum mechanics, but as was clear, when one started to analyze ordinary representations, one was led naturally to projective representations. By using a lifting theorem from the theory of Borel spaces, Mackey showed that a projective representation could be thought of as a Borel map U from G to the unitary group satisfying

$$U(s)U(t) = a(s,t)U(st),$$

where A is a Borel function from $G \times G$ to the circle group T that satisfies a certain cocycle identity. For finite groups, it was clear that any projective representation of a group G could be lifted to an ordinary representation of a central extension of G by a cyclic group. In the locally compact case one would like to have the same result but with a central extension of G by the circle group T. If the cocycle above were continuous, it would be obvious how to construct this central extension. Mackey, by a very clever use of Weil's theorem on the converse to Haar measure, showed how to construct this central extension. He also began an exploration of some aspects of the cohomology theory that lay in the background [Ma8].

Mackey's little group method starts with a group G for which we want to compute $\hat{G}$, and it is assumed that N is a closed normal subgroup that has a smooth dual (and is hence now known to be type I). It is assumed that $\hat{N}$ is known, and then G (or really G/N) acts on $\hat{N}$ as a Borel transformation group. Any irreducible representation U of G yields upon restriction to N a direct integral decomposition into multiples of irreducible representations with respect to a measure μ on $\hat{N}$, which Mackey showed was ergodic. Then if the quotient space of $\hat{N}$ by this action is smooth, any ergodic measure is transitive and is carried on some orbit $G \cdot V$ of G on $\hat{N}$. Hence the representation U has a transitive system of imprimitivity based on G/H where H is the isotropy group of V. Hence U is induced by a unique irreducible representation of H, whose restriction to N is a multiple of V, and these can be classified in terms of irreducible representations or projective representations of H/N, which is called the "little group". In an article in 1958 Mackey laid out his systematic theory of projective representations [Ma11]. Mackey's work also builds on Wigner's analysis in 1939 of the special case of unitary representations of the inhomogeneous

Lorentz group [W]. Mackey's little group method, an enormously effective, systematic tool for analyzing representations of many different groups, was used to good effect by many workers, and has been extended in different directions.

In the summer of 1955 Mackey was invited to be a visiting professor at the University of Chicago, and he gave a course that laid out his theory of group representations that we have briefly described. Notes from the course prepared by J. M. G. Fell and D. B. Lowenslager circulated informally for years, and generations of students (including the author) learned Mackey's theory from these famous notes. In 1976 Mackey agreed to publish an edited version of these notes, together with an expository article summarizing progress in the field since 1955 [Ma16]. Of course the Mackey machine runs into trouble when the action of G on $\hat{N}$ is not smooth, and nontransitive ergodic measures appear in the analysis above. In his 1961 AMS Colloquium Lectures [Ma13] Mackey laid out a new approach to this problem and introduced the notion of virtual group. He observed that a Borel group action of a group G on a measure space Y defines a groupoid—a set with a partially defined multiplication where inverses exist. The groupoid consists of $Y \times G$ where the product $(y, g) \cdot (z, h)$ is defined when $z = y \cdot g$, and the product is (y, gh) where it is convenient to write G as operating on the right. This set has a Borel structure and a measure—the measure μ on Y cross Haar measure that has the appropriate "invariance" properties. If the measure μ is ergodic, then Mackey called the construction an ergodic measured groupoid. Mackey realized that different such objects needed to be grouped together under a notion he called similarity, and he defined a virtual group to be an equivalence class under similarity of ergodic measure groupoids. In the case of a groupoid coming from a transitive action of G on a coset space $H\backslash G$ of itself, the similarity notion makes the measured groupoid $H\backslash G \times G$ similar to the measured groupoid that is simply the group H (with Haar measure) and so puts them in the same equivalence class. Hence in this case the transitive measured groupoid is literally a subgroup of G, and Mackey's point here is that it would be very productive to look at a general ergodic action of G as a kind of generalized (or virtual) subgroup of G via the language of groupoids and virtual groups. Then one could begin a systematic study of the representations of virtual groups, induced representations, etc. The imprimitivity theorem remains true in the ergodic nontransitive case in that the irreducible representation of G is now induced by an irreducible representation of a virtual subgroup.

Mackey laid out his theory in two subsequent publications in 1963 and 1966 [Ma14, Ma15]. His initial notion of similarity had to be adjusted a bit in subsequent work to make it function properly. In a real sense, the point of view introduced here by Mackey was the opening shot in the whole program of noncommutative topology and geometry that was to develop. One particularly rich theme has emerged from the special case when the group action is free, in which case the groupoid is simply an equivalence relation, and Mackey defines what one means by a measured ergodic equivalence relation. Isomorphism of measured equivalence relations amounts to orbit equivalence of the group actions, a notion that was foreign in ergodic theory up to that point but which has been of overriding importance in developments since then. In fact [Ma14] adumbrates some of the subsequent developments that have sprung from his work.

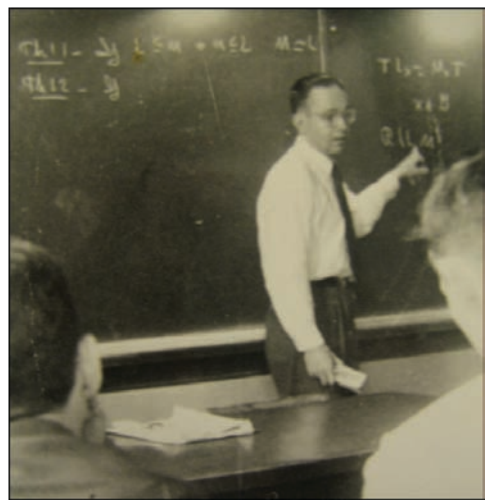

Photo courtesy of Ann Mackey.

Mackey at the University of Chicago, 1955.

As has already been suggested, Mackey maintained a lively and inquiring lifelong interest in mathematical physics and especially the foundations of quantum theory, quantum field theory, and statistical mechanics. In [Ma10] Mackey explored the abstract relationship between quantum states and quantum observables and raised the question of whether some very general axioms about that relationship necessarily led to the classical von Neumann formulation. This exposition inspired Andrew Gleason to prove a strengthened version of Mackey's results, which then enabled him to formulate a general result that showed that the von Neumann formulation followed from a much weaker set of axioms. Also Mackey's work on the unicity of the Heisenberg commutation relations gave an indication why, when the number of $p(i)$'s and $q(i)$'s is infinite (quantum field theory), the uniqueness breaks down.

In his later years Mackey wrote a number of fascinating historical and integrative papers and books about group representations, and harmonic analysis and its applications and significance for other areas of mathematics and in the mathematical foundations of physics. The theme of Norbert Wiener's definition of chaos, or homogeneous chaos, was a favorite theme in his writings. Also applications of group representations to number theory was another common theme, among many others. Mackey was invited to be the George Eastman Visiting Professor at Oxford University for 1966–67, and he and Alice spent the year in Oxford. He gave a broad-ranging series of lectures during the year, which he subsequently published in 1978 as *Unitary Group Representations in Physics, Probability, and Number Theory* [Ma17]. George Mackey will be remembered and honored for his seminal contributions to group representations and ergodic theory and mathematical physics and for his fascinating expositions on these subjects.

Let me close this summary of Mackey's research contributions with some personal thoughts. I first met George Mackey in 1956 when I was entering my junior year at Harvard. He became my advisor and mentor both as an undergraduate and when I continued as a graduate student at Harvard. I learned how to be a mathematician from him, and I valued his friendship, guidance, and encouragement for over fifty years. Many of my own accomplishments can be traced back to ideas and inspirations coming from him. George was a uniquely gifted and inspiring individual, and we miss him very much.

Photo courtesy of Arthur Jaffe.

George and Alice Mackey, May 1984, Berkeley Faculty Club, at a conference in honor of Mackey.

George visited Berkeley on several occasions, and two incidents stick in my mind that are in some ways characteristic of him. One time, probably in the 1960s or 1970s when George was visiting Berkeley, a group of us were walking to lunch and talking mathematics. In this discussion I described a certain theorem that was relevant for the discussion (unfortunately I cannot recall what the theorem was), and George remarked to the effect, "Well, that's a very nice result. Who proved it?" My response was "You proved it." Well, from one perspective it is nice to have proved so many good theorems that you can forget a few.

The other incident or series of incidents occurred in 1983–84, when I had arranged for George to visit at the Mathematical Sciences Research Institute (MSRI) for a year. The housing officer at MSRI found him and Alice a beautiful rental house for the year that belonged to Geoff Chew, a faculty member in physics who was on sabbatical for the year. The only problem was that the house came with some animals, cats and dogs, that the tenants would have to take care of. But the Mackeys said that would be no problem. George arrived a month or two before Alice could come so that the house would not be vacant with no one to take care of the animals. George truly had his hands full with the animals, and even after Alice arrived to take over housekeeping, they had many amusing incidents. When they returned to Cambridge after their year in the "Wild West", Alice wrote a fascinating and hilarious article for the Wellesley alumni magazine about George's and her travails with the Chew menagerie.

References

[Gl] James Glimm, Type *I* C^*-Algebras, *Ann. Math.* **73** (1961), 572–612.

[Ma1] George W. Mackey, On convex topological linear spaces, *Proc. Nat. Acad. Sci. U.S.A.* **29** (1943), 315–319, and Erratum 30, 24.

[Ma2] ———, On convex topological linear spaces, *Trans. Amer. Math. Soc.* **60** (1946), 519–537.

[Ma3] ———, A theorem of Stone and von Neumann, *Duke Math. J.* **16** (1949), 313–326.

[Ma4] ———, Imprimitivity for representations of locally compact groups, I. *Proc. Nat. Acad. Sci. U.S.A.* **35** (1949), 537–545.

[Ma5] ———, On induced representations of groups, *Amer. J. Math.* **73** (1951), 576–592.

[Ma6] ———, Induced representations of locally compact groups, I, *Ann. of Math.* (2) **55** (1952), 101–139.

[Ma7] ———, Induced representations of locally compact groups, II, The Frobenius reciprocity theorem, *Ann. of Math.* (2) **58** (1953), 193–221.

[Ma8] ———, Les ensembles boréliens et les extensions des groupes, *J. Math. Pures Appl.* (9) **36** (1957), 171–178.

[Ma9] ———, Borel structure in groups and their duals, *Trans. Amer. Math. Soc.* **85** (1957), 134–165.

[Ma10] ———, Quantum mechanics and Hilbert space, *Amer. Math. Monthly* **64** (1957), no. 8, part II, 45–57.

[Ma11] ———, Unitary representations of group extensions, I, *Acta Math.* **99** (1958), 265–311.

[Ma12] ———, Point realizations of transformation groups, *Illinois J. Math.* **6** (1962), 327–335.

[Ma13] ———, Infinite-dimensional group representations, *Bull. Amer. Math. Soc.* **69** (1963), 628–686.

[Ma14] ———, Ergodic theory, group theory, and differential geometry, *Proc. Nat. Acad. Sci. U.S.A.* **50** (1963), 1184–1191.

[Ma15] ——— , Ergodic theory and virtual groups, *Math. Ann.* **166** (1966), 187–207.

[Ma16] ———, *The Theory of Unitary Group Representations*, based on notes by James M. G. Fell and David B. Lowdenslager of lectures given at the University of Chicago, Chicago, IL, 1955, Chicago

Lectures in Mathematics, University of Chicago Press, Chicago, IL/London, 1976, x+372 pp.

[Ma17] ———, *Unitary Group Representations in Physics, Probability, and Number Theory*, Mathematics Lecture Note Series, 55, Benjamin/Cummings Publishing Co., Inc., Reading, MA, 1978, xiv+402 pp.

[vN] John von Neumann, Die eindeutigkeit der Schrödingerschen operatoren, *Math. Ann.* **104** (1931), 570–578.

[S] M. H. Stone, Linear transformations in Hilbert space III, operational methods and group theory, *Proc. Nat. Acad. Sci. U.S.A.* **16** (1930), 172–175.

[W] Eugene Wigner, Unitary representations of the inhomogeneous Lorentz group, *Ann. Math.* **40** (1939), 149–204.

J. M. G. (Michael) Fell

Recollections of George Mackey

If there was one individual who influenced the course of my mathematical life more than any other, it was George Mackey. I first met George (about four years after receiving my Ph.D.) at the 1955 Summer Conference on Functional Analysis and Group Representations, held at the University of Chicago. George's lectures there on group representations were an inspiration to me. His contagious enthusiasm spurred me to join with David Lowenslager in writing up the notes of his lectures and aroused in me an enthusiasm for his approach to the subject which has lasted all through my mathematical life.

I would like to make a few remarks about why I found George's approach to group representations so appealing (though I must apologize here for the fact that these remarks say more about me than about George!). His approach fascinated me because it seemed to have a beauty and universality that were almost Pythagorean in scope. We live in a universe whose laws are invariant under a certain symmetry group (for example, the Lorentz group in the case of the universe of special relativity). It seems plausible that the kinds of "irreducible" particles that can exist in a quantum-mechanical universe should be correlated with the possible irreducible representations of its underlying symmetry group. If this is so, then it should be a physically meaningful project to classify all the possible irreducible representations of that group. And now here in George's lectures was a three-phase program laid out as a first step toward just that purpose—indeed, for classifying the irreducible representations of all possible symmetry groups! To my mind this was an extremely exciting and emotionally satisfying idea, though, in hindsight, I think I conceived of it in a naive and narrow manner. But it appealed to me because of the beauty of the concept of symmetry as a fundamental fact of nature. Never mind whether it was in conformity with recent discoveries in physics!!

But George's mind was much broader than this. He seemed willing to embrace scientific reality wherever he found it. One of the aspects of his mathematical creativity that especially struck me was his interest—and success—in finding applications of the theory of group representations to quite different fields of mathematics, including mathematical physics! It seems that his mind was impelling him toward the ideal of a universal mathematician, so hard to attain these days of ever-increasing specialization.

Moreover, he had an idealistic, one might say "aristocratic", view of what it takes to be a true mathematician. I remember him saying once that there are two kinds of mathematicians (and I suppose that he would have made the same distinction for any field of intellectual endeavor): there are those who are "inner-directed" who work because they are impelled by something within them, and there are those who are "outer-directed" who are content to have their work directed for them by the force of outward circumstances. There is no doubt which of these two classes George Mackey was able to belong to.

Unfortunately I had little or no personal contact with George after about 1970. But in the years when we knew him personally, my wife and I always found George—and his good wife, Alice, also—to be very friendly and approachable. Along with his absorption in mathematics, he had an interest in other people. He also had a dry though ready sense of humor. I remember hearing of one episode in the Harvard mathematics department when a young secretary was telling this distinguished professor what he ought to do in a rather bossy manner. George's reply to her was simply "You're talking to me just as if you were my mother."

Now that George is no longer with us, I am one of those who regret not taking the opportunity to meet him and his family during his later years. And my wife and I want to join with all his other friends in condolences to his family over the departure of a great man from our midst.

J. M. G. Fell is emeritus professor of mathematics at the University of Pennsylvania, Philadelphia.

Roger Howe

Recollections of George Mackey

George Mackey was my mathematical grandfather. That is, he was the advisor of my advisor, Calvin Moore of the University of California at Berkeley. According to the Mathematics Genealogy Project, this is a mathematical lineage that goes back to Euler and Leibniz through Marshall Stone, G. D. Birkhoff, E. H. Moore, and Simeon Poisson. The Genealogy Project lists nearly 40,000 mathematical descendants for Euler, of which about 300 come from Mackey. The genealogy lists are updated regularly, so these numbers will increase in the future. Also, the Genealogy Project now lists me as a mathematical grandfather, so George died at least a mathematical great great-grandfather.

Although George was mathematically speaking my grandfather, his influence on me was direct, not just through Calvin Moore. In fact, I met George before I met Cal. This is something you can't do in ordinary genealogy.

I took a course from George in my senior year at Harvard. I had gotten interested in harmonic analysis and representation theory, and everyone spoke of Professor Mackey (as we called him) as the local guru on these subjects. In my last semester, he was giving a course on representation theory. I very much wanted to take it, but it would not fit in my schedule. When I spoke to him about the problem, he volunteered that he was planning to produce detailed written notes and I could take it as a reading course, so that's what I did.

Photo courtesy of Ann Mackey.

George Mackey (left) with his Ph.D. advisor Marshall Stone, 1984.

The last semester of senior year is a time when one's attention is often not on studies. I turned in a five-page paper for an art history course a month late. Also, I had to write my senior thesis. That was my first encounter with mathematics research, and it was very unsettling—somewhat like being possessed. It cost me a lot of sleep. Anyway, it was late in the semester before I even looked at the course notes.

Roger Howe is professor of mathematics at Yale University. His email address is howe@math.yale.edu.

They were not at all what I expected. I had been attracted to representation theory by neat formulas: Schur orthogonality relations, Bessel functions, convolution products. But George's notes were full of Borel sets and Polish spaces and projection-valued measures. They were hard to understand, had a lot of unproved assertions, and were all in all quite strange. Also, they were long! I despaired of making any sense of them at all in the short time I had.

I studied what I could, and George did pass me. He was known for his directness, and his remarks at the end of the exam were quite representative. He didn't say I had done well. He said that I had learned "enough" and that I had done "better than he expected".

In retrospect, I think three things saved me:

1) The exam was postponed. This was actually not on my account; George discovered conflicts and moved the date.

2) There were no proofs to learn. George was not particularly interested in the details of proofs, but much more concerned with the overall structure of the subject. It was easier to get the gist of this than to put together the details of an intricate proof.

3) George really was a wonderful expositor. I didn't understand this at the time because of the newness and strangeness of the material, but since then I have read with pleasure and benefit many of his expositions—of quantum mechanics, induced representations, applications of and history of representation theory. He had a marvelous talent for combining simple ideas to construct rich and coherent pictures of broad areas of mathematics. I continue to recommend his works to younger scholars.

I am grateful that that exam was not the last time I saw George. In fact, our paths crossed regularly, and we developed a cordial relationship. The next time I saw him after Harvard was at a conference at the Battelle Institute in Seattle in the summer of 1969. It was my first professional conference and one of the most delightful I have ever attended. It was intended to encourage interaction between mathematicians and physicists around the applications of representation theory to physics, so there were both mathematicians and physicists there and a sense of addressing important issues. Using representation theory in physics was perhaps George's favorite topic of thought and conversation. I have memories of basking in the sun around the Battelle Institute grounds or on various excursions and in the conversations of the senior scholars: George, in his signature seersucker pinstripe jacket; Cal Moore; B. Kostant; V. Bargmann; and others.

Over the years since, I enjoyed many conversations with George, sometimes in groups and occasionally one-on-one. I have always been impressed by the independence of his views—he came to his own conclusions and advanced them with conviction born of long thought—and by his scholarship—he carefully studied relevant papers in mathematics or physics and took them into account, sometimes accepting, sometimes not, according to what seemed right. I particularly remember a short but highly referenced oral dissertation on the Higgs Boson, delivered for my sole benefit.

I forget when George took to referring to me as his grandstudent, but a particularly memorable occasion when he did so was in introducing me to his advisor, Marshall Stone. The Stone-von Neumann Theorem, which originated as a mathematical characterization of the Heisenberg canonical commutation relations, was reinterpreted by Mackey as a classification theorem for the unitary representations of certain nilpotent groups. Both Cal Moore and I have found new interpretations and applications for it, and now my students use it in their work. In fall 2004 I attended a program at the Newton Institute on quantum information theory (QIT). There I learned that QIT had spurred new interest in Hilbert space geometry. One topic that had attracted substantial attention was *mutually unbiased bases.* Two bases $\{u_j\}$ and $\{v_j\}$, $1 \leq j \leq \dim H$, of a finite-dimensional Hilbert space H, are called *mutually unbiased* if the inner product of u_j with v_k has absolute value $\left(\frac{1}{\dim H}\right)^{\frac{1}{2}}$, independent of j and k. A number of constructions of such bases had been given, and some relations to group theory had been found. The topic attracted me, and in thinking about it, I was amazed and delighted to see that George's work on induced representations, systems of imprimitivity, and the Heisenberg group combined to give a natural and highly effective theory and construction of large families of mutually unbiased bases. It seemed quite wonderful that ideas that George had introduced to clarify the foundations of quantum mechanics would have such a satisfying application to this very different aspect of the subject. I presented my preprint on the subject to George, but at that time his health was in decline, and I am afraid he was not able to share my pleasure at this unexpected application.

I hadn't expected the strange-seeming ideas in George's notes for that reading course to impinge on my research. I had quite different, more algebraic and geometric, ideas about how to approach representation theory. But impinge they did. When I was struggling to understand some qualitative properties of unitary representations of classical Lie groups, I found that the ideas from that course were exactly what I needed. And I am extremely happy not only to have used them (and to have had them to use!) but also to have passed them on: my latest student, Hadi Salmasian, has used these same ideas to take the line of work further and show that what had seemed perhaps ad hoc constructions for classical groups could be seen as a natural part of the representation theory of any semisimple or reductive group. George's body may have given up the ghost, but his spirit and his mathematics will be with us for a long time to come.

Arthur Jaffe

Lunch with George

Background

I was delighted to see that the program of the 2007 New Orleans AMS meeting listed me correctly as a student; in fact I have been a student of George Mackey practically all my mathematical life. George loved interesting and provoking mathematical conversations, and we had many over lunch, explaining my congenial title.

Most of our individual meetings began at the Harvard Faculty Club. George walked there from working at home to meet for our luncheon, and I often watched him pass the reading room windows. Generally our conversations engaged us so we continued afterward in one of our offices, which for years adjoined each other in the mathematics library. Some other occasions also provided opportunity for conversation: thirty years ago the department met over lunch at the Faculty Club. Frequently we also exchanged invitations for dinner at each other's home. Both customs had declined significantly in recent years. Another central fixture revolved about the mathematics colloquium, which for years George organized at Harvard. George and Lars Alfhors invariably attended the dinner, and for many years a party followed in someone's home. George also made sure that each participant paid their exact share of the bill, a role that could not mask the generous side of his character.

Photo courtesy of Ann Mackey.

Mackey in Harvard office.

Arthur Jaffe is the Landon T. Clay Professor of Mathematics and Theoretical Science at Harvard University. His email address is jaffe@math.harvard.edu.

George also enjoyed lunch at the "long table" in the Faculty Club, where a group of regulars gathered weekly. Occasionally I joined him there or more recently at the American Academy of Arts and Sciences, near the Harvard campus. I could count on meeting George at those places without planning in advance. Through these interactions my informal teacher became one of my best Harvard friends. So it was natural that our conversations ultimately led to pleasant evenings at 25 Coolidge Hill Road, where Alice and George were gracious and generous hosts, and on other occasions to 27 Lancaster Street.

While the main topic of our luncheons focused on mathematics, it was usual that the topic of conversation veered to a variety of other subjects, including social questions of the time and even to novels by David Lodge or Anthony Trollope. George seemed to come up with a viewpoint on any topic somewhat orthogonal to mine or to other companions, but one that he defended both with glee as well as success.

George began as a student of physics and found ideas in physics central to his mathematics. Yet George could be called a "quantum field theory skeptic". He never worked directly on this subject, and he remained unsure whether quantum mechanics could be shown to be compatible with special relativity in the framework of the Wightman (or any other) axioms for quantum fields.

When we began to interact, the possibility to give a mathematical foundation to any complete example of a relativistic, nonlinear quantum field appeared far beyond reach. Yet during the first ten years of our acquaintance these mathematical questions underwent a dramatic transition, and the first examples fell into place. George and I discussed this work many times, reviewing how models of quantum field theory in two- and three-dimensional Minkowski space-time could be achieved. While this problem still remains open in four dimensions, our understanding and intuition have advanced to the point that suggests one may find a positive answer for Yang-Mills theory. Yet George remained unsure about whether this culmination of the program is possible, rightfully questioning whether a more sophisticated concept of space-time would revolutionize our view of physics.

Despite this skepticism, George's deep insights, especially those in ergodic theory, connected in uncanny ways to the ongoing progress in quantum field theory throughout his lifetime.

Early Encounters

I first met George face-to-face during a conference organized in September 1965 by Irving Segal and Roe Goodman at Endicott House. Some 41+ years ago, the theme "The Mathematical Theory of Elementary Particles" represented more dream than reality.

I knew George's excellent book on the mathematical foundations of quantum theory, so I looked forward to meeting him and to discussing the laws of particle physics and quantum field theory. George was forty-nine, and I was still a student at Princeton. Perhaps the youngest person at the meeting, I arrived in awe among many experts whose work I had come to admire. George and I enjoyed a number of interesting interactions on that occasion, including our first lunch together.

Our paths crossed again two years later, only weeks before my moving from Stanford to Harvard. That summer we both attended the "Rochester Conference", which brought together particle physicists every couple of years. Returning in 1967 to the University of Rochester where the series began, the organizers made an attempt to involve some mathematicians as well.

The Rochester hosts prepared the proceedings in style. Not only do they include the lectures, but they also include transcripts of the extemporaneous discussions afterward. Today those informal interchanges remain of interest, providing far better insights into the thinking of the time than the prepared lectures that precede them. The discussion following the lecture by Arthur Wightman includes comments by George Mackey, Irving Segal, Klaus Hepp, Rudolf Haag, Stanley Mandelstam, Eugene Wigner, C.-N. Yang, and Richard Feynman. It is hard to imagine that diverse a spectrum of scientists, from mathematicians to physicists, sitting in the same lecture hall—much less discussing a lecture among themselves!

Reading the text with hindsight, I am struck by how the remarks of Mackey and of Feynman hit the bull's-eye. George's comments from the point of view of ergodic theory apply to the physical picture of the vacuum. Feynman's attitude about mathematics has been characterized by "It is a theorem that a mathematician cannot prove a nontrivial theorem, as every proved theorem is trivial," in *Surely You're Joking, Mr. Feynman.* Yet in Rochester, Feynman was intent to know whether quantum electrodynamics could be (or had been) put on a solid mathematical footing. Today we think it unlikely, unlike the situation for Yang-Mills theory.

Harvard

George chaired the mathematics department when I arrived at Harvard in 1967, and from that time we saw each other frequently. We had our private meetings, and we each represented our departments on the Committee for Applied

Mathematics, yet another opportunity to lunch together.

During 1968, Jim Glimm and I gave the first mathematical proof of the existence of the unitary group generated by a Hamiltonian for a nonlinear quantum field in two dimensions. This was a problem with a long history. George's old and dear friend Irving Segal had studied this question for years, and he became upset when he learned of its solution.

At a lunch during April 1969 George asked me my opinion about "the letter", to which I responded, "What letter?" George was referring to an eight-page letter from Segal addressed to Jim and me but which neither of us had received at the time. The letter claimed to point out, among other things, potential gaps in the logic of our published self-adjointness proof. On finally receiving a copy of the letter from the author, I realized immediately that his points did not represent gaps in logic, but they would require a time-consuming response. I spent considerable effort over the next two weeks to prepare a careful and detailed answer.

This put George in a difficult position, but his reaction was typical: George decided to get to the bottom of the mess. This attitude not only reflected George's extreme curiosity but also his tendency to help a friend in need. It meant too that George had to invest considerable time and energy to understand the details of a subtle proof somewhat outside his main area of expertise. And for that effort I am extraordinarily grateful.

It took George weeks to wade through the published paper and the correspondence. Although he did ask a few technical questions along the way, George loved to work things out himself at his own pace. Ultimately George announced (over lunch) the result of his efforts: he had told his old friend Segal that in his opinion the published proof of his younger colleagues was correct. This settled the matter in George's mind once and for all.

We returned to this theme in the summer of 1970 when George, Alice, and their daughter, Ann, spent two long but wonderful months at a marathon summer school in Les Houches, overlooking the French Alps. George (as well as R. Bott and A. Andreotti) were observers for the Battelle Institute, who sponsored the school. During two weeks I gave fifteen hours of lectures on the original work and on later developments—perhaps the most taxing course I ever gave. That summer I got to know the Mackeys well, as the participants dined together almost every day over those eight weeks.

Gradually my research and publications became more and more centered in mathematics than physics, and in 1973 the mathematics department at Harvard invited me to become a full voting member while still retaining my original affiliation with physics. At that point I began to interact with George even more. Following George's retirement in 1985 as the first occupant of his named mathematics professorship, I was humbled to be appointed as the successor to George's chair. I knew that these were huge shoes to fill.

Government

George often gave advice. While this advice might appear at first to be off-the-mark, George could defend its veracity with eloquence. And only after time did the truth of his predictions emerge. One topic dominated all others about science policy: George distrusted the role of government funding.

George often expressed interest in the fact that I had a government research grant. I did this in order to be able to assist students and to hire extraordinary persons interested in collaboration. George often explained why he believed scientists should avoid taking government research money. His theory was simple: the funder over time will ultimately direct the worker and perhaps play a role out of proportion.

When the government funding of research evolved in the 1950s, it seemed at first to work reasonably well. It certainly fueled the expansion of university science in this country during the 1960s and the early 1970s. At that time I believed that the government agencies did a reasonable job in shepherding and nurturing science. The scheme attempted to identify talented and productive researchers and to assist those persons in whatever directions their research drew them. This support represented a subsidy for the universities.

But over time one saw an evolution in the 1970s, much in the way that George had warned. Today the universities have became completely dependent on government support. On the other hand, the government agencies take the initiative to direct and to micro-manage the direction of science, funneling money to programs that appear fashionable or "in the national interest". George warned that such an evolution could undermine the academic independence of the universities, as well as their academic excellence and intellectual standards. It could have a devastating effect on American science as a whole. While we have moved far in the direction of emphasizing programs over discovering and empowering talent, one wonders whether one can alter the apparent asymptotic state.

Personal Matters

George spoke often about the need to use valuable time as well as possible. And the most important point was to conserve productive time for work. Like me, George had his best ideas early in the morning. I was unmarried when our discussions began, and George emphasized to me the need to have a very clear understanding with a partner about keeping working time sacrosanct.

Photo courtesy of Arthur Jaffe.

Ushers at the wedding of Arthur Jaffe, September 1992, (left to right): Raoul Bott, Bernard Saint Donat, George Mackey, Arthur Jaffe, James Glimm, Konrad Osterwalder.

George also described at length how he enjoyed his close relationship with Alice and how they enjoyed many joint private activities, including reading novels to each other, entertaining friends and relatives, and traveling. He also described how he even limited time with daughter Ann. But when he was with Ann, he devoted his total attention to her to the exclusion of all else.

George floated multiple warnings about marriage that I undoubtedly should have taken more seriously. But years later when I remarried, George served as an usher on that occasion; he even ended up driving the minister to the wedding in the countryside. Afterwards George shared a surprising thought: my wedding was the first wedding that he thoroughly enjoyed! In honor of that convivial bond, I wore the necktie chosen for me and the ushers at my wedding at my presentation in the Special Session for George in New Orleans.

Shortly after George retired, I served as department chair. At the beginning of my term I made a strong case that the department needed more office space, as several members had no regular office. Within a year we were able to construct seventeen new offices in contiguous space that had been used for storage and equipment. But before that happened, I had to ask George if he would move from his large office of many years to a smaller one next door. As usual, George understood and graciously obliged.

George's straightforward analysis of the world left one completely disarmed. Memories of this special person abound throughout mathematics. But they also can be heard over lunch at the long table in the Faculty Club and at the weekly luncheons at the American Academy. I am not alone. Everyone misses our fascinating luncheon companion and friend.

David Mumford

To George, My Friend and Teacher*

As a mathematician who worked first in algebraic geometry and later on mathematical models of perception, my research did not overlap very much with George's. But he was, nonetheless, one of the biggest influences on my mathematical career and a very close friend. I met George in the fall of 1954—fifty-three years ago. I was a sophomore at Harvard and was assigned to Kirkland House, known then as a jock house. In this unlikely place, George was a nonresident tutor, and we began to meet weekly for lunch. My father had died three years earlier, and, my being a confused and precocious kid, George became a second father to me. Not that we talked about life! No, he showed me what a beautiful world mathematics is. We worked through his lecture notes, and I ate them up. He showed me the internal logic and coherence of mathematics. It was his personal version of the Bourbaki vision, one in which groups played the central role. Topological vector spaces, operator theory, Lie groups, and group representations were the core, but it was also the lucid sequence of definitions and theorems that was so enticing—a yellow-brick road to more and more amazing places.

This was my first exposure to what higher mathematics is all about. I had other mentors—Oscar Zariski, who radiated the mystery of mathematics; Grothendieck, who simply flew—but George opened the doors and welcomed me into the fold. In those days he led the life of an English don, living in a small apartment with one armchair and a stereo. Here was another side of the life of the intellectual: total devotion to your field, which was something I had never encountered so intensely in anyone in my family's circle. When I graduated, my mother came to Cambridge and wanted to meet one of my professors. We had lunch with George. After that, she said, "This is what I always thought a Harvard professor would be like, the real thing".

Back in the 1960s, government funding of mathematical research was just starting, so of

David Mumford is University Professor in the Division of Applied Mathematics at Brown University. His email address is david_mumford@brown.edu.

**This note is adapted from David Mumford's address at G. Mackey's memorial.*

course everyone was applying. Not George. He rocked the Boston mathematical community—not for the last time—by saying what no one else dared: the government was wasting its money, because all of us would do math all year without the two-ninths raise they were offering. He would not take it. Besides, on a darker note, he predicted all too accurately that when we were bought, the government would try to influence our research. To varying degrees in different fields, this has come to pass. As an applied mathematician for the last twenty years, it is downright embarrassing to see how much government pressure is being applied to create interdisciplinary collaborations. If they happen, great. But this should be each mathematician's personal choice, governed by his interests.

George's outspokenness and his brutal honesty probably got under everyone's skin at some point. He never adjusted his message to his listener. But he often articulated thoughts that we shied away from. Certainly, his carrying his clipboard and catching an hour to do math alone while his wife and daughter went to a museum is a fantasy many mathematicians harbor. George maintained his intellectual schedule through thick and thin. Perhaps my favorite memory of when he voiced a totally unorthodox point of view is this. I asked him once how he survived his three years of Harvard's relentlessly rotating chairmanship. His reply: he was most proud of the fact that, under his watch, nothing had changed; he left everything just as he had found it. For him, a true conservative, the right values never changed.

As I said, we were close friends for all his life. In fact, we continued to meet for lunch, George's favorite way of keeping in touch, until his deteriorating health overtook him and he was forced to retreat to a nursing home. He would always walk from his house on Coolidge Hill to the Faculty Club—perhaps this was his chief source of physical exercise. Over lunch, we would first go over what kind of math each of us was playing with at that time. But then we also talked philosophy and history, both of which attracted George a great deal. He liked the idea that perhaps, as conscious beings, we might not really be living in this world. He used the metaphor, for instance (like the movie *The Matrix*), that our real body could be elsewhere but wearing a diving suit that reproduced the sensations and transmitted our motions to the object that we usually conceive of as our body. The conventional reality of our lives might be a pure illusion.

George was not religious in the conventional sense. He would certainly reject the following if he were alive today, but I think it is fair to say that math was his church. Taking this further, I think his proof of the existence of God was the intertwining of all branches of mathematics and physics. He devoted many years to making manifest the links between mathematics and physics. Many mathematicians have been frustrated by the seeming intractability of the problem of reducing quantum field theory to precise mathematics. But here George was the perennial optimist: for the whole of life he remained sure that the ultimate synthesis was around the corner, and he never dropped this quest. As I learned many years ago from reading about them in his notes, intertwining operators were one of George's favorite mathematical constructs. But I think they are a metaphor for much more in George's life. His wife and daughter were his wonderful support system, and they intertwined George with the real world. There were many wonderful gatherings at their house. George and Alice maintained the long tradition of proper and gracious dinner parties for the mathematical community in Cambridge, long after it had gone out of fashion for the younger generations. His family was truly his lifeline, the hose bringing air to that diving suit.

Judith A. Packer

George Mackey: A Personal Remembrance

George, or Professor Mackey, as I addressed him during my graduate school years, made an indelible impression on me and changed my life for the better, several times: firstly as a brilliant mathematician and my thesis advisor, secondly as a mentor, and finally, as a very kind and sympathetic friend who seemed always to give good advice.

He first entered my radar screen in the fall of 1978, just after I had entered Harvard University's mathematics department graduate program. My undergraduate advisor, Ethan Coven, had suggested that I talk with Mackey, since I had just finished a senior thesis on symbolic dynamics and I thought I knew some ergodic theory. "You must meet George Mackey; he works in ergodic theory," said Ethan, "but you might not recognize it from what you have been studying in your senior thesis. He has a broader approach." This turned out to be an understatement of some large order of magnitude. At the first tea I attended that fall, I immediately spotted George. He was hard to miss, standing in the center of the tea room in his tweed jacket, holding forth on some topic of import and being gregarious to all who came his way, especially if they wanted to talk to him about mathematics or mathematical personalities. I met him and told him of my interest in analysis in

Judith Packer is professor of mathematics at the University of Colorado at Boulder. Her email address is `packer@euclid.colorado.edu`.

general and ergodic theory in particular; he was enthusiastic, and I first began to get a sense that to him analysis was a powerful influence on all areas of mathematics, just as other areas influenced the development of analysis. He immediately gave me the galley proofs of his book *Unitary Group Representations in Physics, Probability, and Number Theory* that had just been published [4] and directed me to the chapter in question. I was stunned, of course: I had thought I knew a lot about an area, and I began to see how I just knew a lot about a teeny speck in a corner of one of the chapters of the book. From a small subset of shift-invariant subspaces, I stared down into the deep ocean of locally compact group actions on standard Borel spaces, neither of which, the groups nor the spaces, I knew much about. Also, Mackey's use of ergodic theory in the *Unitary Group Representations* book was aimed towards an understanding of certain quasi-orbits arising from the theory of induced representations. Going back to the drawing board, I told him I needed to know more analysis; I had studied most of first-year analysis, but knew nothing about the spectral theory of normal operators. Well, he said, why didn't I grade homework for the first-year analysis course he was teaching? It would be a good way to earn a bit of pocket money and learn more analysis at the same time, he opined, and we agreed that when the lectures arrived at spectral theory, I could leave my grading duties aside and take notes. I did as he suggested, feeling a bit guilty on behalf of the students enrolled in the course, but I knew I was fortunate.

The spring semester of my first year in graduate school I took a course on the history of harmonic analysis from Professor Mackey. The point of view in this course, as it was in all of his mathematics, was that harmonic analysis gave a unified way to attack problems in physics, probability, and number theory. His approach to mathematics was much more broad than any I had experienced before. Rather than focusing on any specific problem and performing narrow problem solving (which he must have had an obvious talent for, being one of the first Putnam fellows), he preferred to give a very broad overview so that we could view everything from a high perch looking down. "I want to use a telescope, not a microscope!" he said. He felt deeply that so many aspects of mathematics were "seemingly distinct, yet inseparably intertwined." Every lecture in this course would start out with him writing down the names of five, six, sometimes up to twelve, famous mathematicians who were contemporaries. One would think he would never be able to get through all of the mathematicians in one lecture, but by the end of the lecture he would have connected them all. His enthusiasm was such that he would often lean against the board, and if one's concentration dipped right after lunch, when the class was held, one could entertain oneself by reading the names "Bernoulli", "Euler", "D'Alembert", "Lagrange", "Laplace", "Legendre", etc., outlined in mirror symmetry across his jacket.

Mackey was fascinated by this intertwining of different areas for as long as I knew him, and whenever he gave a lecture, be it public or private, one could see that he was totally immersed in the mathematics that he so loved. His private lectures were just as well prepared and thought-out as his public ones. Whenever he came back from an important conference, he would give me a series of lectures on the main topic of the day. "K-theory for C^*-algebras! You must learn K-theory for C^*-algebras!" he said after returning from Europe one August, and he proceeded to give me a private series of three lectures that he had carefully crafted himself.

All the professors at Harvard were world famous of course, and all were powerful mathematicians, including the Benjamin Peirce Assistant Professors. At the time I was in graduate school, the department was particularly well known in the areas of algebraic geometry and number theory. Because of this and because of Mackey's "telescope rather than microscope" approach, a few "youngsters" did not know of his technical prowess. I remember a colleague was shocked when he was informed that George had been one of the first Putnam fellows. This surprise would have been avoided if he had read some of Mackey's deep papers on Borel spaces, for example "Borel structure in groups and their duals", appearing in 1957 [2], or "Point realizations of transformation groups", appearing in 1962 [4], both showing a total mastery of powerful techniques needed for specific results.

A few graduate students of my era, maybe because they were in a different area and did not know much of his work, wondered why Mackey did not support the department by getting National Science Foundation (NSF) grants. At that time, George's opposition on principle to accepting grant money might not have been widely known among graduate students. This was just five years after Watergate and the end of the Vietnam War, but only one or two years prior to the election of Ronald Reagan. Not many people at the time were suspicious of the NSF's control over research, which George had predicted decades earlier could arise. He told me that he found it impossible to write up a plan saying what he was going to do in the future with any precision and to write out progress reports to explain to what extent he had stuck to his "research plan". He also did not see any reason why an organization funded by the government should need such a plan anyway. "I will go where the mathematics leads me," he said. "Why would I follow any preassigned plan if I found something even better?" He did not seem to get involved in the interdepartmental politics of academe and completely enjoyed his mathematics and his world travels, where he had an amazing amount of recognition at other institutions in the U.S. and overseas.

I want to discuss a little bit of George's mathematics that most influenced me. The paper "Unitary representations of group extensions, I" [3], which appeared in 1958, was a beautiful combination of theory and technical prowess used towards the goal of understanding the theory of unitary representations. In this particular paper a variety of different areas of his expertise were apparent: induced representations, the use of group actions on standard Borel spaces, and projective representations. He formalized a method, the "Mackey machine", which allowed one to deduce all the equivalence classes of unitary representations of a locally compact group G if one knew them a priori for a closed normal subgroup N of G (e.g., if N were abelian) and if G/N acted on $\hat{N}$ = {unitary equivalence classes of unitary representations of N} in a nice enough way.

Indeed, Mackey showed in the above article that if N were type I and regularly embedded in G, then the set-theoretic structure of the dual of G could be described completely in terms of $\hat{N}$, $\widehat{G/N}$, and certain projective dual spaces of subgroups of G/N. In such a case, the Mackey machine can roughly be described as follows:

(1) Fix $\chi \in \hat{N}$.
(2) Let $K = G/N$; then K acts on $\hat{N}$ via $g \cdot \chi(n) = \chi(gng^{-1})$. Let K_χ be the stabilizer subgroup of χ, and let $G_\chi = \pi^{-1}(K_\chi)$. The group K_χ is called, following E. Wigner, the "little group".
(3) Extend χ from a unitary representation of N to a possibly projective unitary representation on L on G_χ with a representation space $\mathcal{H}_\chi$. The representation χ of N uniquely determines the equivalence class of the multiplier σ on G_χ. This is the "Mackey obstruction". One can choose things so that σ is a lift of a multiplier ω on the quotient group K_χ.
(4) If ω is nontrivial, let M be any irreducible $\overline{\omega}$ representation of K_χ and lift it to an irreducible representation of G_χ on the Hilbert space $\mathcal{H}_{\overline{\sigma}}$, also denoted by M. The tensor product representation $L \otimes M$ will then be an irreducible (nonprojective) unitary representation of G_χ.
(5) There may be many such choices, depending on the σ-representation theory of G_χ.
(6) Form the induced representation $\mathrm{Ind}_{G_\chi}^{G}(L \otimes M)$. Again, under appropriate conditions, this representation will be an irreducible representation of G.

This procedure provided up to unitary equivalence all possible irreducible representations of G. One sees here how Mackey used the induction process, the action of the group K on the Borel space $\hat{N}$, and the theory of projective representations—all were unified towards the solution of the problem of describing the structure of $\hat{G}$. With the Mackey machine, new technical difficulties arose in studying the action of K on $\hat{N}$; this was one reason for his initial interest in ergodic theory. Moreover, projective representations arose very naturally in mathematical physics, so it was not at all surprising that he was drawn to projective multipliers on locally compact groups. I think this paper is characteristic of George's method of drawing together tools from seemingly disparate areas towards giving a solution of a particular problem. At the end of this and many other papers, he was often more intrigued by any new questions that had arisen while solving the given problem. His approach led to certain developments in the theory of operator algebras; in particular the study of the structure of certain crossed product C^*-algebras was very much influenced by the "Mackey machine".

Meanwhile, to jump forward in time a bit, I kept attending George's courses in graduate school, even those in topics where my background was less than ideal. He encouraged me to attend his course on quantum mechanics, and when I remarked that I had just had one year of college physics, he said that was plenty for what he would do. I still remember when he told us in an excited manner that the mathematics behind quantum field theory could explain "...why copper sulfate was blue! Boiling points, colors of chemicals, all of these can be worked mathematically!" I remember being particularly struck by the fact that some chemical compound was blue precisely because of the mathematics rather than the physics. This appealed to me greatly, because it gave me hope that I could understand a bit of physics after all if I only could learn the mathematics.

I continued to lurk in George's peripheral vision, and both of us gradually realized that I wanted to be his student. When that propitious moment came, he went to the corner of his office and picked up a huge stack of preprints from the previous five years or so. "I was going to throw these away, because I have the offprints now, but maybe you will find something interesting here," he said. I hauled the stack of thirty or so preprints in various stages of development off to my cubicle, wondering if this would be like looking for a needle in a haystack. But I did look through all of them; it turned out there were many needles and hardly any strands of hay in the pile. I still have most of these preprints in my files, and most were written by a variety of luminaries (E. Effros, C. C. Moore, A. Ramsay, M. Rieffel, M. Takesaki, C. Series, R. Zimmer, to name just a few) whom I later had the good fortune to be able to meet in my postdoc year at UC Berkeley and during trips to UCLA, Penn, and Colorado through the years. As time went on and I became more mathematically mature, I realized that George had given me a gold mine of information. He turned out to be an excellent advisor, one who gave me wide latitude to think about and work on the thesis problem that was most interesting to me, in my case a study of the relationship between the structure of von Neumann algebras and some subalgebras constructed using ergodic actions and quotient actions. He gave me many great leads and mathematical ideas on my thesis, and on other of my papers because of his all-encompassing knowledge of the literature and understanding of the big picture. Whenever I came in with a question about something, even if I was not able to formulate exactly what I needed to know, he seemed to know what I needed. I remember querying him in a confused fashion about masa's (maximal abelian self-adjoint subalgebras) of von Neumann algebras. "Well, do you know about the paper of Singer? You must read the paper of Singer!" and he zipped out of his office on the third floor to the conveniently located math library just outside of it, and found the paper by I. M. Singer [6] in question in a jiffy. As he had indicated, it was extremely insightful and useful, and invaluable in my thesis work.

Since many people have asked me through the years, I feel here I should address George's views on women and their abilities in mathematics. At about the same time that I began working with George, an article by Camilla Benbow and Julian Stanley had just appeared [1], and the topic of whether or not most females aged twelve and above were indisposed by virtue of their sex to do deep mathematics was splashed across the front page of all the newspapers. The interpretation of the contents of this article became a thorn in the side of women mathematicians in general and women mathematics graduate students in particular. Of course this was talked about at the daily teas in the Harvard math department, and, as Mackey was a constant presence at teatime, he would discuss these results just as he would any other topic of the day. He would ask the graduate students what they felt and would muse on whether or not this article provided some sort of "proof" of anything and, in fact, could one obtain a "proof" of this sort rather than merely make observations, which he proceeded to make. I was very annoyed of course; it took me some time to realize that he talked about this partially for the enjoyment of provoking discussion and partially for the enjoyment of provoking. Partly for this reason, a few fellow graduate students, not knowing George well and only seeing him at teatime, thought that because of his age (at that time in his mid-sixties) and his outward manner, he must have been "sexist" as an advisor. I told them that nothing was further from the truth, that all he cared about was his students' interest and abilities in mathematics, and what he most enjoyed doing was talking about mathematics to anyone, male or female. Benbow and Stanley and their ilk were transitory, but mathematics was forever, mathematics was pure, mathematics was sacrosanct. I think that one of his students that he mentioned with the most pride while I was a student was Caroline Series (now a professor at Warwick and also contributing to this article), and when I had the opportunity to meet her, she seemed to share some similar sentiments about George. When it came to the fundamentals of advising and mentoring graduate students, by helping and encouraging them, all that mattered to him was the mathematics they were doing; in my opinion, in the treatment of women mathematicians in his own way he was far ahead of many people many years his junior. He was very comfortable with women's abilities, and with anyone's abilities for that matter, whenever they talked to him about mathematics. This may have been the case because his wife, Alice, was so accomplished and because together they had such a brilliant daughter, Ann, whom he introduced with great pride to me when she was a junior in high school at the department winter holiday party.

I now arrive at his personal kindness. Whenever I was discouraged about my mathematical ability, which was often in the early years following my Ph.D., he would always sound a positive note, telling me of some stellar mathematician who had been similarly discouraged and gone on to great achievements. I know of others who tell similar stories of his kindness and deep sense of loyalty to all of his colleagues and students, regardless of their fame or fortune.

It was fun to catch up with George throughout the years, first in 1983, right after my postdoc year in Berkeley, and finally in the summer of 1999, when my husband, my two young boys, and I visited George and Alice in their Coolidge Hill Road house. They both showed us great hospitality, and George told me of his latest enthusiastic mathematical ideas about finite simple groups and also about his great pride in his young grandson.

I believe that George was one of the great mathematicians and great mathematical characters of the past century. I can see him in my mind's eye now, with his blue seersucker jacket, often with chalk dust on the back of it, with a smile on his face as he is describing the latest advance in mathematical research that either he has uncovered or heard about in the latest conference he has attended. It is hard to think of this world without George in it. I will miss him greatly.

References

[1] C. P. Benbow and J. C. Stanley, Sex differences in mathematical ability: Fact or artifact?, *Science* **210** (1980), 1262–1264.

[2] G. W. Mackey, Borel structure in groups and their duals, *Trans. Amer. Math. Soc.* **85** (1957), 134–165.

[3] ______, Unitary representations of group extensions, I, *Acta Math.* **99** (1958), 265–311.

[4] ______, Point realizations of transformation groups, *Illinois J. Math.* **6** (1962), 327–335.

[5] ______, *Unitary Group Representations in Physics, Probability, and Number Theory*, Benjamin/Cummings Publishing Co., Boston, MA, 1978.

[6] I. M. Singer, Automorphisms of finite factors, *Amer. J. Math.* **77** (1955), 117–133.

Richard Palais

George Mackey had a pivotal influence on my life: my contacts with him, early and late, determined who I was, what I would become, and how my life and career would play out. He was in many ways a model for me, and throughout my career as mathematician and teacher I have frequently realized that in some important decision I made or in some way that I behaved I was attempting to emulate him. If I were an isolated case, this would hardly be worth noting here, but there is considerable evidence and testimony that George influenced very many others with whom he had contact in similar ways.

I first got to know George in 1949. I was then in my sophomore year at Harvard and took his famous Math 212 course. It started from the most elementary and fundamental part of mathematics, axiomatic set theory, proceeded through the development of the various number systems, and ended up with some highly advanced and esoteric topics, such as the Peter-Weyl Theorem. It was exciting and even breathtaking, but also very hard work, and I spent many hours following each lecture trying to digest it all. But what made it a truly life-altering experience was that George was also a tutor in my dorm, Kirkland House, and lived there himself. He encouraged me to take several meals with him each week, where we discussed my questions about the course but also about mathematics and life in general. By the end of that year I decided to switch my major from physics to mathematics, and from then on I think I took every course George gave, and our meals together became even more frequent.

Richard Palais is professor emeritus at Brandeis University and adjunct professor of mathematics at the University of California at Irvine. His email address is `palais@uci.edu`.

In my senior year, George said he felt that I should have some diversity in my mathematical educational experience and advised me to go elsewhere for my graduate training. But this was one piece of advice from him that I ignored, and I was very glad for his continued vital help and encouragement as I worked toward my Ph.D. degree at Harvard. While Andy Gleason became my thesis advisor (on Mackey's advice), George was still an important advisor and mentor during my graduate years, and in the Mathematics Genealogy Project I added him as my second advisor. And George also played an important role in getting me an instructorship at the University of Chicago, then the preeminent place for a first academic job. He had many friends there, and he put in a personal good word for me with them. Experiences such as mine were repeated over and over with many other young mathematicians just starting their careers. For while George was totally devoted to his mathematical research, he was never so busy that he could not spare some time to help a student or younger colleague in their studies and their research.

George and I renewed our friendship when I returned to the Boston area in 1960 to take a faculty position at Brandeis, and over the next forty years, unless one of us was away on sabbatical, we made a practice of meeting frequently for lunch in Harvard Square and discussing our respective research. This gave me a good and perhaps unique perspective of George's long-term research program and goals as he himself saw them. So, although I will leave to others in accompanying articles any detailed review or appraisal of his research, I feel it may be of some interest if I comment on his research from this unusual perspective.

If you asked Mackey what were the most important ideas and concepts that he contributed, I think he would have cited first the circle of ideas around his generalizations to locally compact groups of the imprimitivity theorem, induced representations, and Frobenius reciprocity for finite groups, and secondly what he called Virtual Groups. Indeed, his 1949 article "Imprimitivity for representations of locally compact groups I" in the *Proceedings of the National Academy of Sciences* was a ground-breaking and very highly cited paper that gave rise to a whole industry of generalizations and applications, both in pure mathematics and in physics. George himself used it as a tool to analyze the unitary representations of semidirect products, and it was an essential part of the techniques that, in the hands of Armand Borel and Harish-Chandra, led to the beautiful structure theory for the unitary representations of semisimple Lie groups. But Virtual Groups, introduced in his paper "Ergodic theory and virtual groups" (*Math. Ann.* **186**, 1966, 187–207) were another story. Even the name did not catch on, and they are now usually called groupoids, although that name is used for a great many other closely related concepts as well. Mackey always felt that people never fully understood or appreciated his Virtual Group concept and believed that eventually they would be found to be an important unifying principle. In the later years of his life he tried to work out his ideas in this direction further.

Another aspect of his research about which Mackey was justifiably proud was his contributions to developing rigorous mathematical foundations for quantum mechanics. His 1963 book *Mathematical Foundations of Quantum Mechanics* was highly influential and is still often quoted. (The title is a direct translation of von Neumann's famous and ground-breaking *Mathematische Grundlagen der Quantenmechanik*, which George told me was intentional.) His work in this area stops with the quantum theory of particle mechanics, and several times I suggested that he go further and develop a rigorous mathematical foundation for quantum field theory. But he seemed rather dismissive of quantum field theory and, as far as I could tell, was dubious that it was a valid physical theory.

George was a great believer in what Wigner called "the unreasonable effectiveness of Mathematics in the natural sciences." I want to close with a quote from a lecture that Mackey gave that I feel epitomizes his feeling of wonder at the beauty and coherence of mathematics, a feeling that motivated his whole approach to his research.

> While it is natural to suppose that one cannot do anything very useful in tool making and tool improvement, without keeping a close eye on what the tool is to be used for, this supposition turns out to be largely wrong. Mathematics has sort of inevitable structure which unfolds as one studies it perceptively. It is as though it were already there and one had only to uncover it. Pure mathematicians are people who have a sensitivity to this structure and such a love for the beauties it presents that they will devote themselves voluntarily and with enthusiasm to uncovering more and more of it, whenever the opportunity presents itself.

Arlan Ramsay

Remembering George Mackey

Like so many others, I have benefited often from the clarity of understanding and of exposition of George W. Mackey. If this had been only via his publications, it would have been quite stimulating and valuable, but it has been a truly wonderful opportunity to learn from him in courses and series of lectures and even on a one-to-one basis.

Moreover, after a time, our relationship became one of comfortable friends. My wife and I have both enjoyed knowing George and his delightful wife, Alice. The pleasures have been substantial and greatly enjoyed.

As I revise this, I am visiting at the Institut Henri Poincaré, where there have been numerous talks about uses of groupoids in physics and geometry, particularly noncommutative geometry. If George were here, he would make wonderful reports on the activities. Even with his example to learn from, I can achieve only a poor imitation. Still, I am happy to have the example as a standard.

My first encounter with George was in the undergraduate course he taught in 1958–59 on projective geometry. Already I found his attitude and style appealing.

Then I was in the class in the spring of 1960 for which he wrote the notes that became the book *Mathematical Foundations of Quantum Mechanics* [M1963]. The book by John von Neumann of the same title had aroused my curiosity about the subject, so this course was clearly a golden opportunity, but the reality far exceeded my expectations.

With his customary thoroughness, Mackey began the course with a discussion of classical mechanics in order to be able to explain the quantizing of classical systems and the need for quantum mechanics. He explained about the problem of blackbody radiation and Planck's idea for solving the problem, along with other precursors of quantum mechanics. We heard about the indistinguishability of electrons and the idea that electrons are all associated with a greater totality, like waves in a string are configurations of the string. There were many such insights to go along with the discussion of axioms for systems of states and observables that exhibit appropriate behavior to be models for quantum mechanics. This course answered many questions, and then the answers raised further questions. It was just what was needed and gave me a start on a long-term interest in quantum physics.

George had invested a great deal in support of his aim to understand quantum mechanics. He spoke of reading parts of the book by Hermann

Weyl, *The Theory of Groups and Quantum Mechanics*, and then working at length to explain the material in his own framework. Having done that, he was happy to pass along his understanding in the course and the notes. He was an example of the best practices in communicating mathematics, always ready to be a student or a teacher, as appropriate.

The course George taught in the academic year 1960–1961 was on unitary representations of locally compact groups. He also gave an informal seminar on the historical roots of harmonic analysis. His constant interest in historical relationships was well exhibited in the course and those lectures.

He connected representation theory to quantum mechanics by way of the symmetry one might expect for a single particle in Euclidean 3-space. The "total position observable" of a particle in $\mathbb{R}^3$ should be a system of imprimitivity for a representation of the Euclidean group $\mathbb{E}_3$ as explained in [M1968] and in Chapter 18 of [M1978]. The basic reason is that a Euclidean motion can be regarded as a change of coordinates, and measurements in one system should correspond to measurements in another in a systematic way. If Q_E is the question whether the particle is in a Borel set E and $g \in \mathbb{E}_3$, the relationship desired is that

$$Q_{Eg} = U(g)^{-1} Q_E U(g).$$

Moreover, if the particle is to be treated in the framework of special relativity, then U must extend to the appropriate group of space-time symmetries, the Poincaré group. By using Mackey's analysis of representations of group extensions [M1958], such representations of the Poincaré group can be classified, and the mass and spin of the particle appear from the analysis. This approach to particles as objects that can be localized was also used by A. S. Wightman in [W1962].

The connection to physics was only one of a number of reasons that Mackey was so interested in understanding and exploiting symmetry. Number theory, probability, and ergodic theory also came under that umbrella [M1978].

Over the years I had several other opportunities to learn from George's unique perspective in person. The first instance was a visit to Cambridge in the mid 1960s, and the next was at a conference in 1972 at TCU, organized by Robert Doran. There was a great deal of excitement about the relationship between virtual groups and orbit equivalence in ergodic theory. George was delighted to learn of earlier results of Henry Dye about orbit equivalence for countable abelian groups.

During the following academic year, George gave the De Long Lectures at the University of Colorado in Boulder, adding his own distinction to the list of distinguished speakers. He gave outstanding lectures on the uses of symmetry, particularly on the application of finite groups to the spectra of atoms.

Then at MSRI in 1983–1984, he gave a long series of lectures about number theory. As was his habit, the historical background was an important feature, and symmetry was the star of the show. A dinner at the Mackeys' was one of the memorable social events for us.

Almost twenty years later, in November of 2002, George had an interest in symmetry that was as intense as ever. He was also interested in discussing a variety of other topics: human nature, recently published books, etc. That was our last conversation, and I wish there could be many more. Regarding mathematics and physics, what interested him most at that time was the potential uses of symmetry.

Regarding George's papers, of special interest to me was [M1958]. In it George investigated the unitary representations of G, where G is a (second countable and) locally compact group and N is a type I normal subgroup. The idea is to gain some information from the presence of N and the way G acts on N and hence the unitary dual of N, $\hat{N}$ (unitary equivalence classes of irreducible representations of N). Composing with inner automorphisms of G restricted to N gives a natural action of G on $\hat{N}$. He proved that if L is a factor representation of G, then the canonical decomposition of $L|N$ over $\hat{N}$ uses a measure class $[\mu]$ that is quasiinvariant and ergodic for that natural action. Call $[\mu]$ the measure class *associated with* L. In many cases, N is what he called *regularly imbedded*; i.e., every measure class on $\hat{N}$ that is ergodic and quasiinvariant for the action of G is concentrated on an orbit.

The proofs used in [M1958] end by using transitivity, i.e., by working on a coset space and taking advantage of the existence of a stabilizer that carries the information that is needed. However, his proof of the Imprimitivity Theorem in particular begins in a way that works for general quasiinvariant measures, and his method for getting past the sets of measure 0 can be adapted to the general case.

Suppose that $[\mu]$ is a measure class concentrated on an orbit in $\hat{N}$ and that H is the subgroup of G/N stabilizing a point on that orbit. Then all the representations of G whose associated class is $[\mu]$ can be expressed in terms of the (possibly multiplier) representations of H/N (see the contributions above by C. C. Moore and J. Packer). Moreover, every representation produced by the construction is irreducible. The Imprimitivity Theorem plays a key role in the analysis. If N is not regularly imbedded, the required information is not contained in individual orbits, but more general quasiorbits.

In the more general case, George recognized that the action of G on $\hat{N}$ can be used to give $\hat{N} \times G$ a groupoid structure and that the first step in his proof of the Imprimitivity Theorem in [M1958] constructs a unitary valued function that satisfies the equation defining a groupoid homomorphism almost everywhere on $\hat{N} \times G$. This fact motivated his introduction of ergodic groupoids and their *similarity* equivalence classes, the latter being what he called *virtual groups.* His plan was to use virtual subgroups of G to replace the stabilizers that appear in the regularly imbedded case. In that case $\hat{N} \times G$ is equivalent to the stabilizer of any point in the orbit carrying $[\mu]$, and it follows that the representation behavior for $\hat{N} \times G$ is exactly the same as it is for the stabilizer. Of course, changing the choice of point in the orbit changes the stabilizer to a conjugate subgroup, so the action itself is equivalent to a conjugacy class of subgroups. Since a virtual group is an equivalence class of groupoids, it is essentially inevitable that one tends to work with groupoids themselves. There are also other kinds of equivalence besides similarity, and groupoids persist in all the contexts. The fundamental idea remains the same.

I want to say something about the existence of [R1976]. I inquired about whether George was planning to write the continuation of [M1958], and George generously encouraged me to carry out that project myself.

Those results were combined with other work to show that it is possible to transfer information from one nonregularly imbedded group extension to another in [BMR] and to investigate nonmonomial multiplier representations of abelian groups in [BCMR]. Of course finding all representations for a given virtual group is virtually impossible, but useful information can be obtained. George's notion of virtual group is now thriving in a number of areas, and representation theory remains part of the program.

References

[BMR] L. W. Baggett, W. Mitchell, and A. Ramsay, Representations of the discrete Heisenberg group and cocycles of an irrational rotation, *Michigan Math. J.* **31** (1984), 263–273.

[BCMR] L. W. Baggett, A. L. Carey, W. Moran, and A. Ramsay, Non-monomial multipler representations of Abelian groups, *J. Funct. Anal.* **97** (1991), 361–372.

[M1958] George W. Mackey, Unitary representations of group extensions I, *Acta Mathematica* **99** (1958), 265–311.

[M1963] ——, *The Mathematical Foundations of Quantum Mechanics,* W. A. Benjamin, Inc., New York-Amsterdam, 1963.

[M1968] ——, *Induced Representations of Groups and Quantum Mechanics*, W. A. Benjamin, Inc., New York-Amsterdam; Editore Boringhieri, Turin, 1968, viii+167 pp.

[M1978] ——, *Unitary Group Representations in Physics, Probability, and Number Theory*, Mathematics Lecture Note Series, 55, Benjamin/Cummings Publishing Co., Inc., Reading, MA, 1978, xiv+402 pp.

[R1976] Arlan Ramsay, Nontransitive quasiorbits in Mackey's analysis of group extensions, *Acta Mathematica* **137** (1976), 17–48.

[W1962] A. S. Wightman, On the localizability of quantum mechanical systems, *Rev. Modern Physics* **34** (1962), 845–872.

Caroline Series

George Mackey

In my memory, George Mackey scarcely changed from the time I first approached him as a prospective graduate student in the mid-1970s until the last time I saw him a couple of years before his death. He was a scholar in the truest sense, his entire life dedicated to mathematics. He lived a life of extraordinary self-discipline and regularity, timing his walk to his office like clockwork and managing, how one cannot imagine, to avoid teaching in the mornings, this prime time being devoted to research. I was once given a privileged view of his study on the top floor of their elegant Cambridge house. There, surrounded on all sides by books and journals shelved from floor to ceiling, he had created a private library in which he immersed himself in a quiet haven of mathematical and intellectual scholarship. I was lucky to arrive in Harvard at the time when he was working on what subsequently became his famous Oxford and Chicago lecture notes [7, 8]. These masterly surveys convey the sweep of huge parts of mathematics from group representations up. I do not know any other writer with quite his gift of sifting out the essentials and exposing the bare bones of a subject. There is no doubt that his unique ability to cut through the technicalities and draw diverse strands together into one grand story has been a hugely wide and enduring influence.

Mackey believed strongly in letting students find their own thesis problem. Set loose reading his notes, I reported to him every couple of weeks, and he was always ready to point me down some new yet relevant avenue. He never helped with technical problems, always saying, "Think about it and come back next week if you are still stuck."

Caroline Series is professor of mathematics at the University of Warwick, UK. Her email address is `C.M.Series@warwick.ac.uk`.

Sometimes I envied the other students, whom I, somewhat naïvely, assumed were being told exactly what to do, but in retrospect this was a most valuable training. Stemming from his interest in ergodic theory, I was given a partially guided tour of a wide swathe of dynamical systems.This loose but broad direction stood me in good stead later; I often think of it when under pressure to hand out precisely doable thesis problems when students have barely started their studies.

I finally settled for working on Mackey's wonderful invention, *virtual groups.* The idea, touched on in C. C. Moore's article, is laid out in most detail in Mackey's paper [5]. His explanation to me was characteristically simple. He was interested in group actions on measure spaces, because a measure-preserving action of a group induces a natural unitary representation on the associated L^2 space. As with any class of mathematical objects, he said, if you want to understand group actions, you should split them up into the simplest possible pieces. The simplest kind of group action is a transitive one, that is, one with a single orbit. Being a generalist, Mackey wanted to work in the category of Borel actions, so in particular the groups he cared about always had a standard Borel structure, that is, were either countable or Borel isomorphic to the unit interval. As he pointed out, an action being Borel has unexpectedly strong consequences; in particular the stabilisers of points are closed. A transitive action is clearly determined by the stabiliser of a single point or, more precisely, since the choice of point is arbitrary, by a conjugacy class of such. Thus a transitive action of a Borel group on a standard Borel space is equivalent to the specification of a conjugacy class of closed subgroups.

What is the next simplest type of action? Since we are in a category of measure spaces, an "indecomposable" action means that the underlying space should not split into nontrivial and measurable "subactions". Assuming the group preserves a measure or at least a measure class, this is precisely what is meant by the action being ergodic: there are no "nontrivial" group invariant measurable subsets, where "nontrivial" means neither a null set nor the complement of a null set. Mackey's idea was that, since a transitive action is determined by a closed subgroup, then wouldn't it be nice if an ergodic action were similarly determined by a new kind of "subobject" of the group, which he named in advance a "virtual group". There is a touch of genius in his passage from this apparently simplistic idea to a formal mathematical structure yielding deep insights. The key point is that the sought-after virtual group should be the *groupoid* $S \times G$, in which the base space is the underlying space of the action S and the arrows are pairs (s, g) where (s, g) has initial point s and final point $g \cdot s$ (or rather $s \cdot g$, since Mackey insisted on doing all his group actions from the right). Thus (s, g) could be composed with (s', g') if and only if $s' = s \cdot g$ and then $(s, g) \circ (s', g') = (s, gg')$.

The next step is perhaps the most interesting: a homomorphism from $S \times G$ to a group H should be a cocycle: that is, a map $a : S \times G \to H$ such that $a(s, g)a(sg, g') = a(a, gg')$. This led Mackey to his construction of the "range of the homomorphism". Observe that, in deference to Mackey's order, the direct product $G \times H$ acts on $S \times H$ by $(s, h) \cdot (g, h') = (sg, h'^{-1}ha(s, g))$. The "range" is essentially the action of H on the space of G orbits: if this space is not a standard Borel space, as, for example, if the G action is properly ergodic, one replaces it by the largest standard Borel quotient. Mackey saw this as the generalisation of the dynamical systems construction of a "flow built under a function". (A cocycle for a $\mathbb{Z}$-action can be defined additively given a single function from S to H.) This circle of ideas was seminal for much future work, in particular that of Mackey's former student Robert Zimmer. A special case is the Radon Nikodym derivative of a measure class preserving group action, which can be viewed as a groupoid homomorphism to $\mathbb{R}$, of which more shortly.

A special case arises when all the stabilisers of points are trivial. Mackey's natural relation of similarity between groupoids leads to the classification of free ergodic actions up to "orbit equivalence". Two measure-class-preserving actions of groups G, G' on spaces S, S' are called *orbit equivalent* if there is a Borel measure-class-preserving map $\phi : S \to S'$ with the property that two points in the same G-orbit in S are mapped to points in the same G'-orbit in S'. (This is *much* weaker than the usual notion of conjugacy, in which one insists that $G = G'$ and that $\phi(sg) = \phi(s)g$.) Just how much weaker is expressed in a remarkable theorem discovered by Mackey's student Peter Forrest: *any* two finite measure-preserving actions of $\mathbb{Z}$ are orbit equivalent [3]. Subsequently, Mackey learnt that the theorem had previously been proved by H. Dye [2] and became a great publicist. He took pleasure in telling me it had also been proved by the Russian woman mathematician R. M. Belinskaya [1]. More generally, any equivalence relation orbit equivalent to a $\mathbb{Z}$-action is called *hyperfinite.* Dye's theorem extends to show that actions of a much wider class, including all abelian groups, are hyperfinite, culminating in Zimmer's result [9] that an action is hyperfinite if and only if the equivalence relation is amenable in a suitable sense.

The classification of non-measure-preserving $\mathbb{Z}$-actions up to orbit equivalence is even more remarkable. Regarding the Radon Nikodym derivative as a cocycle to $\mathbb{R}$ as above, Mackey's "range" is known to dynamicists as the Poincaré flow. At the time Mackey did not perhaps appreciate just how far-reaching a construction this was. If the original $\mathbb{Z}$-action is measure-preserving, it is called Type I, II_0, II_∞, depending on whether the range $\mathbb{R}$-action is transitive or preserves a finite or infinite measure respectively. If the original action only preserves a measure class, it is called Type III_1, III_λ, and III_0, depending on whether the range groupoid is $\{\mathrm{id}\}, \lambda\mathbb{Z}$ for some $\lambda \in \mathbb{R}^+$, or properly ergodic. In a beautiful and remarkable piece of mathematics, pushed to its conclusion by W. Krieger [4], it turns out that the range completely classifies the original $\mathbb{Z}$-action up to orbit equivalence. The same construction gives rise to a rich fund of examples of von Neumann algebras, a fact widely exploited by A. Connes. My training under Mackey was an ideal foundation from which to appreciate all this work, which was developing rapidly in the late 1970s just about the time I finished my thesis.

Photo courtesy of Richard Palais.

Andrew Gleason (left) with Mackey at an 80th birthday party for Alice Mackey.

To return to Mackey as a person. Everyone who knew him will remember his uncompromising and sometimes uncomfortably forthright intellectual honesty. He took pleasure in following through a line of thought to its conclusion: political correctness was not for him. He wrote several articles about the invidious effects of federal research funding [6]. He may have lost the battle, but what he said was quite true.

I must say something about the widely held view that Mackey was against women mathematicians. All that I can say is that I never experienced the slightest prejudice from him and am proud to be what he referred to as "his first mathematical daughter." His straightforwardness was perhaps easy to misinterpret. For example, he might say something like "There have been historically

Eulogy by Andy Gleason

It was nearly sixty years ago that I first met George. The circumstances were that I had just arrived here in Cambridge myself, and I started to listen to his course. He was then an assistant professor, and he gave a course on locally compact groups, something he did many, many times thereafter—a course he gave often.

And I went to his course and listened, and pretty soon we started having after-class conversations. And then I began a policy of frequently going around to his rooms and talking mathematics. Just about general parts of mathematics. Not about his course particularly, maybe a little of this, maybe a little of that, but whatever it was.

And I, in any event, learned a great deal from these conversations. And yet, when I look back, I can't really focus on a specific thought that comes out of those conversations. It was a very general thing; it's because when we talked, we didn't talk about mathematics at the level where you see it in publications, with the revolving theme of first definition, then a theorem, then a proof. We didn't do that; we talked in very speculative terms. And I just found that I learned a great deal from this kind of conversation, and I'm really very deeply indebted to George for having told me these things—many, many things.

We went on having those conversations for the next two years, and then he went away to France for a year, and when he came back we started up again. Then I was away for two years, and we had to start again.

Finally, we used to talk after we were married and I and he both lived out toward the west end of Cambridge. We used to walk along Brattle Street, continuing the conversations of then, by this time, twenty years before.

And they were the same kinds of things. They were speculations about the nature of mathematics. They weren't theorems about mathematics; they were just speculations. And some of them worked out ultimately, some of them did not, and that's the way mathematics is. And I very much appreciate the fact that I learned a great deal from George and I went on from there, and eventually was able to become his colleague.

Thank you.

Andrew Gleason

almost no women mathematicians of stature. Therefore, on a statistical basis it is unlikely that a particular woman will be one." Of course such a view ignores all the complicated historical and cultural factors which explain why this might be so; nevertheless, the fact was hard to dispute. But what remains in my memory is that Mackey was always open-minded and unprejudiced, willing to take on-board new insights or experiences and accept new people on their merits, exactly as they came along.

Mackey stood for the highest standards. He lived his life by precise rules, but he enjoyed it to the fullest. Within his mathematics he found a fulfilment to which few can aspire. The spartan simplicity and mild disorder of his office contrasted sharply with the comfortable elegance created by Alice in their Cambridge home. Her wonderful old-world dinner parties were memorable occasions at which it was a privilege to be a guest. Mackey was sometimes disarmingly open about his family life, but through it all shone human warmth and love: their strictly scheduled but vastly important time reading aloud in the evenings, his pride in their daughter, Ann.

Mackey had the habit of writing lengthy letters about his latest discoveries. Long after retirement, indeed right up to a couple of years before his death, he continued working on various projects which between them seemed to involve nothing less than unravelling the entire mathematical history of the twentieth century. Subjects expanded to include statistical mechanics, number theory, complex analysis, probability, and more. He explained that group representations encompassed more or less everything, given that starting from quantum theory one obviously had to include chemistry and thus also biology. One might argue that things are a little more complicated; indeed I am sure with a twinkle in his eye he would agree. What is certain is that his ability to strip things down to their essential mathematical structure put a hugely influential stamp on generations of mathematicians and physicists. He was a man whose unique qualities, insights, and enthusiasms touched us all.

References

[1] R. M. Belinskaya, Partitions of Lebesgue space in trajectories defined by ergodic auto-morphisms, *Funktsional. Analiz i Ego Prilozen* **2**, No. 3 (1968), 4–16 (Russian).

[2] H. A. Dye, On a group of measure-preserving transformations, I, II, *Amer. J. Math.* **81** (1959), 119–159.

[3] P. Forrest, Virtual subgroups of $\mathbb{R}^n$ and $\mathbb{Z}^n$, *Adv. Math.* **3** (1974), 187–207.

[4] W. Kreiger, On ergodic flows and the isomorphism of factors, *Math. Ann.* **223** (1976), 19–70.

[5] G. Mackey, Ergodic theory and virtual groups, *Math. Ann.* **166** (1966), 187–207.

[6] ______, Effort reporting and cost sharing of federal research, *Science* **153** (Sept. 1966), 1057.

[7] ______, *The Theory of Unitary Group Representations*, Univ. of Chicago Press, 1976.

[8] ______, *Unitary Group Representations in Physics, Probability and Number Theory*, Benjamin, 1978.

[9] R. J. Zimmer, Hyperfinite factors and amenable actions, *Invent. Math.* **41** (1977), 23–31.

V. S. Varadarajan

I first met George Mackey in the summer of 1961 when he visited the University of Washington at Seattle, WA, as the Walker-Ames professor to give a few lectures on unitary representations and on quantum mechanics. The lectures were a great revelation to me, as they revealed a great master at work, touching a huge number of themes and emphasizing the conceptual unity of diverse topics. I had the opportunity to meet at leisure with him and talk about things, and his advice was invaluable to me. I was just getting started in representation theory, and his suggestion that I should start trying to understand Harish-Chandra's vast theory (based on a "terrifying technique of Lie algebras" as he put it) was one that gave my research career the direction and boost it needed.

In representation theory Mackey's goal was to erect a theory of unitary representations in the category of *all* second countable locally compact groups. This was decades ahead of his time, considering that all the emphasis on doing representation theory and harmonic analysis on p-adic and adelic groups would be ten to fifteen years in the future. In the aftermath of the fusion of number theory and representation theory, the original goal of Mackey seems to have fallen by the wayside; I feel this is unfortunate and that this is still a fertile program for young people to get into. It may also reveal hidden features of the p-adic theories that have escaped detection because of overspecialization.

I should mention his work on induced representations of finite groups. Even though this is a very classical subject, he brought fresh viewpoints that were extraordinarily stimulating for future research. A basic question in the theory is to compute the dimension of the space of intertwining operators between two induced representations of a finite group G, induction being from two possibly different subgroups $H_i (i = 1, 2)$. Mackey found a formula for this as a sum over the double coset space $H_1\backslash G/H_2$

V. S. Varadarajan is professor of mathematics at the University of California, Los Angeles. His email address is `vsv@math.ucla.edu`.

of local (so to speak) intertwining numbers. The method on the surface does not seem to apply when G is infinite, for instance, when G is a Lie group. However, François Bruhat, then a student at Paris and interacting with Henri Cartan and Laurent Schwartz, realized that in order to make Mackey's method work one has to represent (following a famous theorem of Laurent Schwartz) the intertwining operators by suitable *distributions* on G that have a given behavior under the action of $H_1 \times H_2$ (from the left and right) and obtained a formula remarkably similar to Mackey's, at least when the double coset space was finite (as it is when G is semisimple and $H_1 = H_2$ is minimal parabolic). The Mackey-Bruhat theory has had a profound influence on the subject, as can be seen from Bruhat's own work on induced representations of p-adic groups and the later work of Harish-Chandra on the representations induced from an arbitrary parabolic and, later still, his work on the Whittaker representations of a semisimple Lie group.

The lectures he gave on quantum mechanics at Seattle were an eye-opener for me. He was the first person who fully understood the points of view of von Neumann and Hermann Weyl, made them his own, and then took them to even greater heights. His group-theoretic analysis of the fundamental aspects of quantum kinematics and dynamics was very beautiful, and he adumbrated this in a number of expositions. But it was his analysis of the foundations that was very original. The basic question, one that von Neumann first discussed in his book *Grundlagen der Quantenmechanik*, is the following: Is it possible to derive the statistical results of quantum theory by averaging a more precise theory involving several *hidden variables*? In his analysis von Neumann introduced the model-independent *Expectation functional* E on the space $\mathbb{O}$ of all bounded observables and showed that if $\mathbb{O} = B_{\mathbb{R}}(\mathcal{H})$, the space of bounded self-adjoint operators on the Hilbert space $\mathcal{H}$ (the model for quantum theory), then E is necessarily of the form $E(A) = \mathrm{Tr}(UA) = \mathrm{Tr}(U^{1/2}AU^{1/2})$ for all $A \in B_{\mathbb{R}}(\mathcal{H})$, for some positive operator U of trace 1. The states, which are identified with the Expectation functionals, form a convex set, and its *extreme points* are, by the above result of von Neumann, the one-dimensional projections $U = P_\phi$, the ϕ being unit vectors in the quantum Hilbert space $\mathcal{H}$. If there were hidden variables, the states defined by the P_ϕ would be convex combinations of the (idealized states) defined by giving specific values to the hidden variables, a contradiction.

The basic assumption that von Neumann made about the functionals E was their *unrestricted additivity*, namely, that $E(A+B) = E(A) + E(B)$ for *any two* $A, B \in B_{\mathbb{R}}(\mathcal{H})$. If A and B commute, they represent simultaneously measurable observables, and so they are random variables on the same probability space, thus presenting no obstacle to the assumption of the additivity of the expectation values. But if A and B *do not commute*, there is no single probability space on which they both can be regarded as random variables, and so in this case the assumption of additivity is less convincing. Mackey saw that to have the most forceful answer to the hidden variables question, the additivity can be assumed only for *commuting* observables. If E is assumed to be additive only for commuting observables, its restriction to the lattice $\mathcal{L}(\mathcal{H})$ of projections in $\mathcal{H}$ would be a *measure*, i.e., additive over orthogonal projections. Conversely, any such measure would define an expectation value that has all the properties that von Neumann demanded, except that additivity will be valid only for commuting observables. Andrew Gleason then showed, after Mackey brought his attention to the question of determining all the measures on the orthocomplemented lattice $\mathcal{L}(\mathcal{H})$, that the only countably additive measures are of the form $P \longmapsto \mathrm{Tr}(UP) = \mathrm{Tr}(U^{1/2}PU^{1/2})$ (when $\dim(\mathcal{H}) \geq 3$); from this point on, the argument is the same as von Neumann's. One can use the Gleason result to show also that there are no two-valued *finitely additive* measures on the lattice $\mathcal{L}(\mathcal{H})$ if $3 \leq \dim(\mathcal{H}) \leq \infty$, thus showing that there are no dispersion-free states. This analysis of hidden variables à la Mackey is a significant sharpening of von Neumann's analysis and reveals the depth of Mackey's understanding of the mathematical and phenomenological issues connected to this question.

In his view of quantum kinematics, Mackey formulated the covariance of a quantum system with a manifold M as its configuration space as the giving of a pair (U, P) where U is a unitary representation of a group G acting on M and P is a projection-valued measure. For any Borel set $E \subset M$, $P(E)$ is the observable that is 1 or 0 according to whether the system falls in E or not, and G is the given symmetry group or at least a central extension of it (the latter is necessary, as the symmetries act as automorphisms of the lattice of projections and so are given by unitary operators defined only up to a phase factor). Covariance is then given by the relations $U(g)P(E)U(g)^{-1} = P(g[E])$. This is exactly what Mackey had called a *system of imprimitivity for* G in his pioneering work on the extension of the theory of Frobenius (of induced representations) to the full category of all separable locally compact groups, and he was thus able to subsume the fundamental aspects of quantum kinematics under his own work. The story, as told to me by him in Seattle, of how he came upon this formulation of quantum kinematics is quite interesting.

Irving Segal, who had gone to attend a conference, sent him (Mackey) a postcard saying that Wightman had in his lectures at the conference used Mackey's work to discuss kinematic covariance in quantum mechanics, and Mackey then deduced his entire formulation with no further help other than this cryptic postcard! Of course there is more to covariance than a system of imprimitivity—indeed, even in ordinary quantum mechanics, this formulation does not prevent position observables from being defined for the relativistic photon, as Laurent Schwartz discovered, although we know that there is no frame where the photon is at rest, and so position operators cannot be defined for it. Much work thus remains to be done in giving shape to Mackey's dreams of a group-theoretic universe.

In recent years, when attention has been given to quantum systems arising from models of space-time based on non-Archimedean geometry, or super geometry, the Mackey formulation has offered the surest guide to progress. As an example one may mention the classification of superparticles carried out in the 1970s by physicists whose rigorous formulation needs an extension of Mackey's imprimitivity theorem to the context of homogeneous spaces for the super Lie groups, such as the super Poincaré groups.

In all my encounters with him he was always considerate, full of humor, and never condescending. He was aware that his strength was more in ideas than in technique (although his best work reveals technical mastery in functional analysis of a high order) and repeatedly told me that as one gets older, technique will disappear and so one has to pay more attention to ideas. His view of mathematics and its role in the physical world was a mature one, full of understanding and admiration for the physicists' struggles to create a coherent world picture, yet aware of the important but perhaps not decisive role of mathematics in its creation. In his own way he achieved a beautiful synthesis of mathematics and physics that will be the standard for many years to come.

Ann Mackey

Eulogy for My Father, George Mackey*

Before I lose my composure, I want to offer thanks. My mother and I thank my father for everything he was to us, and we thank everyone here today for coming to share in this memorial service, especially those who have spoken and captured my father's essence so beautifully.

My father had a wonderful life and knew it. Having started out feeling like a misfit who couldn't begin to live up to the material and social expectations set for him by his parents, he felt extraordinarily fortunate to have found his way to a world in which he could do what he loved best in such a rarified atmosphere. The world opened up for him, literally and figuratively, when he discovered mathematics, and he never looked back. He devoted all the time he could to his work, spending most of his days in his third-floor study, emerging only for meals. If I came upstairs to look for him, I could count on seeing him slumped in his chair with clipboard in hand, lost in thought and often chewing on one of the buttons on his shirt. That clipboard traveled with him around the world, and my mother and I left him working on countless park benches while we pursued separate adventures.

My father was notoriously eccentric and proud of it. Having found a clothing style that suited him early in life, gold pocket watch included, he resisted straying any further from that than absolutely required. No matter how hot it got, he could rarely be persuaded to remove his jacket and tie, even at the beach (and we have the pictures to prove it here today). I will leave to your imaginations the discussions he had with my mother on the subject of his attire.

Photo courtesy of Ann Mackey.

Mackey with daughter Ann in Zurich, 1971.

He disclaimed any formal interest in people, but his conversations were filled with news of those he had encountered during his day. Although he resisted any event that might interfere with his work time, he actively enjoyed socializing. Bedtime was sacred, though, so he was often the first to leave a party.

The visual arts left him unmoved; instead, he saw great beauty in mathematics. New insights thrilled him, and he would emerge from his study giddy with excitement about a new idea.

He was not musical, but enjoyed classical music in modest doses and had a great fondness for Gilbert and Sullivan tunes. He would often burst into song spontaneously, generally off-key. He taught himself to play the piano by creating his own numeric notational system, with pluses for sharps and minus signs for flats. He called it the "touch typewriter method" and built up quite a repertoire of his favorite tunes. He took a similar approach to foreign language study.

He was more sentimental than he would ever admit, often choking up as he read certain passages aloud.

Remarks at the memorial service, Harvard Chapel, April 29, 2006.

Many people have already touched on my father's honesty and adherence to principle, so I'm going try to avoid redundancy. However, I did want to stress that as much as he enjoyed a good argument and as much as his honesty could sometimes come at the expense of tact, he never wished to be unkind. He often agonized about how to deliver his message without giving serious offense. He could be absolutely infuriating, and there was sometimes an element of mischief and contrariness in his arguments, but his words truly bore no malice.

Photo courtesy of Robert Doran.

Mackey with his famous clipboard, 1988.

He rarely signed on to any idea or behavior before he had thoroughly researched the subject in question and had reached his own conclusions. He was not a quick study, and this could be infuriating when something seemed obvious on its face. We joked that despite his physical caution, his aversion to taking orders might someday cause him to walk straight off a precipice if he wasn't given clear, provable evidence that the precipice existed.

I loved my father dearly. Although he was fond of asserting to anyone who would listen that he'd never wanted a family, it was clear to everyone that once he stumbled into marriage and fatherhood, he relished and cherished it, even as he struggled to adapt to the compromises it asked of him.

His marriage to my mother, his opposite in so many ways, had its share of memorably dramatic moments, but at its core was love and respect. They lived much of their daily lives independently, but were together for most meals, especially their nightly cocktails and candlelit dinners, where they shared news of their days, followed by my father reading aloud to my mother as I drifted off to asleep upstairs. They enriched each other's lives in ways neither expected and particularly enjoyed their travels together and the friends they made around the globe. Despite their different outlooks on many things, each was the other's best and most trusted friend.

As serious as my father was about his work, he was a child at heart. He was a wonderful, playful father to me when I was young, reading to me, drawing me pictures, crawling around on all fours, and patiently playing endless games of Monopoly, so long as we kept the latter to morning play times, lest the competitive stress of the game interfere with his sleep. He was a thoughtful, concerned—albeit somewhat befuddled—advocate for me as I navigated the tricky landscape of adolescence. He was a refuge when I was sad or anxious. He was patient.

Photo courtesy of Ann Mackey.

George Mackey.

And although mystified that anyone, including myself, would not choose a career in the sciences, he believed that one should follow one's heart and always supported the choices I made, regardless of whether he fully understood them.

He was a dedicated letter writer, sending long, affectionate, newsy updates to us when he traveled, and for me, scientific and mathematical expositions, including letters that would end, and I quote, "Q.E.D. Love, Daddy." In his later years he eagerly embraced email as a way to communicate with me. At my request, he sent me a serialized account of his life, filled with all the stories he'd told me of his childhood and continuing partway through graduate school. A year or so ago, although he had lost the ability to put more than a few words on paper, he narrated further accounts aloud for my mother to transcribe. We have all these words to hold on to, and I am deeply grateful for that.

Please forgive me if I take this moment to encourage everyone here to make the time to write to those they love.

My father was a realist about death. Conscious of being an older parent, he had, to some extent, been preparing me for his eventual demise virtually since the day I was born. But it was very hard to watch the man we knew and loved slip away from us over these past few years. We are fortunate that in many fundamental ways he did retain much of his old self, but it was a sad journey.

Certainly we appreciate that death is part of the natural cycle of life and that we should rejoice that he lived so fully for so long. Those thoughts, and the memories he left, and those shared by so many people in their letters and phone calls do help immensely. I am glad that his grandchildren were at least able to spend time with him, if not know him as he once was. But standing right here, I am conscious mostly of what we have lost. My mother and I miss him terribly, and it is very hard to say goodbye.