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 $\{v_j\}$, $1 \le j \le \dim H$, 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.

Photo courtesy of Ann Mackey.

Mackey in Harvard office.

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.

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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 im-

portant 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.

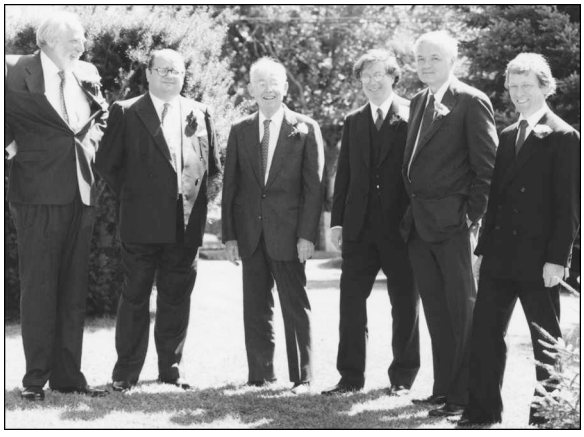

Photo courtesy of Arthur Jaffe.

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

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