Irving Ezra Segal
(1918–1998)

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Irving Segal died suddenly on August 30, 1998, while taking an evening walk. He was seventy-nine and was vigorously engaged in research.

Born on September 13, 1918, in the Bronx, he grew up in Trenton and received his A.B. from Princeton in 1937. What must it have been like to be a member of the Jewish quota at Princeton in the 1930s? He told me once that a fellow undergraduate offered him money to take an exam in his stead and was surprised when Irving turned him down.

He received his Ph.D. from Yale in 1940. His thesis was written under the nominal direction of Einar Hille, who suggested that Segal continue his and Tamarkin's investigation of the ideal theory of the algebra of Laplace-Stieltjes transforms absolutely convergent in a fixed half-plane. But, Segal wrote, "For conceptual clarification and for other reasons, an investigation of the group algebra of a general [locally compact] abelian group was of interest." And the thesis was not restricted to abelian groups.

Segal was an instructor at Harvard in 1941, and then war work—first at Princeton and later in the army at the Aberdeen Proving Ground—prevented a full publication of the thesis until 1947.

Looking edgewise at a bound journal volume, one perceives a band spectrum for the articles—the darker the band, the more intensely has the article been studied. Segal's thesis acquired a dark band indeed. Together with M. H. Stone and I. M. Gelfand, he was one of the principal architects of the application of algebraic methods to analysis, vastly simplifying and extending classical results of harmonic analysis.

After the war Segal spent two years at the Institute for Advanced Study, where he held the first of the three Guggenheim Fellowships that he was to win. Other honors included election to the National Academy of Sciences in 1973 and the Humboldt Award in 1981. At the University of Chicago from 1948 to 1960, he had fifteen doctoral students, and at MIT, where he was professor from 1960 on, formally retiring in 1989, he had twenty-five. Segal's mathematical ancestry runs from Hille and Marcel Riesz through Fejér and Schwarz to Weierstrass.

I had the great fortune to be one of Irving's students. After telling him what I intended to do in my thesis, I was embarrassed to learn from a fellow student that one is supposed to ask for a topic. But Irving never demurred; he gave me free rein and helped launch me on a career. I shall repeat here something I wrote on the occasion of his sixtieth birthday, since it recounts an early experience that helped shape my mathematical life. His encouragement was strong when I was writing a
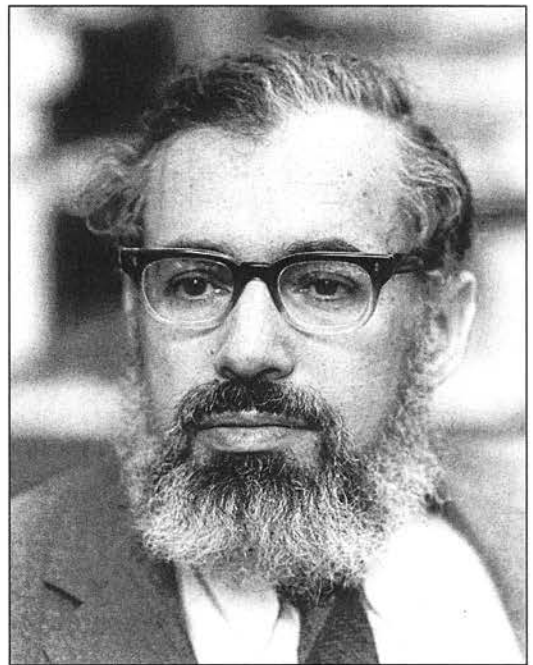
Irving Segal

thesis, and equally important was his total lack of encouragement when I found a result unrelated to anything beyond itself. One of the chief characteristics of Segal's work is that his theorems are part of theories, and this sense of the global nature of mathematical research was one of the most valuable things that he imparted to his students.

Segal had an extraordinary intuition for the essential. The work of N. Wiener and of R. H. Cameron and W. T. Martin on Brownian motion was tied to a particular representation; in Segal's hands, it became a general theory of Gaussian integration on Hilbert space. There is no orthogonally invariant Gaussian measure on an infinite-dimensional real Hilbert space, but Segal constructed the corresponding algebra of random variables. And he invariably produced new concrete results that followed from his abstract constructions. Similarly, quantum theory—especially of systems of infinitely many degrees of freedom—was tied to particular representations by operators on some Hilbert space. It was Segal who realized that the structure of physical relevance was the C^*-algebra generated by the observables, a discovery that was largely ignored at first and then became taken for granted. These two developments were unified in a theory of algebraic integration that applies to commutative and noncommutative systems alike, with applications to stochastic processes, a Plancherel formula for unimodular Type I locally compact groups, and implementability of canonical transforms in quantum systems of infinitely many degrees of freedom.

In all his work Segal was a pioneer. To mention one example not discussed elsewhere in this article, Sergiu Klainerman, in accepting the Bôcher Prize (*Notices*, April 1999), credits Segal with being the first to point out the role of space-time inequalities for nonlinear hyperbolic equations.

In the 1960s Segal organized two conferences at MIT that were the occasion of an initial breakthrough in constructive quantum field theory. The extraordinary subsequent development, primarily by James Glimm and Arthur Jaffe, was not along lines that Segal favored—a viewpoint that he made painfully clear.

The last thirty years of his professional life were dominated by a discovery he published in 1951. In the last section of a wide-ranging article [1], Segal initiated the theory of deformations of Lie algebras. (Deformations became "contractions" in the physics literature and were "limiting cases" in the article.) Classical mechanics is a limiting case of quantum mechanics as $\hbar \to 0$; the corresponding commutative Lie algebra is a deformation of the Heisenberg algebra. Nonrelativistic mechanics is a limiting case of relativistic mechanics as $c \to \infty$; the Lie algebra of the Galilei group is a deformation of the Lie algebra of the inhomogeneous Lorentz group. But Segal showed that the latter is itself a deformation of the Lie algebra of the conformal group, and now we have reached the end of the road: this Lie algebra is rigid.

Segal's vision was that the universe is the universal cover **M** of the conformal compactification of Minkowski space—Einstein's spherical universe—with the universal cover of the conformal group as symmetry group. He pursued this vision with passion and immense industry. In cosmology it yields an alternative explanation of the redshift as due to the difference between chronometric time and the time measured in an observatory. In quantum field theory the compactness of space in the Einstein universe (it is S^3) and a natural time cyclicity mollify the divergence problems. Together with Zhengfang Zhou, Segal constructed quantum electrodynamics and a nontrivial ϕ^4 quantum field on **M**. Here is a summary he wrote [2] in 1992:

Universal space-time is a natural candidate for the "bare" arena of the fundamental forces, being the maximal 4-dimensional manifold having physically indicated properties of causality and symmetry. It is locally conformal to Minkowski space, and globally conformal to the Einstein universe $\mathbf{E} \sim \mathbf{R}^1 \times S^3$. The Einstein energy exceeds that in the canonically imbedded Minkowski space, and the difference has been proposed by the chronometric theory to represent the redshift. Although this eliminates adjustable cosmological parameters, the directly observable implications of this proposal have been statistically quite consistent with direct observations in objective samples of redshifted sources. These developments represent a mathematical specification of proposals by Mach, Einstein, Minkowski, and Hubble and Tolman. They suggest that the fundamental forces of Nature are conformally invariant, but that the state of the Universe breaks the symmetry down to the Einstein isometry group. This provides an alternative to the Higgs mechanism, and otherwise has implications for particle physics, including the elimination of ultraviolet divergences in representative nonlinear quantum fields, the formulation of a unified invariant interaction Lagrangian, assignments of observed elementary particles to irreducible unitary positive-energy representations of the conformal group, and the correlation of the S-matrix with the action in **E** of the generator of the infinite cyclic center of the simply-connected form of the conformal group.

Why has this work not received an adequate evaluation? Part of the reason lies in Segal's style of scientific exchange—at times it resembles that of Giordano Bruno (later burned at the stake), who very shortly after his arrival in Geneva issued a pamphlet on *Twenty Errors Committed by Professor De la Faye in a Single Lesson*. But part of the fault lies with cosmologists and particle physicists intent on defending turf.

The time for polemics is past. Segal's work on the Einstein universe as the arena for cosmology and particle physics is a vast unfinished edifice, constructed with a handful of collaborators. It is rare for a mathematician to produce a life work that at the time can be fully and confidently evaluated by no one, but the full impact of the work of Irving Ezra Segal will become known only to future generations.

—Edward Nelson

Bertram Kostant

I was a graduate student at Chicago in the early 1950s, and I became Irving Segal's Ph.D. student in the 1951–52 academic year. I want to say a little bit more about how that came about. To do so, I should say something about what Chicago was like in the early 1950s. It always seemed to me that the graduate school environment at that time and place was unlike anything I have subsequently seen throughout my career. The place was teeming with students, and the intellectual atmosphere was such that one was made to feel that doing mathematics was the most important thing one could do with one's life. Perhaps the person who most contributed to this particular feeling in me was Irving Segal.

But back to the story about how I became his student. Frankly, Segal did not have a great reputation as a teacher. However, Chicago's graduate education system was such that there were certain courses in geometry, algebra, and analysis that one had to take. To fill the latter requirement, I found myself in Irving Segal's course in measure theory. To my surprise and delight it turned out to be a marvelous course. Segal worked very hard on it. Each lecture was slowly and carefully delivered. There were typed notes, there was no waving of hands, and every epsilon and delta was there. In fact, the whole set of notes produced a book, which in my opinion was superior to Halmos's newly published book on measure theory.

One day I noticed I had a counterexample to one of Segal's lemmas. I had never had a personal conversation with him, and my wrong impression was that he would not welcome one. It was at the urging of friends of mine that I finally mustered the courage to go to his office and show him my counterexample. He graciously agreed that I was correct. However, it was only a small matter. He had just neglected to add some rather natural hypothesis. As I was walking out the door he suddenly stopped me and asked, "What do you know about Lie groups?" I replied that I knew something about that subject, since I was currently taking a course with Ed Spanier on Lie groups. Without saying a word he went to his desk and started writing. He then got up, handed me a paper, and said, "Okay, here's your Ph.D. thesis problem." I was totally stunned. This was the beginning of a period in my life when I could not say "no" to Irving Segal. Here I had walked into his office just to discuss some small matter about his course, and I walked out not only having a thesis advisor but also having a particular thesis problem.

After that my graduate career radically changed. Segal was very good to his students. Also, I began to know him quite well on a personal basis. There was an intensity about Irving that resonated with me. He had an apartment off the Midway and on Friday nights held open house. These were the only occasions I can remember at Chicago where one could socially meet faculty members, visitors, and members of other departments. Segal would walk around joining small groups of his guests, becoming a catalyst for good conversations.

It is an understatement to say that he affected the course my life took after I became his student. Here are some details supporting that statement. It was through Irving that I got a two-year appointment, starting in 1953, to the Institute for Advanced Study (even before I began writing my thesis). This was a rare opportunity, since among other things I met such luminaries as Einstein, von Neumann, and Hermann Weyl not very long before their deaths. After my stay at the IAS I am sure it was due to Irving's influence with W. Feller that I received a one-year offer as Higgins Lecturer at Princeton. After that I went to Berkeley on my own. But it was after only a few years, while still an assistant professor, that I received an offer of a full professorship at the University of Chicago. I can only imagine that this was engineered by Irving and perhaps Adrian Albert.

But leaving a rising Berkeley (and the beauty of California) to go to what I sensed was a declining Chicago was not terribly appealing to me. This was my first "no" to Irving. A few years later Irving moved from Chicago to MIT. Not long after that, I received a full professorship offer from MIT. By this time (1961–62) my interest in Berkeley had already peaked. It would be painful to break a strong

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tie I had developed with Gerhard Hochschild, but Irving convinced me that mathematically Boston was the place to be. "The winters are not that bad. Sure, it snows a lot, but you can learn to cross-country ski." I accepted the offer from MIT. But this began the period when I found it easier to say "no" to Irving.

One of the reasons our work went separate ways was that Irving really focused only on those aspects of mathematics, and in particular only on those aspects of representation theory, that he felt dealt directly with physical theory. He ignored the revolution brought about by Harish-Chandra and I. M. Gelfand. Besides becoming interested in that development, I also became interested in geometry and other areas that it seemed Irving found easy to ignore. Irving was single-mindedly driven to find the right mathematical models to describe certain physical theories, such as cosmology and quantum field theory. In his later life, I think, cosmology superseded quantum field theory. At the heart of the cosmology theory that Segal developed was the 15-dimensional Lie group $SU(2, 2)$, referred to by physicists as the "conformal group". He focused all of his attention on this group. There are certain properties of this group that he felt were at the heart of understanding important things.

Irving often pointed to certain phenomena that turned out to be the tips of icebergs. For example, he was fascinated by the fact that the conformal group stabilized the solutions of the wave equation even though the wave operator did not commute with the group. He asked me about this, and it seemed indeed to be an interesting question. I thought about it and wrote a paper called "Quasi-invariant differential operators" which made connections between a number of things, including intertwining operators on Verma modules. The latter subject was carried to deeper levels by the Gelfand school and eventually led to the Kazhdan-Lusztig theory, an important development in modern Lie theory.

Another aspect of the conformal group that fascinated him was that its Lie algebra has elements that, for many representations, have a nonnegative spectrum. From his perspective this could make them candidates to represent energy in physical applications. One particular nilpotent element, in the representation of $SU(2, 2)$ associated with solutions of Maxwell's equations, defines the standard operator to determine the frequencies of light waves. But Irving focused on another element with a nonnegative spectrum, an element that was elliptic and not nilpotent, but closely related to the above-mentioned nilpotent element via a theorem of Morosov. This elliptic element has beautiful mathematical properties, like generating an invariant cone. This is the tip of another iceberg. I became involved in this study, producing a theorem determining exactly when invariant cones in semisimple Lie algebras exist. A closer study of such cones is today an active subject. This elliptic element is at the heart of Segal's cosmological theory. What he is saying is that it is the elliptic element that should be used to determine the energy of an electromagnetic wave, and not the nilpotent element. There was no big bang and no expansion of the universe. The redshift is not a Doppler effect. It is accounted for by the difference between the elliptic and nilpotent elements—negligible locally, but significant at great distances. Although his cosmological theory has thus far attracted very few supporters, there is clearly much that is unsatisfactory in the widely accepted big bang theory. I have it from a highly reliable but unnamed source that there is a growing group of cosmologists who have come to believe that the correct understanding of the redshift is some sort of fusion of the Doppler effect and Irving's theory. So it is not impossible that Irving could turn out to be correct after all.

Irving Segal was a unique individual who affected the lives and thoughts of a large number of people, certainly including me. With his passing I think the world is a poorer place.

Edwin F. Beschler

I first met Irving when, as acquisitions editor for Academic Press, I was seeking someone to establish a journal in the field of functional analysis. This was in the early 1960s when the boom in specialized journals was about to begin. Irving's name, along with that of Ralph Phillips, with whom I had also spoken, was among the most often mentioned. When I approached him, his response was incisive and immediate—almost as if he had anticipated the question. With a clear understanding of editorial autonomy and assurance of support from Ralph and at least one other colleague, he agreed to undertake the task. In short order he had brought Paul Malliavin into the group, an agreement was reached within a few months of the first discussions, and the first issue of the *Journal of Functional Analysis* (JFA) appeared not more than a year later. Irving was not one to procrastinate.

The concomitance of the three editors' views and the firm leadership provided by Irving was remarkable. Through the next twenty years, editorial board meetings consisted of a get-together of the four of us for coffee or tea every four years at the International Congress of Mathematicians (I missed one or two), with agreement that everything

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was fine. In between, the JFA worked smoothly and efficiently, and it was always a pleasure to deal with Irving and the board. In my tenure I do not recall a single problem that was not handled fairly and expeditiously. If there were editorial problems of which we at the publishers were unaware (and one suspects they arose, as they surely do in even the best-ordered groups of researchers), it was another mark of Irving's style that he settled them with the least amount of fuss possible. He managed to create, from my point of view, a model of that peculiar mix of autocracy and democracy required to make a journal work. The model served me well in the following years, though I cannot say I was often able to replicate it.

On a more personal note, I remember with nostalgic amusement my arrival, along with Irving and at least another one hundred or so members of the AMS, at Shmeretvyo Airport in Moscow in 1966. In those days one did not learn the name of one's hotel assignment until arrival at the airport. Our group found itself lined up in front of a small table, staffed by two Intourist employees with a smattering of English and armed with a ledger book in which was inscribed each of our groups' names, in Cyrillic—and I suspect not even in that alphabet's order. The procedure was that the first person in line pronounced a name and then a search through the list was conducted, attempting to find a reasonable match. It was obvious after the first two or three such searches that the process would take all night. Rising above the growing din of complaints was Irving's voice, coming from far back in the queue as he approached the table, protesting something like "NO, NO, NO!! This will never do!" Irving firmly commandeered the book, began at the top of the list, and called out the name of the first person on the list, then the second, and so on. The Intourist employees were startled and, I think, uncertain as to whether to be angry or simply amazed. They apparently had never seen such a performance nor imagined such a procedure. Irving was in charge, and the sense of gratitude among the group was palpable. He was not able to save us from a six-hour wait in our hotel's lobby for room assignments, but I know he saved us an equal amount of time at the airport.

John C. Baez

I met Irving Segal in 1982 shortly after I came to MIT in order to get my Ph.D. in mathematics. As a slouching, scruffy graduate student who pre-

Ph.D. Students of Irving Segal

Isadore M. Singer, Chicago (1950)	John Chadam, MIT (1965)
Henry A. Dye Jr., Chicago (1950)	Jan M. Chaiken, MIT (1966)
Joseph M. Cook, Chicago (1951)	Robert R. Kallman, MIT (1968)
Ernest A. Michael, Chicago (1951)	Michael Weinless, MIT (1968)
Ernest L. Griffin Jr., Chicago (1952)	Michael J. J. Lennon, MIT (1969)
Jacob Feldman, Chicago (1954)	Niels Skovhus Poulsen, MIT (1970)
Bertram Kostant, Chicago (1954)	Tomas P. Schonbek, MIT (1970)
Lester E. Dubins, Chicago (1955)	Arthur Lieberman, MIT (1971)
Edward Nelson, Chicago (1955)	Abel Klein, MIT (1971)
Brian Abrahamson, Chicago (1957)	Stephen Berman, MIT (1972)
Ray A. Kunze, Chicago (1957)	Steven Robbins, MIT (1973)
W. Forrest Stinespring, Chicago (1957)	Edmund G. Lee, MIT (1975)
Robert J. Blattner, Chicago (1957)	Hans Plesner Jakobsen, MIT (1976)
Leonard Gross, Chicago (1958)	Bent Ørsted, MIT (1976)
David Shale, Chicago (1960)	Thomas P. Branson, MIT (1979)
Walter A. Strauss, MIT (1962)	Mark A. Kon, MIT (1979)
Roe W. Goodman, MIT (1963)	Stephen M. Paneitz, MIT (1980)
Matthew Hackman, MIT (1963)	Derrick C. Niederman, MIT (1981)
A. Robert Brodsky, MIT (1965)	John C. Baez, MIT (1986)
Richard B. Lavine, MIT (1965)	Jan Pedersen, MIT (1991)

ferred to be barefoot whenever possible, I was somewhat intimidated by his appearance. He was always impeccably dressed in a suit, he wore a goatee shaved short in a no-nonsense sort of way, and he made up for his lack of height by an erect posture and commanding manner. But I decided to work with him because of all the pure mathematics faculty, he seemed the most passionate about physics, not just as a source of mathematics problems, but as an end in itself.

I wanted to work on quantum gravity, but at MIT everyone interested in this subject was working on superstrings, for which I had little taste. Segal himself found Einstein's equations too ill-behaved to bother trying to quantize them. The lack of a conserved energy, the tendency for solutions to develop singularities—these qualities convinced him that general relativity was fatally flawed. My arguments in favor of general relativity failed to convince him, so I wound up working on one of his specialties, the mathematical foundations of quantum field theory.

I learned a lot and successfully completed a thesis, but I did not have much success proving really interesting theorems. Later, as a postdoc, I decided that quantum field theory was too hard for me, so I worked with Segal and Zhengfang Zhou on classical field theory, i.e., nonlinear wave equations, another of Segal's specialties. The three

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of us wrote some papers together and also coauthored a book [3] summarizing Segal's work on quantum fields. Thus I spent about six years in close contact with him and came to know him rather well.

We would typically discuss mathematics in his office, taking turns scribbling equations on the blackboard. He had a devastating way of expressing doubt when my reasoning failed to convince him. Without saying a word, he would gradually raise his eyebrows higher and higher as I spoke. As they slowly climbed up his forehead, it became ever more difficult to keep up the momentum of my reasoning. When I finally lost the thread of what I was saying, he would interrupt and point out my error as he saw it. Being stubborn, I would not always accept these criticisms. As he was even more stubborn, our discussions sometimes became quite heated. Zhengfang Zhou served as a calming influence when he was around.

Segal's office was a cozy, lived-in place, cluttered with decades of accumulated papers. He had a couch where sometimes he would take short naps. He also made coffee in his office, refusing to touch the stuff served in the mathematics department lounge. He took coffee very seriously, grinding the beans in his office, using only distilled water, and heating it to a precisely optimized temperature. (He claimed to have done a study to determine this optimal temperature.) He often let me work on his computer while he worked at his desk or typewriter. Sometimes when he wanted to prove a theorem, he made a great show of setting a kitchen timer, allowing himself no more than thirty minutes to get the job done. This was but one of many ways he emphasized the importance of a businesslike attitude. When I passed my thesis defense, the first thing he said was "Good, now we can get back to work." He never slacked off; he often came to the office on weekends, and his retirement seemed not to slow him down in the least.

People who failed to understand the essentially prickly nature of Segal's relationship to the world would sometimes misinterpret his actions. For example, he recently wrote a review of Alain Connes's *Noncommutative Geometry* for the *Bulletin of the AMS*. While largely positive, the review contained a number of serious criticisms. For example, he expressed disappointment that Connes, with all his mastery of analysis, still treated quantum field theory the way most particle physicists do, using perturbative Lagrangian methods rather than the more rigorous framework of algebraic quantum field theory pioneered by Segal and others. Some mathematicians were greatly upset by these criticisms. What they perhaps failed to understand was that merely by writing the review, Segal was saying that Connes's work was of the highest caliber! Indeed, the only other articles about the work

of others I recall his writing concerned von Neumann and Wiener.

In his later years Segal spent most of his time on an alternative to the big bang cosmology in which redshifts were to be explained, not by the expansion of the universe, but by an effect of conformal geometry. According to him, his theory predicted a quadratic redshift-distance relation instead of the usual linear one. He spent a lot of time statistically analyzing redshift-brightness data for quasars and galaxies and wrote papers claiming they supported his theory. Most astronomers disagreed.

I thought long and hard about his derivation of the quadratic redshift-distance law from his theory, and it never seemed right to me. At first I hoped I was making a mistake, so I tried to get him to explain this derivation. His explanation did not convince me. Later I tried to explain what I thought was his error. He became quite angry. When I realized we would never see eye-to-eye on this subject, I tried to avoid it. But this was very difficult, and our relationship became strained. I am sad to say that I eventually wound up avoiding him.

Despite this, I remain very fond of Segal, because he had a real passion for understanding the universe. He did not believe in God and was suspicious of all forms of organized religion. The quest for perfection that some express through religion he expressed through mathematical physics. He could never take it lightly!

Arthur S. Wightman

I first encountered Irving Segal in the winter of 1946–47 when he spoke in a seminar in (old) Fine Hall at Princeton on the results of his forthcoming paper on postulates for general quantum mechanics. I was only partly prepared for the grand sweep of the talk and the paper that followed. I had studied John von Neumann's book *Mathematical Foundations of Quantum Mechanics* in the Dover reprint of the German edition of 1932, available during the war, and had heard of the work of Gelfand and Naimark on C^*-algebras discussed in a mathematics seminar in New Haven, but I did not know of the existence of von Neumann's paper on an algebraic generalization of the quantum mechanical formalism, which Segal mentioned as being most closely connected with his work. Both papers can be regarded in retrospect as part of a mathematical reaction to the physical discoveries of the quantum mechanical revolution of 1925–1927. They had a twofold motivation: on the one hand, to distill the essence of the mathemat-

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ical structure of quantum mechanics and, on the other, to state its principles in a form that might make it possible to go beyond quantum mechanics. The latter was surely a prime impulse of P. Jordan in the paper that led, via the joint investigation of Jordan, von Neumann, and Wigner, to von Neumann's paper.

As Segal pointed out, the most conspicuous example of a system of observables satisfying his postulates is the set of all self-adjoint elements of a C^*-algebra. He left as an open problem to prove whether up to isomorphism these were the only such. In the decade after the appearance of the postulates this problem was studied by a number of authors, of whom I will mention only two: David Lowdenslager and Seymour Sherman. These authors constructed a rich class of examples of Segal systems of observables that are not isomorphic to the self-adjoint elements of a C^*-algebra. Furthermore, they gave necessary and sufficient conditions for Segal systems that such an isomorphism hold, thereby completing Segal's postulate system. Segal's insight that the C^*-algebra is the object with physical meaning and not any particular representation of it on a Hilbert space is now a commonplace of mathematical physics.

Meanwhile, in physics Rudolf Haag was struggling to understand how the fact that physical measurements take place in bounded regions of space-time should affect the structure of algebras generated by observables. He worked originally with algebras of unbounded operators, but over the course of a decade, in part in joint work with Huzihiro Araki and Daniel Kastler, he came to the conclusion that algebras of bounded operators, and in particular C^*-algebras, provided a language best suited to the expression of the ideas of quantum field theory, which is what Segal maintained in the first place. In the hands of Haag, Doplicher, and Roberts localization in space-time and the Gelfand-Naimark-Segal construction led to a systematic theory of superselection rules. This was the beginning of a profound theory created over three decades and summarized in Haag's 1992 book.

Leonard Gross

Irving Segal always had lots and lots of ideas. I remember when, in 1958, I returned for a few days to the mother institution, the University of Chicago, for my Ph.D. exam after being away for almost a year. At the end of the visit, as Irving drove me back to the bus station, he used every minute to provide me with a goodly supply of ideas to keep me busy after I went back out into the wilderness. I

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was not able to absorb much of this. His knowledge base was much more sophisticated than mine. His ideas came forth quickly. Even after I returned to Yale I received letters from him raising interesting questions close to my area of expertise. In retrospect I realize that he was driven not only by his sense of duty to provide his intellectual progeny with plenty of food for the mind but also by his single-minded determination to solve one of the big problems of mathematical physics: the existence of interacting quantum fields. Although much of his work may seem to many mathematicians to be motivated simply by the usual aesthetic considerations—and is certainly justified by the intrinsic beauty of his ideas—Irving told me a few years ago that all of his work was aimed in one way or another at understanding quantum physics.

Among the many papers of Irving's that formed the core of my mathematical education, one group of his papers influenced my own work and the work of my students in two distinct directions. Irving's papers [4, 5, 6] were aimed at understanding the mathematical structure of the Hilbert spaces associated to a variable number of identical quantum mechanical particles. Although a significant part of the problems he addressed pertained to integration over an infinite-dimensional Hilbert space, some of the ideas of these papers are most easily understood in finite dimensions. Let p_t and μ_t, for $t > 0$, be the heat kernels on $\mathbb{R}^n$ and $\mathbb{C}^n$ respectively. One need only write out the convolution $p_t * f$ to see that if f is in $L^2(\mathbb{R}^n, p_t(x)\, dx)$, then $p_t * f$ has an analytic continuation, h, to the entire complex space $\mathbb{C}^n$. The map $S_t : f \mapsto h$, the Segal-Bargmann transform, is a unitary operator from $L^2(\mathbb{R}^n, p_t(x)\, dx)$ onto the space $\mathcal{H}_t^2$ consisting of holomorphic functions in $L^2(\mathbb{C}^n, \mu_t(z)\, dx\, dy)$. Furthermore, the Taylor coefficients at 0 of the holomorphic function h may be assembled so as to define an element α of the space of all symmetric tensors over the dual space $(\mathbb{C}^n)^*$. The Taylor map $T : h \mapsto \alpha$ is also unitary, this time with domain $\mathcal{H}_t^2$ and range equal to the "Fock space" $\mathcal{F}_t$ consisting of those symmetric tensors with a finite (t dependent) norm. Now the overall unitary map $TS_t : L^2(\mathbb{R}^n, p_t(x)\, dx) \to \mathcal{F}_t$ can be described in many other ways: there are Hermite polynomials lurking in these maps. But the description of these maps given above provides a stepping stone to some recent extensions. Just a few years ago Brian Hall generalized the Segal-Bargmann transform, replacing $\mathbb{R}^n$ by a compact, connected, simply connected Lie group and $\mathbb{C}^n$ by the complexification of the group. Soon afterward, Bruce Driver proved that the Taylor map in that context is also a unitary map in a natural way. A survey of these theorems and their link to the work of Segal, Bargmann, Cameron and Martin, and P. Krée is given in [2].

Actually, Irving focused primarily on the infinite-dimensional versions of these isomorphisms over linear spaces: one must replace $\mathbb{R}^n$ by an infinite-dimensional real Hilbert space, H. There are substantial problems in giving orthogonally invariant meaning to $L^2(H, p_t(x)\,dx)$ when H is infinite dimensional. For example, if one chooses an orthonormal basis of H, and thereby identifies H with l^2, then the measure $p_t(x)\,dx$, when $n = \infty$, has a clear interpretation as a product measure on $\mathbb{R}^\infty$. But the subset l^2 has measure zero. There is no way to interpret the expression for $p_t(x)\,dx$ as a countably additive measure on H. One always needs to choose some kind of enlargement of H on which the measure will sit.

Typically, all really useful enlargements have some orthogonal noninvariance built in. The classical example is that of Wiener space C, consisting of all continuous real-valued functions on $[0, 1]$ vanishing at 0. C carries a natural probability measure, namely Wiener measure. The subspace C' consisting of absolutely continuous functions with square integrable derivative is a Hilbert space. When $H = C'$, the proper interpretation of the informal expression $p_t(x)\,dx$ (for $t = 1$) is precisely Wiener measure on C. While C is recoverable from the Hilbert space C' as the completion with respect to the supremum norm on C', this norm is not orthogonally invariant.

In order to emphasize the central role of the Hilbert space H, as opposed to the accidental form of some convenient ambient measure space, Irving gave a definition of integration over H with the help of an equivalence class of measure spaces. Although the theorems and technology in these papers have influenced much mathematical activity, the slightly complicated, though orthogonally invariant, meaning that he gave to the expression $p_t(x)\,dx$ has not been as widely adopted. This writer, strongly influenced by Irving's view of the primacy of H in infinite-dimensional Gaussian integration theory but forced by my foray into infinite-dimensional potential theory to have the measure $p_t(x)\,dx$ live in some Banach space, abstracted the ordinary Wiener space: if one completes the Hilbert space H with respect to a second, extremely weak norm, then the completion will support a measure that can, in a precise sense, be interpreted as the measure $p_t(x)\,dx$. The influence of one part of mathematics on another is quite visible here: few of the probabilists who are the current users of these abstract Wiener spaces have an interest in or knowledge of the quantum field theory problems that led Irving to study these structures.

Michèle Vergne

I met Irving Segal at the meeting of the American Mathematical Society in Williamstown in 1973. One year later my mother committed suicide, and this dumped me into devastating thoughts. I stopped working. A friend in the United States, Graciela Chichilnisky, persuaded me, rightly, to move from France to the U.S. Segal was the attractive presence at MIT. Of course, attractive also were the other giant figures in the field of group representations—Sigurdur Helgason and Bert Kostant. But Segal had a special talent for making one feel wanted. In front of him I felt unimportant and little, but I felt that my work was needed. He soon became essential to me. I remember something he said to me: "You do not need to have many friends. One is enough." So I had two friends—my friend Graciela and him—and this was more than enough to make life worth living again. He was fascinating for me, an immense spiritual power in a tiny body. He was passionately interested in his ideas; intellectual work was ranked by him above all other activities. Maybe in a more radical manner, all other work, especially all traditional feminine duties, were considered as no work at all, just pleasurable distractions. To his credit, unlike most of us, he would also take care of all material issues. In fact, nothing seemed to be difficult for him. He cared for others, especially children, with great pleasure. My daughter, when she was a little girl, loved to come into his office, get an orange, and start a spirited conversation with him. My father came to the U.S. to visit me. Segal kindly invited us to his house. What a shock for my father to see a man of his age, and a professor, serving him. But he was not doing so as an obligation due to liberal beliefs. He was just happy taking care of others. Indeed, there was something highly charming in him that nobody could resist.

With Hugo Rossi I had done some work before 1974 on certain special unitary representations of semisimple Lie groups. This work was of high interest for Segal's cosmological theory. My past work gained immediate significance. Segal had projects where my contribution was impatiently expected. Due to his influence and strong will, I was able to work again. In fact, I remember those first years at MIT as one of the most happy periods in my life. I would again work and work and work and report on my work almost every day to Segal. Hans Jakobsen and Bent Ørsted were Danish students of Irving. They were bright, friendly, and amusing. Birgit Speh, a student of Bert Kostant, was also very often with us. Later on, there was also Steve Paneitz, who died tragically when swimming with Segal in a lake. We would all meet regularly for an informal seminar in his office. We could also go for long walks along the Charles River or have dinner at his home. As if prepared by a genie of fairy tales, suddenly in his house there was a dinner ready for

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everybody, prepared by him, while we discussed cosmology passionately.

The model that Segal proposed for space-time is a model where space is finite but the time infinite. This space-time, the 3-dimensional sphere for space variables and the real line for the time variable, can be equipped at each point with the cone of possible directions of the future. The group of causal transformations of this manifold is the universal cover of the identity component of the indefinite orthogonal group $O(2, 4)$, a 15-dimensional symmetry group. This group, the conformal group, became my favorite group. It contains the Poincaré group of symmetries of the usual Minkowski space. Segal's cosmological space is deduced from the Minkowski space by compactification of the space variables. Let us call the infinitesimal generator of time translation in Segal's space the Segal energy. A representation of the conformal group (more precisely of its universal cover) has positive energy if Segal's energy has positive discrete eigenvalues. Many questions were raised by Segal for describing all positive energy representations, their tensor products, the description of their K-types, etc. Segal's work is sometimes highly conceptual, as are his fundamental discoveries of the metaplectic representation or of the abstract Plancherel theorem, and sometimes very applied and concrete. In particular, Segal's work led to a detailed study of representations of the conformal group.

It was challenging for me to apply my knowledge of small representations to this special group. It was not easy to obtain concrete results as needed and to recognize well-known physical equations in my purely mathematical world. In these projects everybody around Segal was adding his or her own contribution to Segal's work. We were all working incredibly hard. Many results were obtained in a small amount of time. Results obtained in the particular example of the conformal group had impact for the general theory of representations. The invariance of the wave equation under the conformal group had a fundamental significance for Irving Segal. It was proved by Bert Kostant. Masaki Kashiwara and I showed that invariance of the wave equation implied invariance of the Maxwell equation. Hans Jakobsen proved the unitarity of the representation of the conformal group in the space of solutions of the Maxwell equation. Birgit Speh described the list of K-types of some of the positive energy representations. This list of asymptotic directions in K-types led to the general concept of singular support of a representation. Bent Ørsted studied relations of these representations to nilpotent orbits contained in an invariant convex cone. Steve Paneitz studied the image of some of the nilpotent orbits of the conformal group under the moment map. He also classified all possible invariant cones. Bert Kostant and David Vogan were consulted as experts on all these issues.

There was a friendly competition among us. Work was the most important part of my life. But, following Segal's example, I found nothing more pleasurable in life than thinking and working. In fact, between 1975 and 1981 all my work on representations was inspired by Segal's demands. I was not, properly speaking, working for him; I was pursuing my own research, but I was cheered up by the pleasant idea that it was useful to him. Now many mathematicians continue to work following paths Segal opened on positive energy representations, semigroups of causal transformations, and decomposition of representations related to the metaplectic representation.

Spoken at the memorial service: Once again I am here at MIT. But this time, Irving, I will not be able to knock at your door; enter your office; see you, a very small man welcoming me warmly behind huge piles of papers. You would start an impish conversation about mathematicians, colleagues, astronomers, life. It would be highly amusing. You had posted proudly in your office a drawing done by your daughter, Karen, representing you as a little devil. It was quite true to life; indeed, you loved to be provocative. To be sad or depressed was a form of weakness, to be sick was not allowed, to be unsure of myself was to draw your fire on me. In front of me, you considered women with open contempt, maybe just to know how I would react. But you certainly were influential in attracting me to MIT and took me onto the board of editors of your journal. Today I am very sad. I also feel that I did not always behave right towards you. I loved you, but it was not easy to be oneself and stand in front of you.

Dear Irving, you had the strong power to influence the directions of people's lives. You have given to many people the love of research. You employed your charm to develop the creativity of those around you. I am very grateful to you. You gave me essential help when my life was darkened by tragic sorrows. You made me a better mathematician. You taught me that friendship was sacred. If I needed you, you would always be there. And today, you are here.

References

[1] JOHN C. BAEZ, IRVING E. SEGAL, and ZHENGFANG ZHOU, Introduction to algebraic and constructive quantum field theory, Princeton Series in Physics, Princeton Univ. Press, Princeton, NJ, 1992.

[2] L. GROSS and P. MALLIAVIN, Hall's transform and the Segal-Bargmann map, *Ito's Stochastic Calculus and Probability Theory* (Ikeda, Watanabe, Fukushima, Kunita, eds.), Springer-Verlag, Tokyo, 1996, pp. 73–116.

[3] I. E. SEGAL, A class of operator algebras which are determined by groups, *Duke Math. J.* **18** (1951), 221–265.

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[4] _____ , Tensor algebras over Hilbert spaces, *Trans. Amer. Math. Soc.* **81** (1956), 106–134.

[5] _____ , Mathematical characterization of the physical vacuum for a linear Bose–Einstein field, *Illinois J. Math.* **6** (1962), 500–523.

[6] _____ , The complex wave representation of the free boson field, *Topics in Functional Analysis: Essays Dedicated to M. G. Krein on the Occasion of His 70th Birthday*, Adv. in Math. Suppl. Stud. 3 (I. Gohberg and M. Kac, eds.), Academic Press, New York, 1978, pp. 321–344.

[7] _____ , Fundamental physics in universal space-time, *Physics on Manifolds* (Paris, 1992), Kluwer, Dordrecht, 1994, pp. 253–264.