

Irving Kaplansky's Role in Mid-Twentieth Century Functional Analysis

Richard V. Kadison

The mathematical work of Irving Kaplansky, who passed away in June 2006, is largely devoted to algebra. Irving's base camp in mathematics was undeniably algebra. But all of us in functional analysis classify Irving as a *mathematician*—someone capable of working in and drawing inspiration from all areas of mathematics. He is considered by all my coworkers in operator algebra theory to be one of the greatest figures of the early days of the subject.

The role of algebra in functional analysis is mainly one of formulation—but it's a crucial role. Any analyst who hasn't understood that fact has not understood the large picture. The *idea* of what we are doing is often synonymous with the *formulation*. Algebraic formulation gets us to consider certain constructs and to ask provocative questions in functional analysis: Is such-and-such an algebra simple or semi-simple? Can we describe the ideals? Are the four or five sensible ways of viewing semi-simplicity the same (as they are known to be in the finite-dimensional case) or to what extent do they differ? What are the automorphisms and derivations like? The methods and arguments for what we are doing in functional analysis come mostly from analysis (in the broadest sense), but algebra often provides us with clever "tricks".

At this point, an account of Kap's specific contributions to functional analysis with references to his bibliography might be most appropriate. However, those contributions are numerous and basic; so much so, that anything but a dry recital would surely exceed reasonable space limitations.

Richard V. Kadison is professor of mathematics at the University of Pennsylvania, Philadelphia. He is currently visiting Louisiana State University, Baton Rouge. His email address is kadison@math.upenn.edu.

In addition, we have Irv's "Selecta" [IKa1] with his "afterthoughts", which contains a few of the articles that constitute his *published* contribution to functional analysis. Some of the articles that had a profound influence on functional analysis do not appear in his Selecta. My guess is that Kap considered those articles as "routine" (that is, not having caused him particular difficulties in completing) and was only dimly aware (if at all) of the great stir and large amount of further research they stimulated. He probably also felt that he could not append a sufficiently authoritative "afterthought"; all this, of course, in the context of Irv's true humility and complete honesty.

Let me touch on just a few items—doing some of the mathematics only for those that do not appear in the Selecta. First, and foremost in technical utility, is the celebrated "Kaplansky Density Theorem". In an "afterthought", Kap quotes G. K. Pedersen's book [GKP1, p.25], "The density theorem is Kaplansky's great gift to mankind. It can be used everyday, and twice on Sunday[s]." It was reported to me that when Arveson heard that quote, he remarked, "I use it twice on Saturdays, too." The title of my lecture at the memorial conference for Irv at the Mathematical Sciences Research Institute in Berkeley was "And Twice on Sundays". The two great density theorems, von Neumann's "Double Commutant Theorem" [JvN1], the first theorem of the subject of operator algebras, and Kap's density theorem were discussed along with my "transitivity theorem" [RKa3] (also a density theorem), which relies heavily on the Kaplansky Density Theorem. I was happy to note that the proof of the transitivity theorem has at its heart a Picard-successive-approximation argument that uses Kaplansky Density at each one of its countably (infinite) many stages—and there it was, only Friday! Another of Kap's "afterthoughts" on his

density theorem includes the observation, “the applicability of a piece of mathematics is hard to predict,” to which I shall add, “Amen.” So much mathematics has been applied in vital and totally unanticipated ways.

Irv was plagued by people who asked, “Why did you do this or that mathematics?” He appropriated a famed mountaineer’s answer to the question, “Why do you want to climb Everest?” namely, “Because it is there.” From early observation, I concluded that Irv was driven by internal beauty in mathematics. Again from observation, Irv was quite tolerant of “applicability driven” researchers. Gert Pedersen was also driven by the beauty in mathematics (again, from extended observation). He is quoted in another of Irv’s “afterthoughts” [IKa1, p.86] as saying during a lecture at UC Berkeley, about Irv’s introduction of AW*-algebras, “the subject refuses to die.” In this same “afterthought”, a Godement review is paraphrased as saying, “What is the point of this generalization from W^* to AW*, except, perhaps, to offer simplified proofs?” To which Irv replies, “I am pleased that he noticed the simplified proofs.” Irv also adds, “As for the main charge, I plead guilty and throw myself on the mercy of the court.” Why “guilty”? Well, such “charges” are not easy to defend against by yourself. Anything you say in your own defense seems to be coming from the very deep hole of self-serving self-interest. Many of us deal with it as Irv did, in essence, sometimes with the comment, so popular these days, “Whatever!” (roughly the equivalent of the older “Have it your way,” or “Suit yourself”).

It is worthwhile to discuss the possible motivations for those comments of Godement’s and Pedersen’s, not just for their “gossip” aspects, but for some deeper understanding of what Kap had contributed by his introduction of AW* algebras. To begin with Godement was a clever and strong young worker, in the earliest stage of the development of the theory of unitary representations of locally compact groups. Irving Segal was one of the great early pioneers and originators of that theory, especially as it concerned “operator algebras” as group algebras for such groups and the connection between the Hilbert space representations of each. Of course, there was the great Gelfand and his friend and collaborator Neumark (not to go into the details of the magnificent Soviet school—Krein, Raikov, Silov, and others), who were an inspiration to Segal, and largely through Segal, to Kap and to me. Godement was a rising young star in that representation theory, attracting a good deal of attention and, of course, plenty of French support. That was enough to put Segal squarely “on his case”. A bitter mathematical controversy erupted and played out in the pages of the *Mathematical Reviews* of those days. Now, the point: I would guess that Godement was

taking a “swipe” at Kap with his AW* comment, whether consciously or not, whom he regarded as an ally and friend of Segal, certainly a colleague and member of the great Chicago school. That comment was very likely regarded by Godement as a small skirmish in his larger battle with Segal. Godement was much too honest to make that comment without including that (vitiating!) addendum on “simplified proofs”. There was a double irony, indeed, a web of ironies, here. For one thing, Segal and Kap were not on good terms at the time (but, more about that at a later point). Above all, most of the main players seemed to have missed the point to Kap’s introduction of AW* algebras: It was important (I would elevate that to *crucial*) to separate what was algebraic in the theory of von Neumann algebras from what required analytic (that is, primarily, measure-theoretic) considerations. Kap felt this instinctively, although the crushing weight of evidence for that importance had not yet mounted when he poured his time, thought, and energy into that project. In that same “afterthought” [IKa1,p.86], Kap refers to [RKa1] as “worthy of note”. I hope so; it provided the representation-independent characterization that people had sought of the von Neumann algebras (“rings of operators”, “ W^* -algebras”). The search for such a characterization was one of the two problems in this area that fascinated von Neumann most. (It is relevant for the application of such operator algebras as models of many quantum-mechanical systems—allowing us to select the family of states, the “normal states”, that is appropriate to the particular expectations to be measured, by passing to the correct, Hilbert-space representation.) That characterization would not have been found if Kap had not made clear the need to separate and study the algebraic properties of such algebras. The other information vital to producing the characterization [RKa1] of von Neumann algebras, which was almost, but not quite, available to von Neumann, is the 1943 characterization [G-N] of C^* -algebras by Gelfand and Neumark. (Von Neumann had ceased active work in the area by that time, but always retained a strong interest in the subject.) Also needed was Segal’s recognition [Se1] that [G-N] embodied the representation techniques of the GNS construction. It seems appropriate to mention, as well, the very pretty and popular characterization of von Neumann algebras [Sa1] by Sakai as those C^* -algebras that are isometrically isomorphic to the (norm-)dual of some Banach space.

As Irv notes [IKa1, p.86], still with reference to [RKa1], “By assuming suitable least upper bounds not just for projections but for all self-adjoint elements, one intrinsically characterizes W^* -algebras.” However, the “suitable” (least upper bounds) masks the all-important assumption that

there is a family of “states” of the algebra (“normal states”—order-preserving, linear functionals taking the value 1 at the identity operator) that respects the upper bounds and that “separates” the algebra (two elements with the same values at all those states are identical). It is that assumption that distinguishes the essentially algebraic from the measure-theoretic. Dixmier [JD_i] had recognized that in the abelian case (the “classical” measure-theoretic situation), by providing an example of an abelian AW*-algebra that is not isomorphic to any abelian von Neumann algebra (such a von Neumann algebra is isomorphic to the algebra of bounded measurable functions on some measure space). It was, then, certain that the assumption of measures (or their corresponding integrals, the normal states) for the algebra was necessary for it to be a von Neumann algebra. Conjoining that assumption with some (infinite) “algebraic” assumption was then completely natural and what a few of us “dreamed” (rather than “conjectured”) might be true. My order assumption on monotone nets (sequences, in the separable case) was the obvious way for me to go as I had recognized, during my thesis work, that the best way to study operator algebras, in the presence of noncommutativity, was through their order structure (as partially-ordered vector spaces). In his *Rings of Operators* [IKa2], Kap changes his mind about the proper way to axiomatize AW*-algebras [IKa1, p.86]. “As noted in the preface to [4], I later changed my mind about the proper way to axiomatize AW*-algebras. Why did I do it clumsily in the first place? Lame reply: The process of taking the least upper bound of a set of orthogonal projections was so fundamental and so heavily used that I slid into making it an axiom. However, more was needed since there might not be any projections other than 0 and 1. Making an assumption about maximal commutative subalgebras was unfortunate: Zorn’s lemma had to be invoked every time the axiom was used.” For me and most of my coworkers, I daresay, the use of Zorn’s lemma is no hindrance. I am not in sympathy with Kap’s reasons for renouncing the condition on maximal abelian algebras (very likely, because I view it as an analyst). The maximal abelian algebra is the “protective container” in which the analyst can carry all the vitally needed classical analysis on the journey into the noncommutative. It is unthinkable to research workers in the theory of von Neumann algebras to be without their maximal abelian subalgebras (whenever and wherever they are needed).

Gert Pedersen uses this condition to prove a lovely result [GKP1]: A C*-algebra acting on a Hilbert space with all its maximal abelian C*-subalgebras weak-operator closed is itself weak-operator closed. This result tied together a lot

of loose ends and answered some puzzling questions. In particular, it was the key to showing that Kap’s maximal-abelian-subalgebras axiom together with the “normal states” (of [RKA1], or what corresponds to them in the presence of Kap’s assumption), again, characterizes von Neumann algebras (in a space-independent way). Kap’s initial intuition was correct (and useful)—hardly calling for renunciation. Gert’s contribution, certainly one of the cleverest in that area, underscores the irony in his “refuses to die” remark during his lecture. Why did he make it? As Kap notes in the “afterthought” to his density theorem, Gert was witty. I knew Gert well; he was obsessed with being witty, sometimes to the point where the aptness or accuracy of the “wit” was not a matter of great importance. As was often the case with Gert’s “witty” remarks, they contained deep ambiguities (were they being made “positively”—admiringly, or “negatively”—scornfully) and an ample helping of self-deprecation (after all, Pedersen had been just about the cleverest contributor to the AW*-project; diminishing it diminishes him). What to make of Pedersen’s remark? We shall never know—Kap certainly didn’t. I incline to the view that Gert didn’t have more in mind than the “wit”: It was just a blend of the visceral motivations mentioned. There is still much work to be done in this area. Some of it looks difficult to me, and intimately tied to approximation theory.

As remarked before, Segal and Kap were not on good terms at the time of Godement’s review of [IKa4]. There is double and triple irony here. Kap began with fondness and great respect for Segal, probably growing from contact with Segal at the Institute for Advanced Study just post World War II. Segal was very likely the “linch pin” for a small group at the Institute (among them, Warren Ambrose, Richard Arens, and Kap), succeeding in interesting them all in functional analysis and operator group algebras for locally compact groups. It is also very likely that Kap’s urgings initiated the process that ended with Segal joining the faculty at the University of Chicago in 1947–48. I watched a small, sad drama unfold, silently (almost below the surface) during one day (in 1947–48) in the offices and corridors of Eckhart Hall. That day, as I passed and stopped at Segal’s office, I saw Segal finishing a discussion with Kap in which he was describing a long, complicated measure-and-function-theoretic argument for some result he had just proved about “unitary invariants” for operators that (together with their adjoints) generate a “type I von Neumann algebra”. For finite matrices, the “type I” is no restriction. That restriction takes us as close as we can be to the finite-dimensional case and still include infinite-dimensional Hilbert space. One large component of those invariants is a “multiplicity” decomposition of the action of the von Neumann algebra

on the Hilbert space. Segal had been dealing with that in a heavy-handed, analytic manner. It was a “piece of cake” for Kap’s algebraic, von-Neumann-algebra techniques—exactly what Kap was trying to teach us with his introduction of AW^* -algebras. When Segal had finished, Kap blurted out, “But that’s trivial, it can be done in a few lines.” Segal went sallow, sullen, and silent. (I turned ashen.) After a moment, Segal said something to the effect that Kap hadn’t understood him and Kap should try to write those few lines. Kap would, then, see that Segal was right (on the need for a complicated analytic proof). Kap agreed to try. By some coincidence, I happened to be in Kap’s office talking to him that afternoon when Segal passed the door. Segal stopped and asked Kap if he had tried to write his proof. Maybe it was not a coincidence; maybe Segal, realizing that I had “witnessed his humiliation” that morning, thought that this would be a good time to confront Kap. He could claim his “vindication” and “redemption” (in my eyes) at the same time. That vindication was not forthcoming, and the redemption was never needed. Kap took a step over to his desk, which was covered by a twenty-centimeter-high mound of papers, letters, reprints, preprints, books, and whatnot, plunged his hand into the edge of the mound and plucked from it a partially folded slip of paper (about 7 by 12 centimeters in size) on which he had pencilled a few sentences. He went to his door, where Segal was standing, and handed Segal that slip of paper (with no flourishes, gestures, or facial expressions). Segal glanced at, though did not scrutinize, the slip with an expression that seemed to me to be a combination of annoyance and distaste, and walked on. I concluded that, at that exact moment, Kap had acquired a secure position on Segal’s sizeable, pejorative, innuendo-and-defamation register. What a shame; Segal was one of Kap’s heroes, and with good reason. It was Segal who settled the early open question of whether or not the norm-closed, two-sided ideals in a C^* -algebra are stable under the adjoint operation. He proved [Se2] that they were by showing that each norm-closed, one-sided ideal in a C^* -algebra is generated (as a norm-closed, one-sided ideal) by its positive elements. For the proof, Segal created an ingenious (and very pretty) piece of “noncommutative analysis” (perhaps, the first—at least, the first I know of in the subject). This, together with Segal’s seminal work on operator, group algebras and unitary representations of locally compact groups had won Kap’s undying admiration. Why, then, the “blurring” by Kap? It comes down to a simple fact: Kap was “pure of heart”. There was no malice present or intended when Kap said such things. It was never: “*You* are stupid,” or “I am smarter than you.” It was simply: “*That* is stupid,” or “*You* are *being* silly.” Kap was very definitely an equal opportunity “blurter”—he blurted

mathematical corrections and comments at the powerful and well-established as quickly as at the powerless and not-yet established. André Weil was lecturing at a Chicago colloquium one afternoon. At one point, Weil stumbled in his presentation. He said, in effect, that some space was “complete” because, as Weil had noted, it was homeomorphic to a complete space. It was a slipped-mental-cog occurring during the passion of a lecture; it had nothing to do with the thrust of the argument. Those who were following that thrust were too concentrated to even notice, I imagine. I didn’t notice. Kap did notice, and “blurted out,” “That’s silly, completeness is not a topological invariant.” That to Weil, who had invented uniform structures (one of the lesser of his great contributions, but very useful, and interwoven with the notion of completeness)! The audience was stunned, I could hear no sound, not even breathing. About thirty milliseconds after the rest of the audience stopped breathing, Kap joined them, with an expression spreading over his face that said, clearly, “What on Earth have I done?!—I’ve just told the Great Man that he was being silly.” And Weil’s reaction? He glanced briefly at Kap, a small, restrained smile formed (not apologetic, but amused). Of course, Weil had caught and understood everything, instantly, with that glance. Not a word was said, and Weil lectured on. Hearts began to beat again as the audience realized that no lightning bolt was headed for Eckhart Hall.

Weil was always friendly and kind to me. I spoke to him closely enough to know what he was thinking in that situation. He saw that Kap had just realized the level of Kap’s audacity—a pipsqueak Assistant Professor telling the “*capo di tutti capi*” that he is being silly. More than that, Weil knew that Kap was not going to “suffer” for this “incident”—no broken career, arms, legs, anything, because Weil prized intellectual honesty and integrity. It did not trouble Weil that he was “revealed” as (slightly) less than perfect, capable of an occasional slip. Weil did not, as Segal did, feel “humiliated” by Kap’s “blurring”. Weil took Kap’s intentions for what they were: to have the best, most accurate, and most elegant mathematics out there; nothing personal involved.

I’ll make some final comments about Kap’s algebraic program that are directed, primarily, to the research workers in operator-algebra theory and the areas that make serious use of it. Without the fruits of Kap’s program, we would be condemned to dealing with the general von Neumann algebra by using von Neumann’s direct integral reduction theory [JvN3], its fussy, long, measure-theoretic arguments and (often irrelevant) countability restrictions, and its large, messy, clanking machinery. Where representations are concerned (of groups and group algebras) and decomposition into more basic components is

needed, that decomposition is effected by either the von Neumann or Kaplansky techniques.

Except in very restricted circumstances (“type I von Neumann algebras”), decomposition into irreducibles is not usefully available. Such decomposition can be effected [M1, 2] (much to von Neumann’s surprise), but not *uniquely* (*pathologically* non-uniquely, as Mautner showed us in [M1, 2]); it is not the way to go. In the infinite-dimensional (measure-theoretic) environment, decomposition into basic central components (“factors” in our language) is the appropriate goal. That can be effected by either the von Neumann or Kaplansky techniques—and Kaplansky’s techniques are far superior for those purposes. (It was that “divide” that was ultimately the basis of the Kap-Segal rift I described.) There *is* the argument that the factors contain most of the substance of the subject, so we needn’t bother with the “global” von Neumann algebra. I agree, but largely because Kap’s techniques make the passage between global and local relatively easy. If we were doing this with the clanking machinery of direct integral reduction theory, that passage would be a “subject”, with its own special articles and plenty of mistakes (remember Tomita’s non-separable reduction theory [T], which even found its way into an edition of Neumark’s otherwise fine *Normed Rings*, or Mautner’s otherwise interesting [M1] demonstration that direct integral reduction into irreducibles is, at least, possible).

Perhaps a more alarming illustration of the problems in and pitfalls of working with direct-integral, reduction techniques is seen in the following. My “big (mathematical) brother”, good friend, and occasional mentor, George Mackey, convinced himself (sometime in the mid-1960s, I think) that he had proved that each masa in a factor is “simple” (to use technician’s current terminology — though it was introduced 55 years ago by Ambrose and Singer). It took several transatlantic letters (I was in Europe at the time) to convince George that that wasn’t so. I had produced an example (using “free group factors”) in the early 1950s and reconstructed it for my last letter in that exchange. (That example is, now, recorded in [RKa2, pp.359-60].) Let me add that George was, among other things, a master at navigating the currents of treacherous, measure-theoretic seas, but even great captains have lost ships in such seas. George was rarely foolhardy but always intrepid. On this one, unluckily, he chose the roiling waters of direct-integral, reduction theory to carry his argument.

The algebraic structure approach to operator algebras, in general, and von Neumann algebras, in particular, embodied in Kap’s AW*-algebras [IKa4] was another of Kap’s “great gifts to mankind” (to borrow from Gert Pedersen). It is about as sensible to scorn it as it was to scorn Lebesgue’s measure and integration theory.

A paper [IKa5] that does not appear in *Selecta* [IKa1] had a great influence in the development of operator algebras. In that paper, Kap proves that automorphisms of a type I AW*-algebra that leave the center element-wise fixed are inner. In [IKa6], he had proved the result for * automorphisms of type I AW*-algebras. For the purposes of that proof, Kap introduces and develops the basics of the concept of “Hilbert C*-modules” (over commutative C*-algebras). That concept was broadened and expanded significantly by W. Paschke [Pas] and M. Rieffel [R1]. It has come to play an important role in the theory of operator algebras (e.g., it is a key component in Rieffel’s “Morita-equivalence” results [R2] for C*-algebras). An excellent account, with important additions to the Hilbert C*-module theory, is to be found in the beautiful tract [L] of E. C. Lance.

A companion to Kap’s automorphism result in [IKa5] is his proof that each derivation of a type I AW*-algebra (into itself) is inner. To recall, a derivation δ of an algebra \mathfrak{A} (into itself) is a linear mapping (of \mathfrak{A} into \mathfrak{A}) that satisfies the Leibniz rule:

$$\delta(AB) = \delta(A)B + A\delta(B).$$

If $T \in \mathfrak{A}$, then $\delta_T(A) = AT - TA$ defines a derivation δ_T . Such derivations are said to be *inner*. In more modern form, the derivations are “1-cocycles” of \mathfrak{A} into \mathfrak{A} , in Hochschild’s cohomology of associative algebras, and the inner derivations are the cocycles that “cobound”. It is classic that the derivations of the algebra of all linear transformations of a finite-dimensional, unitary space are inner. (That is, the first Hochschild cohomology group vanishes.) Kap’s derivation result in [IKa5] includes the extension of that fact to the algebra of all bounded operators on a Hilbert space. Kap also noticed that he had proved the continuity of the derivation without assuming it—an “automatic” continuity result; he dares to ask (conjecture?), as the closing words in [IKa5]: “is every derivation of a C*-algebra automatically continuous?” A year or two later, Sakai [Sa2] proved this “conjecture” with an ingenious argument. Ten years later, the fact that all derivations of von Neumann algebras are inner was proved ([Sa3] and [RKa4]), after which a torrent of work on derivations (and even the extension to higher Hochschild cohomology groups) flowed through the literature of operator-algebra theory and mathematical physics.

The connection with mathematical physics is quickly explained (though, perhaps, as a surprise to some). The self-adjoint operator algebras provide the most convenient and natural framework for the mathematical model of quantum mechanics toward which Dirac [PDi] and von Neumann [JvN2] were striving. (It found its sharpest early expression in Segal’s “Postulates...” [Se3].) The self-adjoint elements in the C*-algebra correspond

to the (bounded) observables of the quantum mechanical system to be studied. The automorphisms, or rather, a one-parameter group of $*$ automorphisms of the C^* -algebra describe the (quantum) dynamics of that system. The generator of that one-parameter group of $*$ automorphisms is a $*$ derivation. The physical identification views that derivation as “Lie bracketing” observables with the energy (observable) of the system (which in the Heisenberg picture of dynamics as observables evolving in time corresponds to differentiating the “moving observable” with respect to time). So, while the mathematical development of this theory of derivations and automorphisms is of significant mathematical interest and beauty in its own right, its foundational relation to basic quantum physics is so close and important for an understanding of the mathematics of that physics, that its development cannot be left undone.

Another region of Kap’s art is strewn with his glorious “giveaways”. I’m not alluding to those (sometimes wonderful) ideas and projects we pass on to our (equally wonderful) students; but rather, those thoughts, suggestions, conjectures, questions, and other “tidbits” that some of us occasionally contribute to the “body mathematical”. Some of those “giveaways” can be crucially important for the development of mathematics. The view I have heard expressed on occasion, that if there is “nothing in print” those giveaways were “never there”, is a gross distortion of the way mathematics evolves. I’ll illustrate that by describing two of Kap’s contributions. The first refers to [G-N], mentioned earlier. That paper was one of several “characterization” papers appearing in the early 1940s (among them, [K-M1, 2]). That flurry of activity was stimulated by the success of M. H. Stone’s papers [MSt1, 2] containing a characterization of the Boolean algebra of subsets of a set along with applications of that characterization. The Gelfand-Neumark characterization of norm-closed algebras of bounded operators acting on a Hilbert space, that are stable (“closed”) under the adjoint operation was not among the earliest “characterizations” nor did it seem to have more intrinsic interest than any of the other structures being “characterized”. I’m reasonably sure that Gelfand and Neumark were not especially proud of their contribution to the “characterization derby”. Such characterizations should, at the very least, be elegant. Gelfand and Neumark had found it necessary to append two conditions to their characterization that they admit (in a footnote) to feeling may be superfluous. Nonetheless, their ingenuity (individual, and certainly, combined) shines through each paragraph of their article.

As it turned out, [G-N] is one of Gelfand’s most important (arguably, *the* most important)

contributions—which brings us back to Kap’s earlier quote, “the applicability of a piece of mathematics is hard to predict.” They were characterizing just the right construct, one with a multitude of critical connections. But surely they were disappointed by the “inelegance” of their characterization and its two “dangling” conjectures. Those conjectures quickly became the focus of much effort for the small band of us working in what had become the forefront of that sort of noncommutative harmonic analysis and representation theory. In particular, Kap and I were fascinated by those conjectures. We had many conversations about them. The first of those conjectures asserted that $A^*A + I$ is invertible for each A in the Banach algebra \mathfrak{A} (with unit I), the algebra that Gelfand and Neumark were trying to prove is isometrically $*$ isomorphic to a norm-closed, unital algebra of bounded operators acting on a Hilbert space and stable under the adjoint operation on bounded operators. Kap concentrated mostly on this first conjecture; it easily becomes the assertion that the spectrum of A^*A contains no real numbers less than 0. He had found a very clever argument to prove the conjecture, provided one knew that the sum of positive elements (self-adjoint elements with nonnegative real spectrum) is positive. But Kap couldn’t prove this positivity at that time. In 1952, M. Fukamiya [F] proved that positivity, unaware of Kap’s argument (unpublished, though Irv was willing to show it, and had shown it, to any of us who asked). When Kap saw that article, he discovered who was reviewing it (J. Schatz) and sent him his argument to include in the review [Sch] (to complete “Fukamiya’s proof” of that Gelfand-Neumark conjecture). Kaplansky’s argument was the cleverer part of the proof as far as I can see; he could, with full justice, have written a small note citing the Fukamiya article appropriately, but chose to handle it in the almost totally self-effacing manner I’ve described. It had taken ten years to settle that conjecture. This one of Kap’s “giveaways” is just barely published—in someone else’s review!

The second conjecture asserted that A and A^* have the same norm, for each A in \mathfrak{A} . Irv and I had plenty of fun discussing it. He was fairly convinced that it wasn’t true. I felt that it was true, but with less conviction than Irv had about the contrary position. When Irv wrote to me about finding the Fukamiya article, I looked, again, at the second conjecture (the evening of the day in which Irv’s letter arrived). I managed to prove that $\|A\| = \|A^*\|$ when A is an invertible element in \mathfrak{A} (and that, in consequence, $A \rightarrow A^*$ is norm continuous). I wrote to Kap relaying that information and asked him if he felt that $*$ need not be isometric as strongly as he had. He allowed that his conviction was shaken. Seven years later, Jim Glimm and I proved [G-K] that $\|A\| = \|A^*\|$ for all A in \mathfrak{A} . So,

Gelfand and Neumark were quite right in their feelings, as proven ten and seventeen years later. (See [RKa5] for a full account, with arguments, of the Gelfand-Neumark theorem.)

Another, and final, sample of Kap's "giveaway" activities (though, by no means, the last one available) is a strong illustration of the fact that so much that is important in the development of mathematical ideas occurs (well) below the publication horizon. This sample begins, again, in Kap's office in 1949. Kap had received a reprint from the Soviet Union describing work in the then-nascent, complex-variable-Banach algebra development in functional analysis. It involved constructing an idempotent, different from 0 and the unit, in a commutative Banach algebra. Again, Kap fished it out of the mound of sundries covering his desk with a simple, nonstop motion of his arm. He asked me what I thought about the possibility of the same fact holding in a simple, unital C^* -algebra. That is, must there be a nontrivial idempotent in such an algebra? I thought about it for a minute or two and, responded a little too forcefully, "No! Why should there be?" Kap thought that it actually might hold. That was the algebraist in Kap. He loved idempotents (and infected me with that love). On the other hand, my functional analysis experience had taught me (even in those tender years) that there were many escape routes for idempotents on the cold and forbidding terrain of infinite-dimensional, functional analysis. I was sure that there were simple C^* -algebras with unit and no proper idempotents, and said so to Irv. "Of course there are," I told Kap—and proceeded to miss the big point! I added, "It may not be easy to find a counterexample, and so what, when you do? You then have an isolated counterexample, and what can you do with it?" I commented that I would wait for someone else to find it (Bruce Blackadar did [B] 32 years later), but faithfully passed Irv's question on to others—always making it clear that I felt there was an example (without non-trivial projections) and Irv thought that such idempotents might always be present.

What do I mean when I say, "I missed the big point" with my "so what" comment? Simply that the question Kap had posed was more important than I was allowing (much more!) or than Kap was insisting. More than that, both Kap and I were well aware of all the connections and basic techniques needed to conclude that the question was very important. We just didn't take the time to put it together and draw that conclusion. To begin with, at that point (1949), it was clear to us that the study of C^* -algebras was not just an investigation of an interesting class of infinite-dimensional, semi-simple algebras; it was the study of the natural framework for all of (real) classical analysis, the commutative case, and

thence all of noncommutative (quantum) real analysis through the noncommutative C^* -algebras. We had all learned from Marshall Stone the intimate relation between an (infinite) algebraic structure and a topological space—both of us from Stone's ground-breaking Boolean algebra papers [MSt1, 2] published in the mid-1930s, and I, from a splendid, year-long course, as well, given by Stone the preceding year. Stone's methods for "pulling" points of an associated (topological) space from a given algebraic structure and topologizing that space with the then-developing techniques of functional analysis in the commutative case, was well understood by us. We were also explicitly aware of the paradigm of a general C^* -algebra as the "function algebra" of a *non-commutative* topological space—and spoke of such algebras in those terms when that was useful. We knew, too, that that topological space was uniquely determined (up to homeomorphism, in the commutative case) by the algebraic structure, and that it was best to let the noncommutative space remain "virtual", dealing with it through the algebraic structure, just as it is most often best to let the "eigenvector" corresponding to a general point in the spectrum of a self-adjoint operator remain "virtual" rather than make it explicit with δ functions, approximating vectors, and such. Had Kap and I applied all that knowledge and technique to his idempotent question, we would have concluded, quickly, that we were asking about the existence of compact (because of the *unit*), totally noncommutative (because of the *simplicity*), connected spaces (because of the absence of *proper idempotents*). So, in these terms, I was asserting the existence of compact, (totally) noncommutative, connected topological spaces and Kap, with a lot less conviction (possibly, just for the "sporting" aspect of defending the other position) was asserting that there are no such spaces. On top of that, I was adding the question, "Who cares?" The clear answer to that should be, "Everyone!"—at least, every "hard" theoretician (chemist, physicist, and mathematician—and as we are discovering daily, each biologist as well), certainly everyone for whom quantum mechanical considerations play a role. Had Kap and I spent the extra ten minutes it would have taken thinking about the meaning of the question he had asked, we would both have been alert to its importance and probably "sure" of and agreed on its (affirmative) answer.

As far as I could see, and Kap and I were close enough so that I tended to know what occupied him when he was doing functional analysis, Kap did not think further about the idempotent (projection) question, nor did I, in the years immediately following our conversation. However, all that is just the beginning of the story. Somewhere in the mid-1950s, I thought of ("stumbled on" might be more accurate) an example of a "primitive" C^* -algebra

(one with a faithful, irreducible, representation on some Hilbert space) with no nontrivial projection. It was not particularly elegant or interesting, so I left it as unpublished notes (probably unfindable at this point). No great loss; in the very early 1960s, Jim Glimm found a nice example of such a primitive C^* -algebra (independently—compare [Gr] as well). But those are far from the “simple” target we aimed for. During the academic year 1965–66, I visited Aarhus University in Denmark. At home one evening, doing what seemed to be quite unrelated work, I looked up from the page on which I was writing and realized, with what must be described as a certainty little short of absolute, that the perfect example of what Kap and I were seeking, were the C^* -algebras, $\mathcal{A}_{\mathcal{F}_n}$, arising from the free (non-abelian) groups \mathcal{F}_n on n -generators ($n \geq 2$). Let L_g be the unitary operator on $l_2(\mathcal{F}_n)$ induced by left translation by g of functions in $l_2(\mathcal{F}_n)$, $\mathcal{A}_{\mathcal{F}_n}$ be the algebra of finite, complex, linear combinations of these L_g , $\mathcal{A}_{\mathcal{F}_n}$ be the norm closure of $\mathcal{A}_{\mathcal{F}_n}$, and $\mathcal{L}_{\mathcal{F}_n}$ be the closure of $\mathcal{A}_{\mathcal{F}_n}$ relative to the topology of convergence on vectors in $l_2(\mathcal{F}_n)$, the strong-operator topology. Then $\mathcal{A}_{\mathcal{F}_n}$ is the (left) complex group algebra of \mathcal{F}_n , $\mathcal{A}_{\mathcal{F}_n}$ is the (reduced, left) C^* -group algebra of \mathcal{F}_n , and $\mathcal{L}_{\mathcal{F}_n}$ is the left von Neumann group algebra of \mathcal{F}_n . My “epiphany” revealed to me that each $\mathcal{A}_{\mathcal{F}_n}$ is simple with no proper projections. All I needed was a proof! As noted, that thought occurred to me while I was involved in other research—deeply involved. So, I didn’t try to work on that idea. Several years went by before I mentioned it to anyone. (I suppose I thought that I would get a chance to think, seriously, about it; but I never did.)

At the end of the 1960s (I’m not absolutely sure of the timing), while returning from lunch with the large, active, group of functional analysts and visitors at the University of Pennsylvania at that time, Robert Powers and I were walking side-by-side. He started to tell me of some work he was doing with what is now known as an “irrational rotation C^* -algebra” (a “noncommutative torus”). I knew what was coming because a very good friend of, and frequent visitor to, our department, Daniel Kastler, had told me that Bob was searching for a projectionless, simple C^* -algebra with these “torii”. As Daniel so charmingly (as always) put it, “He is looking for a medal for the other side of his jacket” (an allusion to the spectacular breakthrough Powers had made, with factors of type III [RPo1], a few years before, and what has become known as “the Powers factors”). During that walk, Bob noted that, while he was expecting to show that there were no nontrivial projections in such algebras, he was quite surprised to construct many such projections. Marc Rieffel went on from the Powers computation to construct what seemed to be the full “ K_0 -theoretic” structure of the “torii”

[R3], though he could not establish that he had it all.

When Powers finished telling me about his surprising families of projections, I decided to tell him about my free-group examples. I asked, “Do you want to know a family of simple C^* -algebras with no proper projections?” and announced that I had such a family. That evinced a good deal of sputtering and confusion. Bob took that announcement in the natural way, but with considerable shock. He concluded that I had answered the Kaplansky question but that he didn’t know of it or hadn’t heard of it, for whatever reason that he didn’t understand. Well, I’d had my fun and now, dispelled the confusion at once. “Oh, I have the examples,” I said, “I am just missing the proofs. That is where you come in, if I show you the examples,” I added. Neither Bob nor I would worry about such an arrangement being dealt with fairly. Over the next several months, Powers established that the examples were simple [RPo2] and made extensive calculations of the spectral properties of operators in $\mathcal{A}_{\mathcal{F}_n}$ (not easy work). If the spectrum of such an operator is disconnected, integrating around a connected component of that spectrum, using the Banach-algebra-valued, holomorphic-function calculus produces a nontrivial idempotent in the algebra (and now, we are back to the starting point: Kaplansky, the reprint he had received, and the basis for the question he asked). At any rate, after those few months, Bob had worked so hard and been lost in so many calculation jungles that the mere mention of the problem turned him white as a sheet. I had “assured” him that those examples were what we wanted; he needn’t waste time thinking otherwise. My “arrogant” assurance was partly humor, but mostly genuine conviction. Those examples and that project became widely known. Kaplansky’s question had given birth to my conjecture about the free-group C^* -algebras. Stalwarts other than Bob Powers tried the spectral calculation approach (among them Uffe Haagerup), but without success. Nonetheless, spectral properties of operators in the free-group C^* -algebras has become a topic in its own right (not an easy one), with much information emerging that is vital for other purposes.

As noted, Bruce Blackadar settled the Kaplansky question [B] in a paper appearing in 1981. In 1981, M. Pimsner and D. Voiculescu [P-V1] proved my conjecture by constructing a six-term, cyclic, exact sequence from which they could compute the K -groups of $\mathcal{A}_{\mathcal{F}_n}$ (and even more general C^* -algebras). In 1979, Joel Cohen had shown that the “full” C^* -algebra of \mathcal{F}_n [JCo] (see also [Ch]) has no proper projections. Years earlier, Joel Cohen, a very bright young topologist, had been a postdoc in our department at the University of Pennsylvania. The question of idempotents in the complex, group algebra of \mathcal{F}_n had come up in his work

on topology. He asked me about that and I was able to prove (in fairly short order) that they had no proper projections. (Of course—when we now know that there are no such projections in the larger $\mathfrak{A}_{\mathcal{F}_n}$. But that was still many years from being proved.) Joel was delighted with that. He trained himself to the point where he became a very serious worker on my conjecture, as [JCo] indicates. It is worth noting how much easier what I proved for Joel was than the eventual proof of my conjecture in [P-V1] as an illustration of how seemingly small changes alter the analytical difficulties (passing to the norm closure in this case). A further (stunning) illustration of that, for those who haven't struggled with such questions, is provided by examining $\mathcal{L}_{\mathcal{F}_n}$, the *strong-operator* closure of $\mathfrak{A}_{\mathcal{F}_n}$, a von Neumann algebra. It is filled with proper projections (so many, that finite, linear combinations of them lie norm dense in that algebra). Why that should happen is quickly explained in terms of the paradigm of $\mathfrak{A}_{\mathcal{F}_n}$ as a noncommutative (continuous), function algebra associated with a compact, noncommutative topological space (“connected”, as it turns out), while $\mathcal{L}_{\mathcal{F}_n}$ is the noncommutative (measurable) function algebra associated with a noncommutative measure space, and of course, measure algebras are stuffed with characteristic functions of sets (noncommutative, measurable sets, in this case). These characteristic functions correspond to idempotents in the von Neumann algebra.

Pimsner and Voiculescu [P-V2, 3] also completed Rieffel's program [R3] of determining the K-theory of the noncommutative “torii”, proving that Rieffel had found all the projections. Their proof of my conjecture was considered the first real success of noncommutative K-theory. At the time of that proof, J. Cuntz [JCu1, 2] carried Joel Cohen's work on the full C*-group algebra of \mathcal{F}_n further, determining its \mathcal{K}_1 -group as well. Later, Cuntz found an easier (though highly nontrivial) proof of my conjecture. (It is the way of this area of mathematics that a very long and difficult argument, sooner or later, gets “compactified” into a few-page proof so incredibly clever that no mortal would ever produce it on the first go-around!) A. Connes was able to use his beautiful results to “manufacture” connected, noncommutative spaces “geometrically” [Con]—a very satisfying conclusion to that part of the project.

Well, there is much more to say about this development. It is, finally, the first serious noncommutative algebraic topology. I have only begun to list the great contributions made to that subject and to mention the superb mathematicians who made those contributions. At the base is Irving Kaplansky, whose algebraic curiosity forced him to wonder if there had to be idempotents in a simple, unital C*-algebra.

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About the Cover

JPEG Image Compression

This month's cover was produced entirely by David Austin, who also wrote the article on JPEG in this issue. He tells us:

The cover illustrates some aspects of the JPEG compression algorithms. In the background is a detail of the photograph blown up so that individual pixels become visible. The JPEG algorithm groups these pixels into 8 by 8 blocks, one of which is highlighted to the left of the photograph. Moving to the lower right, we see the luminance values for the pixels in another 8 by 8 block, the quantized Discrete Cosine Transform (DCT) coefficients representing the luminance values, and the zigzag order in which these coefficients are recorded. Finally, in the bottom center is a sequence of numbers—the ordered DCT coefficients for the luminance and blue and red chrominance values—describing the block just to the left and the reconstruction of the block from that sequence.

In the lower left is a representation of the wavelet coefficients that result when the JPEG 2000 algorithm, in which the Discrete Cosine Transform is replaced by a Discrete Wavelet Transform, is applied to this 16 by 16 block. The coefficients in the upper left corner of this block give a lower resolution version of the larger block. The blocks in the other three corners contain the information needed to reconstruct the full 16 by 16 block from the lower resolution.

—Bill Casselman, Graphics Editor
(notices-covers@ams.org)

