Reflections on a career in Mathematics

Masamichi Takesaki
Professor of Mathematics, Emeritus,
Department of Mathematics,
UCLA
Los Angeles, California, USA

My Personal History

As erroneous information about my career has been posted on the internet, I will begin by correcting the record.

- 1956, Spring: Graduated from Mathematical Institute, Tohoku University, earning a Bachelor's Degree.

- 1956-1958: Master's Degree Student at Mathematical Institute, Tohoku University, earning a Master's Degree in Mathematics.

- 1958, April through June: Ph D Student at Mathematical Institute, Tohoku University, withdrawing from the Ph D course in order to take a position at the Tokyo Institute Technology.

- 1958, July through 1963, June: Research Assistant at Department of Mathematics, Tokyo Institute of Technology.

- 1963, July through 1970 June 30: Associate Professor of Mathematics, Mathematical Institute, Tohoku University.

- 1965, May: PhD of Mathematics granted from Tohoku University.

- 1968 September 1- 1969, June 30: Visiting Associate Professor, Department of Mathematics, University of Pennsylvania.

• 1969, July 1 - 1970, to June 30: Visiting Associate Professor, Department of Mathematics, UCLA. Leave of Absence from Tohoku University for 1968 - 1970.

• 1970, July 1 through 2004, June 30: Professor of Mathematics, Department of Mathematics, UCLA.

• 2004 July – present: Professor of Mathematics, Emeritus, Department of Mathematics, UCLA.

• Various Visiting Positions throughout my career at UCLA.

Introduction

I would like to talk about my experiences as an operator algebraist and make some observations based on those experiences. The period 1956 through 1958, when I was working toward my Master's degree, marked a quiet but very significant decision in the US science program, the importance of which was not widely recognized in Japan. During this period, with the success of Sputnik, the US space program was seen to be behind that of the Soviet Union. To overcome this deficit, the US made the decision to strengthen its mathematics capabilities by training 1000 new Mathematics PhDs per year. The Japanese mathematical community did not appreciate the effects this decision would have. Indeed, I recall Professor Tannaka (who told me of the decision on his return to Japan from Princeton in 1958) asking

"How can one provide jobs for so many new PhDs?"

Perhaps surprisingly, US society not only absorbed these new PhD's year after year but also supported the careers of many established mathematicians from all over the world. In particular many first class Japanese mathematicians moved to the US in the 1950s and 1960s, and many talented Japanese students have pursued graduate studies and built careers in the US. This pattern continues today.

This policy change had an immediate effect on research in operator algebras. The 1960's opened with Glimm's ground-breaking works on UHF algebras and on type I C*-algebras. It was R. Kadison who trained Glimm in the period of low activities in operator algebras. Several other powerful young operator algebraists including Ed Effros, Marc Rieffel and Ron Douglas, made significant contributions shortly afterwards. The Japanese operator algebraists (not too many though Shoichiro Sakai, Jun Tomiyama, Teishiro Saito and myself) were almost overwhelmed by this spate of new ideas and techniques. Sakai decided to join the American school of operator algebraists in 1962: first two years at Yale University and then at the University of Pennsylvania. Pennsylvania of course became a haven for the area with Kadison, Effros, Fell, Griffin, Shale and then Powers all working there.

The policy of training 1000 new mathematics PhD's annually and their absorption into the job market made the US very much a mathematics-oriented society. Arguably, it helped lay the "computer age" and the decisive role the US has played in its development. Early days as an aspiring operator algebraist I started to learn functional analysis through a fourth-year undergraduate seminar under the supervision of Professor Misonou (who was then a research assistant). Toward the end of 1955, I ran into John von Neumann's famous paper on the second commutant theorem. I was also taking an undergraduate seminar course on abstract algebra (using van der Waerden as text-book), and was exhilarated by the beautiful interplay between analysis, topology and algebra which von Neumann's paper demonstrated. So I kept studying operator algebras as a graduate student. I was also influenced by Professor Sakai's success in characterizing Von Neumann algebras as duals and by Richard Kadison's paper on the transitivity of an irreducible C*-algebra. At that time the Mathematics Department of Tohoku University had a strong operator algebra research group consisting of Turumaru (Associate Professor), Misonou (Research Assistant), Sakai(Research Assistant), Suzuki(Research Assistant), Tomiyama (Master's Degree Graduate Student, one year ahead of me), and Saito (Master's Degree Graduate Student, also one year ahead of me) and myself. However, the group's supervisor, Professor Fukamiya, was a general functional analyst rather than an operator algebraist, and the disparity between his and the group's interests generated a degree of tension. This tension peaked immediately after Saito had successfully presented his Master's thesis when Professor Fukamiya declared to Saito that he planned to reset the direction of the research group by firing the above group of researchers. Saito, Tomiyama and myself were in a panic. But fortunately the department of mathematics of Tohoku University had a long tradition of liberalism and I was recognized as a student movement leader during my undergraduate study. After long discussions among the three of us, Tomiyama undertook to respond to Fukamiya's plan. Two days later he went to his office declaring:

"As long as the graduate school admits me I will continue my graduate study by myself."

His unusually blunt statement forced Fukamiya to rethink his resetting plan. Namely, if he would pushed his plan as he mentioned, it could spark the strong opposition from the graduate students with Takesaki's leadership. He recognized that his path to restructuring the research direction would be far from smooth so he quietly stepped away from it and accepted the operator algebra group of Tohoku University.

My path to an operator algebraist.

After realizing that my supervisor disliked operator algebra, I decided to hide my desire to be an operator algebraist throughout my master degree years by presenting the newest functional analysis paper at the training seminar avoiding carefully operator algebra papers until the master degree thesis presentation at the end of two year training. There was no way to hide my research area at the MA thesis presentation, operator algebras, at the thesis presentation. My master thesis was on the conjugate space of operator algebras, which was published after the thesis presentation. For my boss, it was a bit of surprise that I had publishable results in the field of operator algebra as I didn't tell what I was doing before the presentation of my Masters Degree Thesis in 1958 February. In May, 1958, I presented my Master thesis at the Annual Spring Meeting of Math.Soc. of Japan which caught the attention of Professor Umegaki. After the meeting, I was told from my Boss if I'm interested in a research assistantship under Professor Umegaki at the Math. Department, Tokyo Institute of Technology. So I was able to begin my mathematical carrier smoothly as opposed to the drama of Saito or Tomiyama a year before.

Unpopularity of Operator Algebras

As mentioned above, my path to becoming an operator algebraist was far from smooth. However, it seems that many operator algebra specialists have experienced similar difficulties. For example, Ed Effros's supervisor at Harvard, G.W. Mackey, didn't like operator algebras and Effros had to learn and push his research in operator algebras by himself with little support or encouragement from the supervisor. Alain Connes' supervisor Dixmier did not encourage him to enter the field of operator algebras; Dixmier was highly respected among operator algebraists, but as the field was unpopular in France, he was concerned that Alain might create obstacles for himself if he chose to work in it. Our dear friend Sir Vaughan F. R. Jones's supervisor André Haefliger wasn't an operator algebraist, but a well known geometer. And the organizer of this seminar, Professor Kawahigashi, had to cross the Pacific Ocean to UCLA when he wanted to work on operator algebras as Tokyo University did not have a an operator algebra specialist when he was a student.

What makes operator algebras unpopular?

Quite simply the field of operator algebras is difficult and the prerequisites can overwhelm beginners. I recall a very able student at UCLA who, after completing her qualifying exams, approached me to ask that I be her PhD supervisor. I gave her a copy of my UCLA lecture notes to read through. She returned a few weeks later saying that the prerequisites were too extensive and that she had anticipated that the material she had studied for the qualifying exams would be sufficient background for beginning her research. She subsequently chose to work in classical analysis and has since become a prominent mathematician in that area. Perhaps unfortunately, the prerequisites keep growing; when I was a student Dixmier's 367-page book was a sufficient background to begin research while today something like my 3 volume 1481 page book is needed. And the problem will only increase as the field continues to grow and interact with ever-widening areas of mathematics and physics. This is not a unique problem as the similar problem overwhelmed algebraic geometers in the 60th and 70th on Gronthendiek's work. The hard fact is that there is no easy entry-path for beginners. The main issues for operator algebraists are to understand what governs the mathematical world in infinite dimensions and in the presence of non-commutativity. Neither issue was present when modern mathematics started in the late 19th century. It was John von Neumann who observed that the development of quantum physics would force us to create mathematics capable of accommodating both non-commutativity and infinite-dimensionality. Neither phenomenon is clearly visible in our every-day world, so there is no familiar picture to show beginners, and we must rely instead on understanding and building upon the knowledge accumulated by the pioneers of the past. Another characteristic of the area is that quite often a problem in operator algebras touches upon the foundations of mathematics; I have observed that quite a few logicians are looking at our achievements and/or problems to motivate their own work.

Truly surprising breakthroughs are unpredictable and carried out by young individuals.

I would like to make note of a few results which I regard as truly remarkable and surprising in the history of operator algebras. The first (for me) is the transitivity theorem for irreducible C*-algebras published by Richard Kadison in 1957. This was a totally unexpected and unanticipated result (as noted in Jack Feldman's review). Similarly, James Glimm's 1959 results on type I C* algebras were quite unexpected, and I do not believe that anybody but Glimm himself (and possibly Kadison) could have predicted results of this type beforehand. Sakai's characterization of W*-algebras was also a big surprise outside of Japan. Even though I was an undergraduate at the time and hence did not experience the surprise directly, I was aware that Sakai's success generated considerable excitement and celebration among the operator algebra group at the Mathematical Institute of Tohoku University. I was however directly involved in my next example, the development of what is now known as Tomita-Takesaki theory. I spent the 1968-1969 academic year as a visitor at the Department of Mathematics at the University of Pennsylvania. When I asked my host Professor Kadison what my duties as a visitor were, his reply was

"There are no duties; spend a good year here".

My reaction was

"Wow, what should I do?"

I felt under considerable pressure to repay his generosity by achieving something of significance. I had realized when I arrived in Philadelphia that nobody in the operator algebra group there accepted the main claims that Tomita had put forward in the preprint he had circulated at the 1967 Baton Rouge conference. Professors Hugenholtz and Winnink had asked me at that conference about Tomita's claims; on returning to Sendai I had checked the manuscript carefully and found that it contained numerous errors, but I was able, with some effort, to reach the same conclusions as Tomita. When I reported this to Professors Hugenholtz and Dixmier, both noted that Tomita's claims, if confirmed, were very important. Now at the University of Pennsylvania, my assessment of Tomita's claims and my colleagues' conclusions were in sharp conflict. Ed. Effros suggested that I write up detailed notes on Tomita's claim in order to resolve the conflict. I followed his suggestion and wrote up my own detailed account of it by December 1968. I treated the project as my own research, and in addition to providing a sound basis for Tomita's conclusions, I added some observations beyond Tomita's original preprint. When I handed my notes to my Pennsylvania colleagues, they were excited and proposed having a seminar at the beginning of 1969 to work through the details. I was asked to deliver an introduction, with subsequent talks being delivered by other seminar participants. By early spring 1969, the group had collectively confirmed the validity of Tomita's claims. There is a lesson to be learned from this;

breakthroughs may remain unrecognized
and
unappreciated if they are not presented carefully and accurately.

Now I would like to talk about the discoveries of our heroes, Alain Connes and Vaughan Jones. I don't need to talk about the importance of the achievements of these giants. Rather, I want to stress just how unpredictable those achievements were.

Who could have predicted Alain Connes' achievements in his thesis?

Did his PhD supervisor Professor Dixmier foresee them?
(An unusual aspect of this thesis is that Professor Dixmier had noted the exceptional depth of Alain's work in 1972 and suggested deferring the PhD for another year).

Who could have predicted the flow of weights on von Neumann algebras of type III,

created by Alain (jointly with myself) in 1974 in his first major post-PhD project?

Who could possibly have predicted his breathtaking drive on injective von Neumann algebras during his stay in Kingston in the 1974-1975 academic year?

Vaughan Jones' achievements have been similarly unexpected.

Who could possibly have predicted his theory of subfactors in 1981 or his knot invariants in 1984?

Actually, Vaughan's' polynomial invariant has an interesting side story. The Mathematical Sciences Research Institute (MSRI) at

Berkeley had, after careful consideration, organized two workshops for the 1984-1985 academic year, one on operator algebras and one on low-dimensional topology. These areas had been chosen because both were quite active at the time and believed to have potential for significant further development, and because between them they would cover a wide spectrum of mathematics. In May 1984 Vaughan told me of his discovery of a new knot invariant through subfactor theory. This was not a surprise to me since I had been kept informed about his research, but I believed that it would be a sensational discovery for the rest of the mathematical community. So I telephoned the Deputy Director of MSRI Professor Calvin Moor and told him of Vaughan's discovery. Cal was lost for words, and the MSRI program had serendipitously found a unifying theme. I think my point is made;

the truly innovative discoveries are unpredictable,
and very often carried out by young researchers.

Unfortunately, these young researchers do not always have the support they need since the research funding agencies often prefer projects with predictable outcomes from established researchers. We need to ensure that appropriate support is made available to the talented and committed young researchers who will drive the further development of mathematics.

Outside of Mathematics

I think that similar patterns can be seen in areas outside of Mathematics. Consider, as an example, industrial development in Japan since WWII. Nowadays most such development is driven by established companies working on very large projects with extensive government support. But there was a brief period immediately after the war when this was not the case. Instead, growth and recovery was being driven by gifted and dedicated young engineers working in relatively small companies such as Sony, Matsushita, Honda and Toyota. It was only after these companies had begun to enjoy some success that they began to attract government support, resulting (eventually) in the industrial giants we see today. Capitalism in Japan was encouraged by government leadership following the Meiji restoration and continues to rely, to a considerable degree, on government support. And today's government, and the banks, tend to see large and established companies as being the more secure investments leading to a situation which potentially curtails the growth of small businesses and suppresses innovation. Looking internationally, it is worth noting that industrial giants of the past such as General Motors and General Electric no longer dominate the economy; they have been surpassed by companies such as Apple and Microsoft, and the tech companies. Most of these new giants have only emerged since the 1970s, and have been lead by gifted young entrepreneurs from outside the commercial establishment which existed at the time. My conclusion is that truly groundbreaking innovations, whether they be in mathematics or industry, or indeed in a vast range of human activity, originate primarily from the drive and creativity of talented and usually young individuals. So let us try to ensure that such people have the support and encouragement they need.

Thank you for your patience in listening to my talk.

Masamichi Takesaki

August 26th, 2021