Interview with Huzihiro Araki: Mathematics and Physics, A Tale of Two Cultures

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Huzihiro Araki

Huzihiro Araki made pioneering and fundamental contributions to axiomatic quantum field theory, statistical mechanics and the structure of von Neumann and C* algebras.

After obtaining a postgraduate diploma from Hideki Yukawa, he arrived in Princeton University in 1957 during what could be considered as the formative years of the development of axiomatic quantum field theory and statistical mechanics using the operator algebra approach. During his short period of study in Princeton, he made fundamental contributions to a wide range of areas in theoretical physics, even before he was formally awarded a PhD in theoretical physics in 1960 (the first Japanese to have been so awarded in the United States). He was also awarded the Doctor of Science by Kyoto University in 1961.

After a short sojourn in Europe and United States, he returned to Japan in 1964, having been recruited by Y Akizuki to join the then newly established Research Institute for Mathematical Sciences (RIMS) of Kyoto University. He became full professor in 1966 and was Director of RIMS from 1993 until his retirement in 1996. He continues to contribute his expertise and experience as professor emeritus at RIMS and professor in the Faculty of Science and Technology of Tokyo University of Science.

His extensive work in physics include deep results in local quantum physics, scattering theory, relative entropy in quantum statistical mechanics, variational principles on quantum lattice models, theory of algebras of local observables, KMS states and uncertainty of quantum measurement. Though his interest in operator algebras was initially sparked by quantum physics, his work (with E J Woods) on ITPFI (infinite tensor product of finite type I) factors had an influence on the classification of von Neumann algebras and could be considered a precursor of the fundamental work of Alain Connes (Fields Medal 1982). In recognition of his far-reaching influence on mathematical physics, Araki was awarded the Henri Poincaré Prize (together with E H Lieb and O Schramm) by the International Association of Mathematical Physics in 2003. His mathematical legacy is evident in the school of operator algebras that is flourishing in Japan today.

Araki's work generated more than 150 single and jointly written research papers and he wrote *Mathematical Theory of Quantum Fields* (1999). He is the founder of *Reviews in Mathematical Physics*. He was on the Advisory Board of *Communications in Mathematical Physics* when it appeared in 1965 and has been on its editorial board since 1973. He also serves on the editorial boards of the journals *Letters in Mathematical Physics, Reports on Mathematical Physics, Nuovo Cimento B, Journal of Mathematical Physics, Open System & Information Dynamics* and of the series Springer Lecture Notes in Physics and the Birkhäuser Monographs in Mathematics.

He is known for his boundless energy and capacity for scientific organisation. He was Vice-Chairman of Kyoto

University's Committee for International Exchange and on the board of the Yukawa Foundation. He was instrumental in the founding of the International Association of Mathematical Physics, of which he was the first president. He was one of the representatives of the International Mathematical Union, and was primarily responsible for the organising and holding of the International Congress of Mathematicians held in Kyoto in 1990.

In addition to promoting mathematical sciences in Japan, he is untiring in his efforts in promoting international cooperation and understanding among mathematical scientists. He was programme coordinator of the Institute's programme "Mathematical Horizons for Quantum Physics" (July 28 – September 21, 2008) jointly organised with the Centre for Quantum Technologies of the National University of Singapore. He also gave a joint colloquium talk on "Points of Contact between Mathematics and Physics". During his visit to NUS, *Imprints* had the opportunity to interview him on September 3, 2008. The following is an edited transcript of this interview in which his recollections of his younger days and early years of research give us a sense of excitement of the vicissitudes of discovery and even missed opportunities. It also offers us an insight into the fruitful interaction between two apparently incompatible disciplines, mathematics and physics, by one whose heart is in mathematics and passion is in theoretical physics.

Imprints: You published a paper on atomic spectroscopy with your physicist father [Gentaro Araki] when you were an undergraduate. Did it ever occur to you to pursue your career as an experimentalist?

Huzihiro Araki: No. From my younger days, I thought the only profession for which I will be good at is mathematics or theoretical physics. I'm not very good [at other things]. When I was young and went with my parents to buy something, I was very much afraid to talk with somebody. I am not good in communicating with others or negotiating something. So it would not be good for me to work in companies. Among academic subjects, I was not very good in humanistic subjects [humanities]. This was also due to the fact that there were no books except [books] on physics in the house. I already looked at some books on quantum mechanics when I was in school. I didn't look at books on other subjects. Science is okay. I am not very good with my hands or anything like that. My father is very good in working with his hands; he makes things by cutting anything. Sometimes my father wanted me to help, but then I made mistakes; sometimes I broke something. So I thought from that point on … later I also had similar experiences. For example, in university, in the first two years we had to do various subjects — in chemistry, for example. In analytical chemistry, you had to find out, given a solution in a test tube, what was inside the solution. But if I do it, then this becomes black. Also, in physics, I had to do three different experiments. I had chosen to build an electric computer, not electronic, using resistances. If you have a diode, you can do addition and some multiplication. When I built it, it didn't work very well. So I set up all these resistances. You had to connect some different parts together. I was not very good at it, and when I measured it there was a lot of resistance here and there where I made, and there should be no resistance. So I usually computed and certainly you can get the right answer. I could find out how I got the wrong answer. However, I was always good in writing reports. So that was my report. The theoretical part is okay, not the experimental part. So I wouldn't do any experiments.

I: Was your father an experimentalist?

A: No, he was a theoretical physicist. Up to my fifth grade in primary school, my father was appointed in the University of Tokyo until he moved to Kyoto. I was born in Tokyo. In Japan, before and during the war, there were only two planetariums — one was in Tokyo some distance from my house. I liked this planetarium and went there regularly during my second or third year in primary school. My father bought me at least two books; one was about the sun and the other about astronomy. One thing I remember about this book is that my father said that the explanation about relativity theory in this astronomy book was incorrect. I liked astronomy.

I: Were the books in Japanese?

A: Yes, everything was in Japanese. I didn't even know the English alphabet until I went to junior high school. You see, this was during the war, so English was, of course, out. The only thing I knew about English in primary school was "C" and "P". I knew they were pronounced "see" and "pee". I never heard about "a, b, c". The reason I knew "C" and "P" was that they were used for combinations and permutations in a science book for children. Also, in Japan we had to remember the Chinese characters [Kanji] and write them. I'm not very good at it. Sometimes students had to present some writing. I couldn't do it because I didn't know what to write. But in arithmetic I was very good, especially in computation. There are three types of exercises — one is to just compute without doing something. For this I was the fastest in the class. The other type, you write and compute — I was fast but not the fastest. The third type, you had to use the abacus and I was not very good at that — you had to use your hands. So experiments were out of the question. I was also not very good in painting or music. For painting, only once my art teacher praised one of my paintings very much. At first, I didn't know why. We were free to paint anything. That was in Kyoto, in my sixth grade in primary school, or maybe junior high school. Near my house, there was some nice house with some trees which were not really Japanese. I wanted to paint it but it was very rough. After many times, it became a confusion [of colours]. The paint was not very good. I had to paint many times with different colours together. I like exact things like in mathematics, but it was not possible [in art]. Later, I understood that the teacher said that the trees I painted didn't have any branches or anything like that, but it was very much like the painting of a famous Impressionist painter. But I didn't know anything about Impressionist painting.

I: Did Yukawa have any influence on your choice of research area in your graduate studies?

A: I knew [Hideki] Yukawa [(1907–1981), Nobel Prize in Physics 1949] when I was in sixth grade of primary school. I have many stories to say. For example, he gave a talk on Dirac's theory. (He had written two books [on quantum mechanics], the advanced one has Dirac's theory.) He started to explain how to compute energy levels of hydrogen by using Dirac's equation instead of the ordinary energy potential for the relativistic equation. This was, of course, written in his book, but he was stuck in the middle. You had to use hypergeometric functions. I knew hypergeometric functions and so I just said you do this and do that to find the formula. So he was not extremely good in mathematics. At that moment he must have probably forgotten about it. But long, long time later, at some popular meeting he talked about it and remembered that class. I also met [Shin-itiro] Tomonaga [(1906–1979), Nobel Prize in Physics 1965] in Tokyo. Tomonaga and Yukawa were completely different in character. Yukawa didn't tell graduate students what to do. In a discussion, when he heard somebody do something, he would want to say some opinion and the opinion would not be about computations but would be more conceptual, like Dirac. He said that he didn't like representations because it was mathematical.

I went to the United States on a Fulbright grant (it only provided travel expenses) and a Hayes grant which provided living expenses. I went to Princeton in '57 after two years studying in Kyoto. In those days, a person could bring out [of Japan] Japanese yen, I think, up to 10 dollars. I didn't bring out anything anyway. The first examination was a written one, together with an oral (sixth grade) examination. They selected a small number; then we had a second (oral) examination at the American embassy. The first step, I had to do it; but for the second step, I had a recommendation letter from Yukawa. That must have been very good. They selected where I should go to.

I: Who was your thesis advisor in Princeton?

A: Professor [Rudolf] Haag. He was a visiting professor at Princeton; he just came exactly when I was there. He's German and was visiting Princeton for two years. I would have worked with [Arthur] Wightman [(1922–2013)] but that year he was away in France. Wightman has been in Princeton for a long time.

I: Did your PhD work determine the direction of your subsequent research?

A: Not in particular. I was already in that direction. During my two-and-a-half years in Princeton, I wrote nearly 10 papers; my thesis [on Hamiltonian formalism] was only one of them. I presented my thesis in one year, maybe two years, anyway, in summer. I don't know whether Professor Haag or Professor Wightman was really my advisor. Haag was the person who supervised my paper. He went away after half a year. He came to Princeton on exchange visa and could not have some position in the United States immediately. He later came back to Illinois. After he went to Illinois, I went there.

I: Did Haag suggest a problem?

A: No, he didn't suggest, but we had a lot of discussions. What happened was that quite often when he wanted to establish some new theory, he thought about something and gave many physical examples. Well, of course, he had to transform them into mathematics. Either this must be true mathematically or he hoped that it was true; then his interpretation is correct. Always in quite a short time, sometimes immediately, it turned out to be incorrect — I gave counterexamples. He changed it slightly, and after some time when I couldn't find any counterexample then it was actually true. The first part was easy because I could easily find counterexamples.

I: Haag depends on intuition?

A: Yeah, it's very important the way Haag was doing it — first you have to use intuition to find out what could be true and then you have to realise it mathematically. You may not succeed initially but you try it repeatedly and finally you get what you want. This is what I learned from Haag. This is how you do physics mathematically. When I was in Yukawa's laboratory in Japan, there was also discussion by people, but nobody was doing things this way. Of course, some of the people in the laboratory, already with some position, reported what they did but with terribly difficult and complicated computations. He [Haag] used a lot of examples. Privately I was also interested [in what he did] and did some computations. So I knew what he wanted could be obtained very simply and neatly by using Fourier transforms. This was some kind of "eternal" conference in summer; there were also people coming from outside. There were many ways to find the right answer and Haag taught me many things. At some point Haag was studying the formulation of operator algebras and was talking about von Neumann algebras during our discussions. So I went to the library and read von Neumann's papers I, II, III, IV. Then Professor Haag lent me a book written by [M A] Naimark [(1909–1978)] in Russia; it was a German translation. I looked at the book. I finished it in a few days because what is written is exactly what is written in von Neumann's papers. That way I switched to doing things in von Neumann algebras. Then afterwards, he asked some questions, so I just tried to answer them and did more.

I: The theory of operator algebras is a purely mathematical field. Was your work on the theory of von Neumann factors of type III motivated by physics?

A: No. You see, I was already involved with von Neumann algebras. When I went to Princeton, Jim [E J] Woods was a student there but I didn't have any discussions with him. First of all, you have to take a general examination; then start to write a thesis. You need one year residency requirement. I went there in autumn and in spring I had the one year requirement, so I took the general examination on all topics and then I did the thesis. Woods is Canadian and he took more time. After I left Princeton, I went back to Japan. Just before I left the United States for Japan, I met Professor [Res] Jost [(1918–1990)] who was professor at ETH in Zurich. He was very much impressed by one of my papers which I had published when I was in the United States. He asked me to come to his place in Zurich, and I said "Yes". So the next academic year in Europe and United States, I went there. Some time when I was there, I received the thesis from Jim Woods. I looked at it. He was saying certain things that I needed, but it was incorrect, because in my thesis, apart from the main part, I dealt with some examples which I put in the appendix but I didn't publish that part of the thesis. From that study, I knew that what Woods was saying was incorrect. I didn't write a paper to correct it. After Zurich, I went to Illinois and again met Professor Haag. Around that time I met Woods and together we got some joint paper. This is not *the* Araki–Woods paper (which is a later one). This is about free Bose gas and this summarises von Neumann algebras' point of view. This was the first time a physical model was summarised that way. This happened in the summer of '67. In those early days, he [Woods] was back in Alberta, Canada. After that, he went to University of Maryland.

In March '67, there was a conference at Baton Rouge organised by [R V] Kadison. In that conference a result of [R] Powers, who was a physics graduate student in Princeton, was presented. Up to that time mathematicians could display only 3 different type III and 3 different type II factors. Powers proved that a 1-parameter family of von Neumann algebras exists. This was a central paper in that conference and he was the first speaker. There was a preprint brought by Tomita and distributed there. This has much more interest later because of the general theory and it also has physics connections. It later became known as the Tomita–Takesaki theory, also called the modular theory of von Neumann algebras. Then I arrived at Maryland to meet Woods and I said that we should put Powers' paper and Tomita's paper together. Powers classified this thing which is a tensor product of Type I factors, which was introduced by von Neumann, but this 1-parameter family is only a very small part of infinite tensor products. I proposed to classify these infinite tensor products generally. Later I found out that he [Powers] was also trying to classify them. The motivation, at the Baton Rouge conference anyway, was Tomita's paper and a paper by Haag, [N M] Hugenholtz [and M Winnink] in statistical mechanics. These two preprints were distributed to friends. We were very much surprised because one is pure mathematics, the other is statistical mechanics. The equations are exactly the same equations. These were further developed later by Takesaki, and the theory is called the Tomita–Takesaki theory. It has great influence in statistical mechanics too. That was the beginning part, but in Tomita's papers, he didn't write proofs.

I: Mathematicians usually like proofs. Is Tomita a mathematician?

A: [Minoru] Tomita is a pure mathematician. There are a lot of algebraists in Japan, including [Masamichi] Takesaki, but Tomita is a completely different kind of person, very "singular". Anyway, I thought this was a very important thing and one should find a general theory and try to classify the tensor products. Then we started generalising Powers' paper and we found very interesting things in one of his lemmas. So we developed a theory out of this lemma. We were successful and I forgot about using Tomita's [paper]. We just used tensor products instead of the invariants he introduced in his modular theory. This was '67, and Takesaki completed his wonderful theory in '70. Then in '71 Takesaki gave a lecture in summer school in Seattle. To this Alain Connes came from France as a student. Then in '72 he started to write a lot of papers. What he did was to produce the invariants, which we used to do classical things, out of the Tomita–Takesaki theory. We should have looked into that direction instead of the other direction. But then he [Connes] just did it in '76.

I: I believe that the great physicist Paul Dirac said something to the effect that a physical theory should be mathematically beautiful. With the trend to resorting to computer simulations, do you think that the element of conceptual beauty and simplicity is now being sacrificed or at least relegated to a lower priority?

A: No. In the area where I'm working, I do not look for phenomena generated by the computer. In my institute, some mathematicians use computers. When you use computers together with mathematics, then it is a theory. First you try what could be true. When you are computing this way and do not get something correct, then you have to do something else. If you do the right thing, then it goes like this. So you understand what is going on. The computer helps you to find the right direction, and from there you do mathematics and prove things. In that way you can use it. I never use it because I'm not very good at using computers. I'm not solving any equations or something …

I: Is there a difference between a mathematical physicist and a theoretical physicist?

A: There is a difference although the boundary is not clear cut. It all depends on how a researcher considers himself or herself. The theoretical physicist, as I understand, does not care about rigour, only about the process. If he gets the right result, that's okay. The mathematical physicist would like to prove it and enjoys proving things. If you get the right result, the process can be anything. If you get the really correct things, then quite often anybody can prove it. The most difficult part is to find out what is the right thing, what is the aim. If you have a good aim, then of course, you can find things out, normally; it's not terribly difficult, but different.

I: Theoretical physics or at least mathematical physics is becoming more and more demanding in terms of the mathematics needed to understand the theory. Is there any danger that physical insight and intuition may be eclipsed by mathematical technicalities?

A: No, I don't think so. The situation in which mathematics was not used in physics before or was quite new at that time, appears in physics. This situation existed before. For example, when the theory of complex variables was developed in Princeton, the analytic properties of regions were not widely known to mathematicians, but physicists used them to compute things. The result is not so spectacular, but at least for some regions, some parts succeeded in some way. This was used by the mathematician [Mikio] Sato (then in Princeton) to develop hyperfunction theory; that partly came from physics.

I: Experimental physics and even theoretical physics seem to have become such a colossal collective enterprise that it may be very difficult for one single person to grasp the intricacies of different areas and their interconnections. Do you think that this spells the demise of the single intellectual "giant" capable of revolutionising physics in the way that Newton and Einstein did?

A: This has been the case in the past. When I was a student, theoretical physicists were divided into two groups — one was working in nuclear physics of elementary particles and the other in solid state physics. Even though they are using the same mathematical processes, they are using completely different terminology and therefore they cannot talk to each other. Of course, mathematicians and physicists also don't talk to each other. Physicists say that the mathematics given by mathematicians is not useful, and the mathematicians say that what the physicists are doing does not have any mathematical rigour, therefore incorrect. I know areas in both physics and mathematics. Quite often I have to be an interpreter. But the only thing lacking is that they don't believe what the other one is saying. If they just try to listen to each other, there are a few things they can learn from each other. At the beginning I was not considered a mathematician. For example, one of the professors, who was teaching functional analysis, was telling me at some point that in the case of the rotation group, if you take the tensor product then it decomposes into irreducibles. This is well-known and also used in physics. When I was a student there were at least three books on group representations and applications to physics, for example, [B L] van der Waerden [(1903–1996)] and [Eugene] Wigner [(1902–1995) Nobel Prize in Physics 1963]. This professor wanted to do this for the inhomogeneous Lorentz group including translations and so forth. So I said that at least for this representation (and also for other representations) this is well-known and very much used by physicists in scattering theory. I started to explain what the result is and how I can prove it, but this professor didn't believe me. You have to listen to see what other people are doing.

I: What advice would you give to graduate students who wish to pursue basic theoretical research in science?

A: I have one story to tell. You see, I am a graduate from the physics department and I am also involved in teaching mathematical physics in the physics department. A professor in the physics department once sent a student to me with two difficult mathematical questions. Quite often, the student said the following (he had studied this area of mathematics very much, meaning he had read one book, and wanted to find some problem to work on) — "Please give me some problem where I can use it." One example is category theory. What I would say is the following. This is not the attitude of the researcher in mathematical physics. The researcher first finds the problem and starts analysing that problem. Then you always find some mathematical problem you should solve before attacking this. Then you look for what is known in mathematics — find some book, read it and apply it. But often you don't find what you need anywhere. Then you have to develop it yourself. That's the way to do mathematical physics. If you just do category theory and then try to find a problem in mathematical physics to use it — that's not a good way.

I: Do you have many students?

A: Not so many. I'm already retired more than ten years now. I used to have one or two students a year in Kyoto. I had many [students] who were at the front of research.

I: You mentioned you were Director of RIMS [Research Institute of Mathematical Sciences]. For how many years?

A: For three years, before retirement. Usually the director is an older person, often from Kyoto University, but sometimes there are exceptions. The Director's job is an administrative job. I was in some research role but not as director. The Director of Research Institute of Mathematical Sciences mainly has to do administration with people outside the institute. In the university, there are no scholars who are administrators. Some administrators come from the Ministry of Education and so forth. Also the university has many different sections. What our institute couldn't do well was to get a new building. But that's a completely different thing — that has to do with matters outside the institute. Some person is needed to take care of the internal things. I did not have an official position but it is a kind of chairman. It is the mathematics department that has a Chairman. The institute has a director — somebody who is like the chairman. I did this job. Also inside the university, I was for a long time Vice-Chairman of the University Committee for International Exchange. When the building for visitors' stay was to be built, I was first in the planning committee. From the time the building was built until my retirement I was Vice-Chairman. The Chairman changes one every one or two years and is usually an older person. But the Vice-Chairman has to do the real things. All the way I was Vice-Chairman.

I: You must have been very capable of doing things.

A: For example, we had the International Congress of Mathematicians in Kyoto [in 1990] and I was the executive secretary. I did everything, every preparation in Kyoto. I claimed at the beginning that I would not able to collect money (donations). I didn't like to collect money (I didn't say that), and I'm not good at it. All other things I can do. I wrote, for example, a proposal to IMU [International Mathematical Union] and planned everything inside.

I: Did you manage to get any donations for the conference?

A: That was done by people in Kyoto. Professor [Kunihiko] Kodaira, an earlier Fields medalist [(1915–1997), Fields Medal 1954] and retired, was president of the committee. He graduated from Tokyo University. His classmates went to different fields; many went to the financial sector. They thought Kodaira could collect money. They suggested that mathematicians collect money from themselves and say that they collected so much; then they can very easily collect from other sources. This was done; I also donated. On the other hand, we were not sure how to proceed from there. Normally you ask some company to do various things. I did it all by myself. By myself, I mean I had ten secretaries for general purposes — housing, accounting, collecting fees from participants and so on. They were all in my office and we worked together. All we required was some administration. It was handled by us and not by a company.

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