ELSEVIER

Coordination Chemistry Reviews
209 (2000) 1–13

COORDINATION CHEMISTRY REVIEWS

www.elsevier.com/locate/ccr

Celebration of Inorganic Lives
Interview with Hitoshi Ohtaki

Tamás Radnái *

Institute of Chemistry, Chemical Research Centre, Hungarian Academy of Sciences, Budapest, Pusztaszeriut 59-67, PO Box 17, H-1525, Hungary

Received 10 November 1999; accepted 10 December 1999

Hitoshi Ohtaki (Ritsumeikan University).

1. Biography

Hitoshi Ohtaki was born in Tokyo, Japan on September 16, 1932. He was educated at Nagoya University where he received a bachelor degree in 1995, a masters in 1957, and a doctorate in 1961. He became a Research Associate at the Research Laboratories for Nuclear Engineering of Tokyo Institute of Technology in 1959. During his term as research associate he spent 3 years, from 1961 to 1964, as a Postdoctoral Research Fellow in the Department of Inorganic Chemistry at the Royal Institute of Technology, Stockholm, Sweden, under the supervision of Professor Lars Gunnar Sillén. He became a lecturer in the Department of Chemistry,

* Fax: +36-1-3257554.
E-mail address: radnait@chemres.hu (T. Radnái).

PII: S0010-8545(99)00255-6

Faculty of Science of Nagoya University in 1965 and was promoted to Associate Professor in 1967. He moved to the Tokyo Institute of Technology in 1970 as an Associate Professor in the Department of Electrochemistry in the Faculty of Engineering, and was then promoted to a Full Professor in 1973. Due to the restructuring of the university system of the Tokyo Institute of Technology in 1975, he changed his professor position to the Department of Electronic Chemistry of the Graduate School of the Tokyo Institute of Technology at Nagatsuta. He moved to the Institute for Molecular Science of the Okazaki National Research Institutes in 1988 and was appointed to the Director of the Coordination Chemistry Laboratories of the Institute. He was also appointed as a Professor of the Graduate University for Advanced Studies in 1988 when the university was established. The Institute for Molecular Science is one of the cooperative institutes. He was the Dean of the School of Mathematical and Physical Sciences of the university from 1992 to 1993. He retired from the Institute for Molecular Science and the Graduate University for Advanced Studies in 1993 and became Professor Emeritus of the Tokyo Institute of Technology and of the Graduate University for Advanced Studies. Then, he was appointed as Professor at the Ritsumeikan University in 1993 and he became the Director of the Institute of Science and Technology of Ritsumeikan University in 1994. He is a member of the Science Council of Japan since 1994 and he became the Vice-President of the Division of Natural Science of the Council in 1997.

His research involved solution chemistry and coordination chemistry and especially he was very much interested in studying the structure of complexes in solution and liquids by using X-ray diffraction and EXAFS methods. He has written more than 300 original papers and reviews, and his papers have been published in many international journals. He has authored or co-authored more than 10 books. He was invited to many international conferences. He served for more than 25 years in IUPAC since 1974. He was the chairman of the Commission of Equilibrium Data of the Analytical Chemistry Division of IUPAC and a Division Member of Inorganic Chemistry Division. At present he is a Bureau Member and an Executive Committee Member of IUPAC.

With Professor Jean-Marie Lehn, France, at ICCC in Florence, 1998.

He was the President of the Federation of the Asian Chemical Societies until November 1999 and was the Vice-Chairman of the International Chemical Congress of Pacific Basin Chemical Societies in 1995. He was also the President of the Japanese Society of Coordination Chemistry from 1992 to 1994, and of the Association of Japanese Solution Chemists from 1993 to 1997. He is a member of international editorial boards of Journal of Solution Chemistry, Journal of Molecular Liquids, and Malaysian Journal of Chemistry. He is one of the three founders of the Eurasia Conference on Chemical Sciences and he was the Chairman of the Second Eurasia Conference held in Seoul, Korea in 1990, and of the Fourth Eurasia Conference held in Kuala Lumpur, Malaysia in 1994. He is a Steering Committee Member of the International Conference on Solution Chemistry. He was the Chairman of the 30th International Conference on Coordination Chemistry in Kyoto, 1994, and of the 26th International Conference on Solution Chemistry in Fukuoka, 1999. He received the awards of the Matsunaga Prize in 1976, the Tejima Memorial Prize in 1989, the Takei Prize of the Electrochemical Society of Japan in 1990, and the prize for Scientific Merits of the Electrochemical Society of Japan in 2000. He was decorated with the National Medal of Purple Ribbon given by the Japanese Government in 1995.

2. Interview

T.R. At first, I should like to hear about your personal links to coordination chemistry. How did you start, how did you get acquainted with coordination chemistry, what school you had, whom you are very proud as being your professors?

When I was a primary school child, I preferred to learn language and mathematics rather than science, because at that time science meant natural science, picking up insects, collecting grasses, trees and weeds. However, when I entered middle high school (later it was changed to a junior high school due to the change in the education system of Japan), I met a very good teacher, his name is Mr Shigeo Murase. He is still living and I see him occasionally. He was a very active chemistry teacher. He taught me at the Okazaki Junior High School from the age of 14 to 15 years old. I learned systematic qualitative analysis from Group I through IV elements. At that time chemicals were very difficult to obtain and expensive, but our teacher obtained some chemicals through his own efforts. I also got some chemicals from my elder brother who was a chemist and was working in the chemical industry. I also learned a lot of chemistry from my elder brother. Therefore, I can say that these two people were very influential for me when I was younger. Thus, I became very much interested in various topics of chemistry, especially in solution equilibria like acid dissociation and pH, buffer capacity, etc. In my brother's bookcase, I found a textbook of General Physical Chemistry written by Professor Toshizo Chitani, who was a professor at Osaka University. The book was very much helpful to me for further studies in chemistry.

T.R. Was it a difficult book to read?

Yes and no. The book is at a graduate student level. I was glad to read it and maybe I understood about a half of its content. When I was a student of Okazaki High School and even later entered Nagoya University, chemistry was a very easy subject for me. Being a senior student of Nagoya University, I entered the laboratory of Professor Kazuo Yamasaki in the Department of Chemistry, and his Associate Professor was Dr Hidetake Kakihana, who later became a professor of Tokyo Institute of the Technology. Professor Yamasaki is a coordination chemist (see *Coord. Chem. Rev.* 3 (1994) 133) and one of the successors of Professor Yuji Shibata, the founder of Coordination Chemistry of Japan. He is interested in antiquarian chemistry through analyses of materials and pigments of historical products. Professor Kakihana was interested in ion-exchange chemistry, and I joined Professor Kakihana's group. After graduation form Nagoya University I went on to the graduate course of the same university. In 1959 I was employed as a Research Associate at the Tokyo Institute of Technology because Professor Kakihana moved there as a full professor. At that time I did not finish my Ph.D. course, but I did continue my research work even after I moved to the new position and I received my doctorate in 1991 from Nagoya University. Professor Kakihana was so kind in this matter, and he sent me to Stockholm immediately after I received my doctorate to the laboratory of Professor Lars Gunnar Sillén in the Department of Inorganic Chemistry of the Royal Institute of Technology as a postdoctoral fellow. Professor Sillén was an excellent solution chemist, and I learned from him a lot of chemistry and philosophical ideas on how chemists should study. He was also one of the chemists who influenced me most. Unfortunately he died in 1970 at the age of only 54 years old.

T.R. So you met several foreign scientists in Sweden.

Yes. During my stay in Stockholm from 1961 to 1964, I met many famous scientists who visited Professor Sillén's laboratory to work with him or to give lectures. Sometimes Nobel Laureates of Physics and Chemistry had lectures at the Royal Institute of Technology in December after the Nobel Prize Ceremony. I could travel in Europe to visit laboratories of famous scientists. Many of my friends in the same laboratory who were almost the same age with me became professors in their own countries after their postdoctoral work. Many of them who visited Professor Sillén's laboratory became my acquaintances and even friends. Some established scientists were so kind to the young chemist from Asia who visited their laboratories. I can mention many names with my thanks. For instance, they are Professor Jannik Bjerrum in Copenhagen, Professor Viktor Gutmann in Vienna, Professor Kalman Burger in Szeged (Budapest at that time), Professor Kim A. Burkov in St. Petersburg, Professor Thomas Spiro in Princeton, the latter three spent their days in Professor Sillén's laboratory together with me for 1 year. Professor Yizhak Marcus of Jerusalem is also a schoolmate of Professor Sillén's laboratory. Professor Jakob Marinsky in Buffalo, who was a good friend of Professor Erik Högfeldt, a co-worker and a close friend of Professor Sillén, became a close friend of mine, too.

With chairpersons of ICCC's. From right, Professor Ivano Bertini, Italy (33rd ICCC), Professor J. Costamagna, Chile (32nd ICCC), Professor Chris Orvig, Canada (31st ICCC) at the banquet of the 30th ICCC in Kyoto, 1994.

T.R. Do you have some names to be mentioned in the laboratory of Professor Sillén?

I must mention Professor Georg Johansson, who was originally an X-ray crystallographer, but was doing solution X-ray diffraction studies in the laboratory. He learned solution X-ray diffraction techniques in the USA and came back to Stockholm just before I went there. However, in the period from 1961 to 1964, I did not have close contact with him. When I went to Sweden again in 1969, I learned solution X-ray diffraction from him. Since then, we cooperated very closely until he retired from the Royal Institute of Technology. The late Professor Erik Högfeldt was also one of my best friends. He was studying ion-exchange chemistry, and thus, we knew names of each other before I went to Stockholm, and then, he changed his direction to solvent extraction and two-phase equilibrium studies. Dr George Biedermann is a person whom I cannot forget. He was rather difficult, but a very excellent experimentalist.

T.R. I want to ask you about your research in Professor Sillén's Laboratory.

Oh yes. Before me, three Japanese scientists worked with Professor Sillén. The first was Professor Kakihana, who stayed there for one and a half years from 1955 to 1956, then Professor Yukiyoshi Sasaki of the University of Tokyo was there. Professor Tatsuya Sekine, who was a postdoctoral student of the University of Tokyo and became Professor Emeritus of the Science University of Tokyo, came to work with Professor David Dyrssen for solvent extraction chemistry. When I was in Professor Sillén's laboratory for the first time, I studied potentiometry, e.g. pH-metry for hydrolysis of metal ions in solution. The first subject I took was hydrolysis of lithium ions, the subject being suggested by Professor Kakihana when I left Japan. This subject is difficult, of course, because Li^+ is hard to hydrolyzed in water. If I could go

back, I would not select this subject. However, I tried to determine the association constant of lithium hydroxide by referring to sodium hydroxide in a 3 M sodium perchlorate ionic medium. The results might include the influence caused by the change in the activity coefficient of the lithium ion, and thus, this study required a better understanding of ionic activity coefficients with the composition of solutes and solvents. The next subject was the hydrolysis of nickel ions. The work was quite satisfactory, and I enjoyed Sillén's mathematical treatments of polynuclear complex formation. I learned computer calculations at that time. In the third year, I selected a study of activity coefficient variations with solutes in solution.

T.R. What did you do then?

When I came back to the Tokyo Institute of Technology after the three year stay in Sweden, my position was filled by another person, and I practically lost my research associate position in the Tokyo Institute of Technology. Professor Yamasaki kindly paid attention to my situation and suggested that I apply for a lecturer position of the Analytical Chemistry Laboratory of the Faculty of Science in Nagoya University. Professor Motoharu Tanaka had just been promoted to Professor, and he accepted me. I wanted to continue the studies I began in Stockholm, and Professor Tanaka gave me the most modern pH-meter made in Japan. However, the equipment was not sufficient to make such a complicated study in solution. After many problems, we bought a pH meter made in Denmark, which was the same one as I used in Sweden. The work I carried out was the hydrolysis of metal ions in aqueous mixed solvents. In mixed solvents, hydrolytic reactions of metal ions take place similar to aqueous solutions, and I found that the equilibrium constants for the hydrolytic reactions, i.e. acid dissociation constants of the metal ions, are practically independent of the concentration of the organic solvent. This behavior is different from acid dissociation constants of the usual acids such as carboxylic acids and protonated amines. The dissociation reaction of protons of hydrated metal ions is a third type of acid dissociation reaction in mixed solvents. I could learn from these studies what are acids and bases. Moreover, the determination of the stability con-

With Professor Kazuo Yamasaki, 1992.

stants and composition of metal hydroxo complexes in solution raised questions about their structures, which moved me to study the structures of metal complexes in solution, and then, those of liquids as well.

T.R. When you returned to Japan from Sweden, was there a strong determination to continue that kind of work, to implement that kind of technique here in Japan, or was that not possible? What was the situation at that time?

When I returned to Japan in 1964, Japanese economic and scientific situations were still very difficult. We did not have a good pH-meter like that which we had used in Stockholm, the precision was ten times less, and therefore, this kind of high precision measurement was very difficult to carry on. Research funds were small, as well as our salary. The pH-metry was a rather highly technical method not only for Japanese but also for other chemists around the world. Therefore, values reported for a given system are often diverse, and thus, Professor Sillén wished to compile stability data in his book 'Stability Constants of Metal-Ion Complexes' edited together with Professor Arthur E. Marthel (in the first edition of the book Professor G. Schwarzenbach was also included in the editors), which led to the establishment of Commission V.6 (Commission of Equilibrium Data) in IUPAC in 1955.

T.R. What was the situation for coordination chemistry in Japan when you came home from Sweden?

Until the first half of the 1960s coordination chemistry was a relatively strong field of chemistry in Japan, but I felt that it was rather classical. The synthetic chemistry of cobalt(III) complexes was still one of the most intensively investigated fields. The spectroscopic series discovered by Professor Ryutaro Tsuchida of Osaka University, who was one of the pupils of Professor Shibata, is a landmark in the study of coordination chemistry in Japan. Reaction kinetics had begun to be studied by some people who had investigated reaction equilibria. Organometallic chemistry was a part of organic chemistry and not inorganic chemistry. Structural determinations of complexes by X-ray diffraction were carried out for many crystals, but it took too much time. The coordination chemistry of optical isomers was a traditional subject of Japan's coordination chemistry since Professor Shibata came back from Professor Alfred Werner's laboratory in 1913 and the determination of the absolute configuration of optical isomers of metal complexes initiated by Professor Yoshihiko Saito of the University of Tokyo (at that time he was a Professor of Osaka City University) was a most remarkable success of Japan's coordination chemistry in this period. In spite of a wide variety of studies of chemistry in solution even at that time, we did not have the term 'solution chemistry' in Japan, and solution chemistry was quite minor compared with coordination chemistry. pH-Metry had just started being used in studies of stability constant measurements, but no computers had been used in the calculations. In fact, when I went to Nagoya University in 1965, no one had used electronic computers in the Department of Chemistry. I suggested to students in our laboratory to use electronic computers in their calculations, but no one accepted my suggestion, except one, Mr Kiyoshi Sawada, now a Professor of Niigata University,

until I left Nagoya University in 1970. We were rather a new wave in solution and coordination chemistry in Japan at that time.

T.R. How did you start solution X-ray diffraction studies in Japan?

That is a rather interesting question. I received many questions as to what the structures of the polynuclear complexes, which I reported, as well as the formation constants. Of course, I could not answer these. Thus, I was more and more attracted to the determination of structures of complexes in solution. Some people had used NMR for the structural studies of hydrated ions and complexes in solution, and Professor Taube was one of them. The solution X-ray diffraction method had not been widely employed by chemists, but I thought that the method would be quite useful. Neutron diffraction was not very familiar and the availability of the equipment needed for such studies was limited. We did not have such a diffractometer in either the Tokyo Institute of Technology or Nagoya University. The EXAFS method had not been developed.

I knew that some people around the world had been studying solution X-ray diffraction for water and complexes, and after some contact, I finally selected Dr Georg Johansson as my new supervisor for studying structures of metal complexes in water. I went to Stockholm again in 1969 for 6 months, when student revolutions began in various universities in Japan. After some experience of this work I came back to Nagoya, and wanted to buy a solution X-ray diffractometer by applying for funds to the Ministry of Education of Japan. However, such a large grant could not be given to a young chemist like me, as senior professors always got the grants. I almost gave up my structural study of solution species. At the beginning of 1970, I was invited to the Department of Electrochemistry of the Faculty of Engineering of the Tokyo Institute of Technology. They needed some young active chemists to study ionic reactions in solution, because a new Laboratory of Fundamental Electrochemistry had been founded in the Department. Thus, I went to the Tokyo Institute of Technology. Professor Masao Mukai, who died some years ago, kindly supported my work, and I obtained a grant from the Ministry of Education in 1972 to buy an X-ray diffractometer

With Professor Henry Taube, USA, at Stanford, 1994.

for solution studies, and thus, I could start solution X-ray diffraction work in the Tokyo Institute of Technology. The first paper, concerning the structure of the tetraiodocadmate(II) complex, was published in 1974, and the second paper was the study of the hydration structure of copper(II) ion in water. This was the first paper to elucidate the copper(II) ion having a six-coordinate, Jahn–Teller distorted octahedral structure in water. Thereafter, our solution X-ray diffraction studies obtained citizenship in the society of coordination chemistry of Japan.

T.R. Could you tell me some basic determining points, time, place and institutes which you consider as turning points in Japanese Coordination Chemistry? How about the foundation of Coordination Chemistry Laboratories in Okazaki?

Yes, coordination chemistry was first introduced in Japan by Professor Shibata in 1913. He made especially important contributions to the synthetic chemistry of metal complexes and visible spectroscopy, because he was a co-worker of Professor Werner in Zurich. The optical isomers synthesized were studied by the method of optical rotation. The application of X-ray diffraction to the determination of the absolute structure of optical isomers was first successfully undertaken by Professor Yoshihiko Saito. Since then, synthetic studies of inert metal complexes followed by crystallographic structural analyses were one of the main subjects of coordination chemistry of Japan, and many new laboratories called 'Coordination Chemistry Laboratory' were established in various Japanese national universities. Thus, the coordination chemists of Japan had a high profile in science, and wanted to establish a National Institute of Coordination Chemistry.

One of the epoch-making occasions of coordination chemistry in Japan in the period 1960–1980 was the organization of the 10th International Conference on Coordination Chemistry (ICCC) in Tokyo and Nikko in 1967. Professor Shibata was the chairman and Professor Yamasaki supported him as the Secretary General. The conference was quite successful. Another important occasion was the formal establishment of the Japan Society of Coordination Chemistry in 1982. Professor Yamasaki was elected as the first president of the society. These two events facilitated the establishment of a national institute for coordination chemistry. Professors Kazuo Yamasaki of Nagoya University, Kazuo Saito and Nobuyuki Tanaka of Tohoku University were the leaders pulling people in that direction. Professor Nobuyuki Tanaka was elected as a member of the Science Council of Japan to promote the establishment of the Institute of Coordination Chemistry. In the period 1960–1970 many suggestions were submitted to the Japanese Government from the Science Council of Japan, and following these suggestions the National Laboratory of High Energy Physics in Tsukuba and the Institute for Molecular Science in Okazaki were established. The Institute of Coordination Chemistry was also expected to be established following the Institute for Molecular Science, but due mainly to economical reasons the establishment of the institute was postponed, and finally, the plan was realized in Okazaki on a smaller scale as the Coordination Chemistry Laboratories which are attached to the Institute for Molecular Science. The first director of these Laboratories was Professor Kazuo Saito (see *Coord. Chem. Rev.* 25 (1994) 133), and then, after his retirement, I was asked to be his successor. The Laboratories played their role as a center for studies

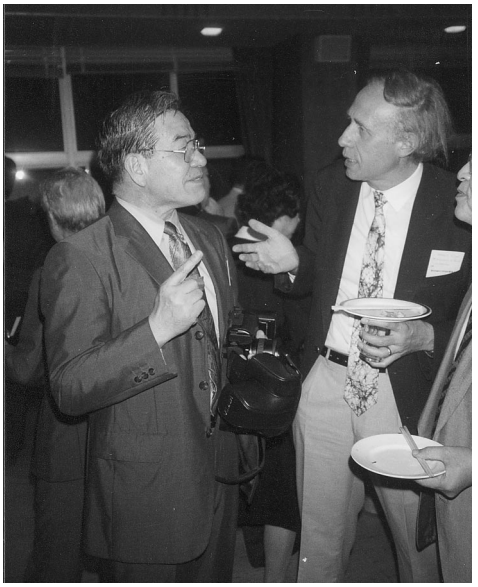
in coordination chemistry in Japan and activated studies and international relationships in coordination chemistry.

T.R. You said that you were a new wave in the 1960s. Do you now see some new waves to come?

We now have very active young chemists in Japan in the field of coordination chemistry, and some of them are working on supramolecular, organometallic and bioinorganic chemistry. We also see some good chemists in coordination chemistry, but I wish them to be more ambitious in their studies. As you know, most Japanese are rather modest and often they admire work done by people in highly developed countries such as America and in Europe and easily follow their work. This behavior is sometimes criticized as a lack of originality. I see some points which may be criticized in coordination chemistry in Japan, and I expect that young scientists will break through such criticisms and will establish new fields of coordination chemistry, new methodologies, and new theories.

T.R. You made excellent progress in the field of coordination chemistry and solution chemistry, but I think that such an achievement could not have been done without good colleagues. Could you mention some of these colleagues in your laboratories?

It is really true that my achievements could not be done without my students, colleagues and friends in my laboratories in the Tokyo Institute of Technology, Nagoya University, Ritsumeikan University, as well as other institutes around the world. Most of my students became very active professors of chemistry all over Japan. When I became professor of the solution chemistry laboratory of the graduate school of the Tokyo Institute of Technology in 1973, we had a new graduate course and the first student of the doctorate course of my laboratory was Mr Toshio Yamaguchi, who is now a professor at Fukuoka University. Dr Maeda, who is now a professor at the Nagoya Institute of Technology, was a research associate who supported my solution X-ray studies in the 1970s. Professor Isao Okada, who is now a professor at Sophia University and Professor Emeritus of the Tokyo Institute of Technology, showed me the usefulness of molecular dynamics simulations in studies of solution chemistry. Owing to his stimulation, I studied dissolution and crystallization processes of ionic crystals in water by molecular dynamics simulations and could make video pictures of the processes. It was the very first attempt to visualize MD simulations on video, which has been used for chemical education in many countries such as Canada, Czech Republics, Hungary, South Africa, Germany, etc. as well as in Japan. Dr Shin-ichi Ishiguro, who is now a professor at Kyushu University, established an automated calorimetric measurement system in our laboratory by connecting a calorimeter with a small computer which I used for mathematical treatments. Thus, MD, thermodynamics and structural chemistry were combined together in order to explain chemical reactions in aqueous and non-aqueous solutions. At that time no other attempt was made anywhere in the world to combine studies of solution structural chemistry with molecular dynamics simulations or thermodynamics within one laboratory, although some groups in different countries or in different laboratories in the same country did so. Professor Ishiguro played an important role at the 26th Interna-



With Professor Helmut Sigel at the 30th ICCC in Kyoto, 1994

tional Conference on Solution Chemistry held in Fukuoka in 1999 as the General Secretary, who led the conference so successfully.

T.R. Can you mention which are the most important scientific results in your research?

In my opinion, I made some important contributions to structural solution chemistry by using mainly the X-ray diffraction method, which was not well accepted before 1970. The structure of solvated ions was not well determined at that time. Before our work the copper(II) ion was believed to have four water molecules in the equatorial position in water. I was very happy when I, together with you, wrote up a review (*Chem. Rev.* 93 (1993) 1157), which was a summary of not only the structural and dynamic data of hydrated ions but also of my own studies.

The stopped-flow-EXAFS method developed at the Institute for Molecular Science in Okazaki is one example which I want to mention. When I planned to do this study, most of our friends did not believe that I could succeed. The success of the work was due to a strong will to succeed, a good selection of systems to be studied, and the research fund given by the Ministry of Education of Japan to set up the equipment for the investigation. Although the results obtained by us are yet rather limited, the method can be a new methodology in investigating mechanisms of chemical reactions in solution. I dare say that bravery with careful planning is necessary to challenge an unknown field, which may be always required of scientists.

With Professor Stanley Kirschner (Detroit, USA) and Professor Flemming Woldbye (Copenhagen, Denmark) in Denmark, 1983

T.R. You have wide international relationships. Could you mention important scientists who supported your international activities?

I have already mentioned some names who helped me before 1970. After 1970 when I started X-ray work, I met many excellent scientists over the world at various occasions such as international conferences and organizations, exchange programs, and visits from both sides. Professor Henry Taube of Stanford University is a very important mentor for me. We never cooperated in any project, but I believe he understands my work very well. When he came to Japan by invitation of JSPS, he did not like solution X-ray studies, because he hardly believed the results published in the USA. I explained what we were doing and showed our results, and then, he believed me and supported the program. Professor Jean-Marie Lehn of Strasbourg is another supporter and friend. When we organized the first Eurasia Conference on Chemistry of Solution (EuAs C_2S; later the name was changed to Eurasia Conference on Chemical Sciences) in Bangkok in January, 1988, we invited him as a plenary lecturer. Just immediately before the conference in 1987 he received the Nobel Prize, and our conference was the first conference for him after the presentation. It was a quite impressive and memorable occasion for him, and he has been so helpful to the Eurasia Conference since then; we became good friends. I met Professor Mihaly Beck of Debrecen, Hungary at IUPAC General Assemblies as members of the same commission. Professor Josef Barthel of Regensburg was always very kind to me. I knew Dr Gábor Pálinkás of Budapest, because he had cooperative work with Dr Karl Heinzinger of Mainz. Professor Bernd M. Rode of Innsbruck was in Japan as a postdoctoral fellow around 1979 and I was introduced to him in Tokyo by the late Professor Viktor Gutmann. Since then, we have become very close friends, and we established, together with Professor Ivano Bertini of Florence, the Eurasia Conference on Chemical Sciences in 1988, which is going on mainly in the Asian area with the help of Professors Jan Reedijk of Leiden,

Geoff Sykes of Newcastle upon Tyne, Gilbert Balavoine of Toulouse, John Webb of Murdoch, Australia, and Mu Shik Jhon of Seoul, who was the chairman of the National Organizing Committee of the second Eurasia Conference held in Seoul. I, among others, was the chairman of the International Organizing Committee. Through the friendship with these eminent scientists inside and outside Japan, I could absorb their wisdom and energies and even manner for the international relationship, and with their help, I could organize many international conferences, symposia and exchange programs successfully.

T.R. Thank you very much for the interesting talk.