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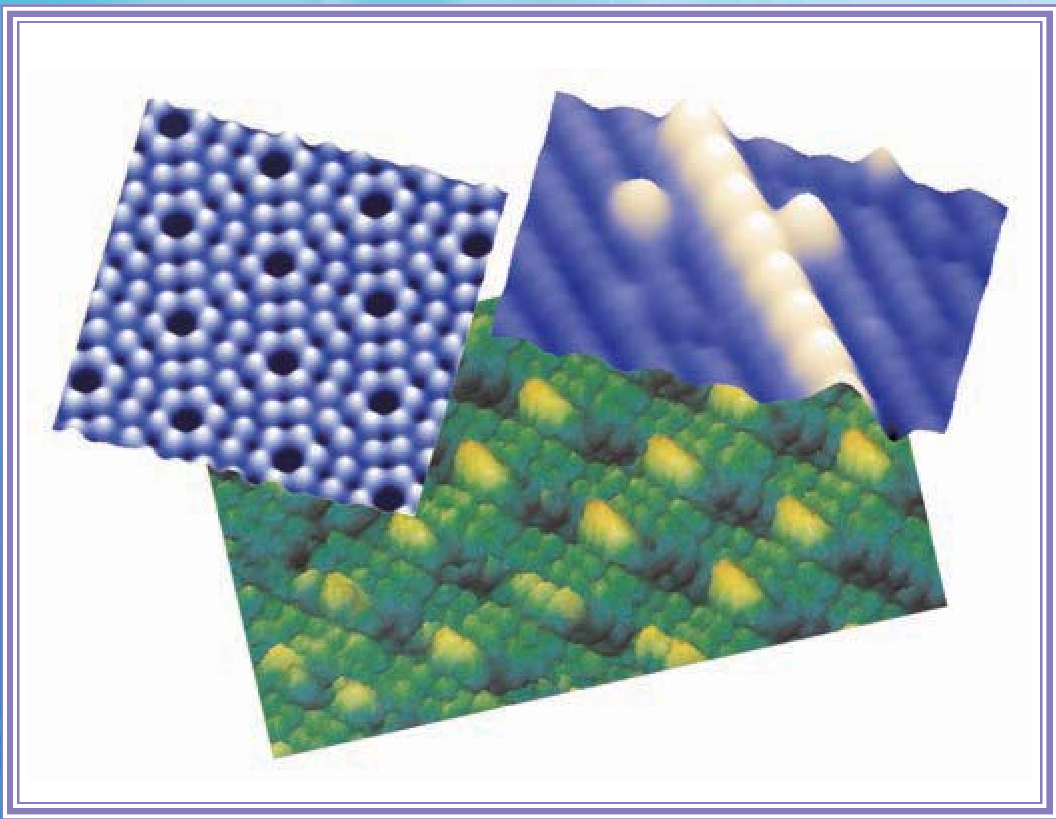
WPI-AIMR NEWS

April24, 2009



World Premier International Research Center
Advanced Institute for Materials Research

Tohoku University

Cover: Scanning tunneling microscope (STM) images of atomic arrangements on semiconductor surfaces by T. Hashizume, Professor of WPI-AIMR

Thermally stable Si(111)-7x7 structure (left), an artificial single-atom-wide wire formed by extracting a chain of hydrogen atoms using an STM tip from a hydrogen covered Si(100) surface (right), and a Ga-rich GaAs(001)-4x6 structure fabricated by MBE (molecular beam epitaxy) (center). These kinds of atomic arrangements were made possible to observe thanks to the invention of STM.

表紙: 半導体表面の原子配列を観察した走査トンネル顕微鏡（ＳＴＭ）像

（WPI-AIMR　橋詰富博教授　提供）

熱的に安定なシリコン（１１１）表面の７×７構造（左）、水素原子に覆われたシリコン（１００）表面から、水素原子を一原子列引き抜いて人工的に作製した原子細線（右）、および、ＭＢＥ（分子線エピタキシ）により調整したガリウム組成比が１／２よりも大きなガリウム砒素（００１）表面の４×６構造(中央)。このような原子配列は、ＳＴＭの発明により観察できるようになった。

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Director: Yoshinori Yamamoto
Editor, writer: Toshio Sakurai
Associate Editor, writer: Hiroshi Komatsu
Associate Editor: Tomihiro Hashizume

We welcome your comments, questions and involvement.
e-mail to: wpi-office@bureau.tohoku.ac.jp, or
sakurai@imr.tohoku.ac.jp

Contents

Yoshinori Yamamoto, Director of WPI-AIMR

Preface

It is a pleasure for me to deliver volume 5 of WPI-AIMR News to all of you. Since the advanced institutes of materials research (AIMR) based on the MEXT program of world premier international research center initiative (WPI) was launched on October 1, 2007, one year and seven months have passed. I want to summarize briefly the progress of WPI-AIMR on this occasion.

(1) World top-class researchers have gathered from all corners of the world, as you can see the table shown below.

Number of Researchers as of March 2009

	PIs	Other Researchers	Total
Japanese	18	32	50
Foreign	11	22	33
Total	29	54	83
Final Goal	30	90	120

Statistics of Foreign Researchers at March, 2009

A. Ratio of Foreign Researchers to;
   a) all researchers: 40% (33/83), b) PIs: 38% (11/29),
   c) other researchers: 41% (22/54)
   cf) other researchers: 26 % at April, 2008

B. Statistics by Country
   a) PIs: USA 4, China 3, UK 2, Germany 1, France 1
   b) other researchers: China 13, Russia 2, Bangladesh 1, Canada 1, Greece 1, India 1, Spain 1, Taiwan 1, UK 1
   c) area total: Asia 19 (58%), Europe 9 (27%), America 5 (15%)

We have 83 researchers at present and we can still hire excellent researchers to reach our final goal of recruitment. The ratio of foreign researchers has increased to around 40 % and I think that this is a significant increase in comparison with 26 % at April, 2008.

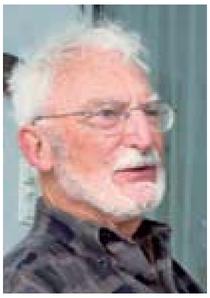
(2) The construction of the main research building of WPI-AIMR (ca. 7000m^2) has now complete, and the WPI-annex building (2500m^2) was complete about one year ago. Accordingly, we have now significant laboratory space at Katahira campus in order to carry out world-top class fusion/integration research on materials science. I hope that we will have more space here in Katahira to accommodate all the researchers of WPI-AIMR who are now pursuing their researches at Aobayama campus.

(3) In the spring meeting of American Physical Society, Professor Akihisa Inoue was awarded

James C. McGroddy Prize which is given to those who have accomplished outstanding achievements in the field of materials physics, and Professor Terunobu Miyazaki was awarded Oliver E. Buckley Condensed Matter Prize which is given to those who have accomplished outstanding contributions to condensed matter physics. This is really good news for all of us. Not only these two international prestigious prizes but also many international and domestic awards were given to both senior and junior researchers of AIMR. Furthermore, significant amounts of research grants have been given from MEXT to our WPI researchers in the 2009 fiscal year; the total amount of FY2009 Grant-in Aid for Scientific Research obtained by AIMR is 385,000,000 yen. Of course, many researchers have also received research grants from other sources, such as JST and another ministry. I believe that the above accomplishments and results indicate clearly our high research activities and excellent contributions to materials science.

Taken together, I think that we have got off to a good start, and I strongly hope that we will be able to continue this high level of activity or even to enhance the activity to much higher level. The volume 5 of WPI-AIMR News contains of the reports of 2009 WPI-AIMR Annual Workshop, in which you will be able to look the present status of our research activities. I appreciate very much all the participants, including not only WPI-AIMR researchers but also those who came to attend the workshop from both domestic institutes and overseas, to join the workshop. I would like to ask all of you continuous support and cooperation to WPI-AIMR.

Interviews



Interview with Dr. Heinrich ROHRER, 1986 Physics Nobel Laureate & Chair, International Advisory board, WPI-AIMR

"Use your unbiased mind and do the best once decided"

Prof. Komatsu (K): I met you a long time ago when you gave a special talk in Stuttgart about STM.

Dr. Rohrer (R): When was this?

K: This took place in 1983. You gave a talk on STM at the ICCG (International Conference on Crystal Growth). In the last slide you showed a figure of the detection limits of microscopy, which I had shown in the IBM Laboratory in Rueschlikon in 1978, when I was invited by Dr. Hans Scheel.

In fact, a few days before the ICCG (1983) in Stutgart, the ISSCG (International Summer School on Crystal Growth) was held in Davos. It was just after my lecture in this Summer School that I learned from Dr. Bednorz for the first time that in IBM the talented young guy (Dr. Binnig) and the expert (you) of physics found new microscopy capable to resolve to the atomic dimensions. This was a great happening to me.

Anyway today, my visit is an interview for the next WPI-AIMR News.

R: I receive them regularly and have read the two previous interviews.

K: I just wanted to roughly know your personal history and motivation for pursuing science and technology. How did you start up your research?

In terms of finding a new process, STM, how did you come across this phenomenon? At the end, I like to ask if you have a message for the younger generation.

A lot of people are interested in your childhood, your readings and, also the most important of all, the motivation that brought you to science. I have heard that you were a student of Professor Wolfgang Pauli.

R: Not a student of Prof. Pauli, I simply took theory courses from Prof. Pauli – he was the only theory Professor. Of course you always feel kind of proud to have had courses with a famous teacher, even as an experimentalist.

K: He was quite an extraordinary man.

R: Yes, he was indeed. He was also quite tough with his theory students and assistants, but quite nice to the experimental physics students. If a student could not answer any of the questions asked, then Pauli asked to tell him what he knew. If Pauli got a coherent story the student passed.

K: I heard that there was a Pauli effect. Whenever Prof. Pauli came close to experiments, something went wrong.
R: Yes, yes. The story is, that even passing in a train nearby was sufficient – quite some long range interaction and quite a different type of Pauli effect.
K: O.K. Anyway, you were born here in Switzerland.
R: Yes, I grew up the first sixteen years in a rural area, in Buchs, a village in the middle Rhine Valley, at the border of Lichtenstein and Austria. My father had a small transport business, with horses. Cars were a rarity in those days of the Second World War, there were very few of them in the village of some 5000 inhabitants. During these years I went to primary and secondary school. I grew up with a good balanced mix of school work, helping at home with horses and many other chores – my father was often away, serving in the Swiss army – but I also had fun and all kinds of outdoor adventures with friends and later with the boy scouts. I guess that this symbiosis of school and practical physical training helped to develop my practical abilities from fingertips to muscle work and also mental flexibility, overview and good understanding. I was reasonably good in school, my school grades ranged in the upper half at the beginning and improved to the top 10% towards the end of secondary school.
K: Better in everything?
R: No, French, for instance, meant too much mouth gymnastics. But I liked everything related to mathematics and geometry.
K: Mathematics. Also science?
R: Well, yes.
K: Natural science.
R: It was not science yet, it was more education about nature, about plants and animals, and also a timid beginning into physics and chemistry. After secondary school we, my mother and my two sisters, moved to the city of Zurich and I was supposed to enter High School there on the recommendation of my former secondary school teachers.

Unfortunately, I failed the entrance exam in order to continue schooling according to my age. They judged that my level was not sufficient, having come from a countryside school and not from another high school and in addition from a different Canton (state or prefecture) away from Zurich. This meant repeating one year in the new high school, but we did not have the money for such a delay in my schooling. My older sister found an affordable private school with the chance of entering University even a year and a half earlier than by a normal curriculum. I made it and could enter the ETH (Swiss Federal Institute of Technology) already with 18 years. This is an example that shows that often an apparently catastrophic incident – in my case failing the entrance exam into the public high school – emerges as a chance for a new path to higher levels, instead of indulging in frustration. Also later, in experimental physics, some new elegant and powerful solutions were so to speak "forced upon me" by failures or mistakes. That is why the freedom of science has to include also the freedom to make mistakes.

For a while, I thought of studying ancient languages, a natural inclination in the Storm and Stress years of a boy eager to impress young girls with his excellent knowledge of Latin and Greek. But somehow I judged my overall humanities vein as being too thin.

Therefore, I finally decided on a more technical profession. It looked to me more down to earth and solid for a lifelong profession. My high school grades in mathematics, chemistry and physics were pretty good, actually top. In addition, building large bridges spanning valleys was one of my boyhood dreams.

Only in the enrollment office of the ETH, I decided to enter the department of Physics and Mathematics, having mathematics in mind. However, I realized during the first year, that thinking in abstract mathematical terms was not my line, so I decided to continue in experimental physics. It seems that it was a reasonable choice.

In summary, I found my way to the University despite my rather modest roots and curriculum. I improved by the challenge to perform as best as I could, not striving for being the best all the time. I believe that ever more demanding challenges are the key for improvement in school or wherever; not contests, competition, ranking and pressure by family and others. If we could realize that, we would be happier and, above all, would have happier children. I got encouragement from my mother, my two sisters and teachers to study, but was never put under pressure. However, I was conscious of the expectations to do my best.

The cultural background, social descent, and family environment seem to play a lesser role for becoming a profound scientist than in many other professions like in art, business and politics. There are always exceptions in the case of the latter, in science it is the rule where nearly every member makes his own career.

K: A personal career. Individualistic.

R: Yes, and that is also the key to a world embracing performance of the highest quality of science and the source of the tremendous scientific progress. Anyone who has science of quality to offer is a most welcome member of the worldwide scientific community. The "science market" is open and accessible to anybody, independent of gender, religion, race, or nationality. The unequaled international touch of science made science to the first and the most successful international enterprise.

K: The scientists have the chance to engage in something new.

R: Yes. After finishing the undergraduate years with a master thesis on fast neutron scattering (neutrons from the fusion process), I had to decide whether I should continue in nuclear physics or change to solid state physics. I chose the latter because a good friend of mine encouraged me to join the small low temperature laboratory of ETH, where he started half a year earlier. Again, it turned out O.K.

M. Sakurai (S): But you did not consult.

R: No, I did not. Choosing and deciding without consulting others is not only a freedom of science, but often an utmost necessity. But once chosen, you have to make all the efforts to do a good job before giving up. It is the determination to do your best which makes you succeed, not lamenting for having missed something. Of course, one is never sure whether one would not have performed better with a different choice, but likewise one has no guarantee that it would have turned out better. Again, if we would finally understand that we would enjoy a constant pleasure of doing science instead of dreaming of or waiting for a big bang.

K: Once you decide.

R: Yes, once you decide. Maybe I also would have become a good chemist. A scientist's career is not very predictable. Hard work is the only sure thing and occasionally lucky circumstances. But the luck you have to grasp by yourself.

K: Yes, some would call it "serendipity."

R: Yes. Serendipity got unfortunately a disparaging taste in science, wrongly, I mean. The word was coined by Horace Walpole (1754) according to the Persian fairy tale "The Three Princes of Serendip." It is cleverness and sagacity which lead them to the unexpected, the latter does not simply fall out of the blue sky.

S: Right, something.

R: Louis Pasteur expressed it differently: "Chance only favors the prepared mind." In both cases the spirit is the same, you have to play an active role, it is not simply roulette.

K: That is right. A prepared mind is important. Preparation can come with it.

R: If your mind is prepared and susceptible, you notice the unexpected and the changes. In the case of the scanning tunneling microscope, a lot of scientists said: "You were simply lucky." I usually responded "Oh, sure. We were lucky, but at least we noticed that we were."

K: Luck is also an element of success and of breakthroughs.

R: Correct. It is not a disgrace to be lucky, even for a scientist. Most people, also scientists, are lucky but only very few notice it. Missing it though, is a disgrace.

The decision to change to Solid State Physics was again a “lucky” decision. The theme of my Ph.D. thesis was measuring the change of length of superconductors at the superconducting-normal transition in an applied magnetic field.

K: The change of the length?

R: Yes. In an applied magnetic field, the transition normal-superconducting is of first order which means a discontinuity in volume. Minute volume changes of solids are difficult to measure, easier length changes though. My thesis advisor Dr. Olsen, actually an advanced Post-Doc from Oxford, had pioneered this type of nano-mechanics by measuring the discontinuity of Young’s Modulus.

K: What material did you use?

R: Practically all superconducting metals with a transition temperature above 1.9 K. I succeeded to measure length changes in the order of 10^{-8} cm of samples of some 10 cm length.

K: Difficult to measure this?

R: Once you have done it, then it does not look that difficult anymore, though maybe delicate. Actually, it was a very crude method and set-up. I transformed the length change of the 10 cm long sample – because of its length it had to be vertical in the liquid Helium Dewar – with two wheels of diameter of about 4 cm and with bendable spokes into a horizontal rotary motion.

The rotary motion got then transferred via a long and thin Invar tube into a room temperature compartment at the top of the Dewar and its rotary motion detected there by reflecting light by a mirror fixed at the rod onto two photocells. Actually the deflection angle was of the same order as that later in the optical detection of the cantilever excursion in AFM.

There were of course several noise sources, the boiling liquid helium and liquid air surrounding the Helium Dewar, and the switching of the magnetic coils surrounding the Dewars. At those times, we just had an air refrigerating machine and after some days, the liquid nitrogen in the storage vessel of some 100 *l* had evaporated and only liquid oxygen was left. I did not care that much about the danger of using liquid oxygen to keep Helium cool, much more annoying was the strong paramagnetic response of the liquid oxygen to switching on and off the magnetic field.

In spite of the crude set-up, the detection of the length changes was delicate enough, that I only could measure at a bandwidth of about 2 Hz and when the town was asleep and no tramways running at a distance of some hundred meters. Once I got very excited by a periodic signal of about 0.2 Hz or so in the measurements. A few days later, I found to my disappointment that this signal was caused by big fir trees in the garden swinging to and fro in a strong wind.

K: This was published?

R: It was published in Helv. Phys. Acta, but I am afraid that I cannot find a copy anymore. I threw most of the paper stuff away after going into retirement.

K: So, that was your Ph.D. thesis?

R: Yes. So far we talked about my professional formation. But there are also other important aspects in one's life. Never forget to have some fun. And our group of about 6 low temperature physics Ph.D. students indeed did not forget it. We went every year together for ski holidays – once we had to return after three days because we ran out of money – organized trips to very fine restaurants with excellent wine – and had to live afterwards on food which barely quenched the hunger, or organized other outings and parties in the laboratory rooms. On one such occasion, I got to know Rose-Marie who later became my wife – and still is. She is extremely language gifted, has an extraordinary aesthetics touch, and is a fabulous cook – all the qualities I do not have in abundance. Nevertheless, she typed my Ph.D. thesis, not understanding a word. The mechanical typewriters of that time did not even have a correction key. Of course, a family stands highest in the non professional life.

As a second important factor, I consider my services in the Swiss Mountain Infantry. I am by no means an army freak, but my country gave me the opportunity to study at one of the top world Research Institutes of Technology, even my tuition was waived. So I considered serving in the army as sort of paying something back. You see, the saying is: "Every country has an army, either one's own or someone's else's" or in other words independence or servitude.

S: Right. Like Japan.

R: And the Swiss were always quite particular about independence, since back to the 12 century. First Lieutenant, usually a platoon leader, was the rank I could reasonably do and afford to do without impairing my scientific career. The service consisted of four blocks of basic training of 17 weeks each according to my final rank at the beginning and annual or biannual repetition courses of 3 weeks each year or second year. I did the first block during the undergraduate years, two during my thesis work and one after my Ph.D. All in all that summed up to over 1000 days, my last service for three weeks in the November Mountains, a year after receiving the Nobel Prize. The services were physically tough at times, but again we had lots of fun in between. We made the best out of what we had to do in any case. I also considered the repetition courses as the most effective vacation time. Up in the mountains I was completely detached from all the professional obligations and returned usually with a cleared brain. But also in other respects I learned quite a lot, most importantly, it was good training for social competence, team work and more. You see again, living up to an obligation towards my country brought me much more than just a good conscience.

K: Very interesting, again serendipity. Now back to physics again. Did you have any influence from your teachers or your friends about experimental physics?

R: No.

K: No, you determined by yourself.

S: How about in undergraduate college, some teachers or professors were very close?

R: No.

S: You did not get any influence from anybody?

R: None of them inspired me really. I think I was a reasonable student but not a brilliant student. I would have liked to be more brilliant, like some of my colleagues.

Most of them became professors, some at the ETH, some at other very good Universities. At the starting of studying physics, we were about 12 physics students. After 4 years, only three of us finished in the minimum time. We were the ones who had to finish and make some money as graduate students as soon as possible.

K: So you were a brilliant student.

R: No, I think every one of the students could have made it if he had to do it. My friends became professors, me an ordinary research staff member. At that time, joining a company's Research Laboratory was rated considerably lower than pursuing an academic career.

K: You went to IBM at that time?

R: No, I first went to the USA. Staying in the USA for a while was considered very helpful for a scientist career.

S: As a Post-Doc?

R: Yes, as a Post-Doc at Rutgers University. Before we went to the United States, we married and the trip to United States became more or less our honeymoon trip. We took a freighter, the least expensive way to travel to the USA. That was one of the very few unfortunate decisions, because I was seasick most of the time. Not exactly what you expect of a honeymoon trip!!

At Rutgers, I measured the thermal conductivity in the mixed state of superconductors. During vacation, we usually went camping. Before returning to Switzerland we explored the USA on a three-month camping trip and arrived in Switzerland pretty broke – but with a contract from IBM in my pocket. A few days before sailing off, President Kennedy got murdered, which complicated our departure due to closed offices and official events. We had to return to Switzerland after two years for immigration reasons.

The same friend, Prof. Bruno Luethi, who brought me to the low temperature Laboratory of the ETH for the Ph.D. thesis convinced me to join the new and small IBM Research Laboratory with about 60 people. Actually, Bruno later became a Professor at Rutgers and then in Frankfurt A. M. and recommended Gerd Binnig, who set up the exercises for his course, very highly for a Research Staff Member position at the IBM Laboratory.

My employment procedure at IBM was very simple. I met the director of the Zurich Research Laboratory, Ambros Speiser, for an interview on one of his visits to the IBM Yorktown Research Center. No lecture, just meeting a few people, amongst them Leo Esaki. Two weeks later, I had the offer from IBM. In the same year, a couple of months earlier, Dr. Alex Müller (1987 Physics Nobel Laureate) also joined the Zurich Laboratory.

K: Oh, it was at the same time.

R: Yes. We joined the same year. So, this director of the Zurich Research Laboratory must have done quite a lot right.

S: Very successful. That is right.

K: And you had quite the freedom to research.
R: All the freedom I wanted.
K: Not confined.
R: No, as long it was scientifically sound and interesting. I started working on magneto resistance in very high, pulsed magnetic fields, which ended up in a series of experiments on the Kondo Effect. After some years I turned to phase diagrams. I took $GdAlO_3$, with a Neel temperature just below 4 K and with the whole phase space within 50 kOe. I used no longer pulsed fields. From a research staff member who left the company I "inherited" a superconducting magnet.

Then I changed to critical and multi-critical phenomena, still in $GdAlO_3$, which became a hot topic in science. I wanted to add NMR to my modest instrumentation methods of susceptibility measurement and alike. The University of California at Santa Barbara was a good place for that and IBM agreed to let me have a year sabbatical there in 1974/75.
K: This time, no cargo boat.
R: No cargo boat. In the last moment some problems with intellectual property rights came up. The University of California requested that every patent made on the University premises belonged to them. IBM on the other hand was obliged due to license agreements to license every invention by an IBM member to its license partners. A couple of days before the planned departure, they found a solution acceptable to all parts. We bought a car on the East Coast and camped our way across the country for about 5 weeks with our two daughters of 9 and 10, a unique experience for them. We stayed a year and then camped back from Santa Barbara to the East Coast. Work at the University was quite tough in the last 4 months, after all the experiments were set up. We investigated the critical properties of MnF_2. The liquid helium for the sc-Magnet of 120 kOe lasted about 13 hours per filling. That meant working until early morning, taking a real power nap and being back at the laboratory after lunch for setting up the next 13 hours round. My younger colleague, Dr. Alan King, got kind of worn out towards the end and was happy for more ordinary time schedule after our departure.
K: You came back to IBM in 1975 from Santa Barbara.
R: Yes. I continued for a while with multi-critical phenomena. This work received reasonable recognition in the community, but I did not see any long term perspectives for me. Also, the NMR methods did not work up to my expectations, so I would have to start with some other experimental methods, in a field where I thought the climax was already passed. One should leave a research area as long as it is still flying.

So I was looking for something else. I knew that nobody in the IBM Laboratories was pursuing transport phenomena in inhomogeneous systems, although that became an issue of general importance. The engineers at our place struggled with the homogeneity of very thin oxide layers for Josephson junctions, a hot topic for the next generation of supercomputers. But there was no coherent effort on the basic homogeneity issues.

Since in addition inhomogeneities played an ever more crucial role in the course of continued miniaturization, I thought that this should become both interesting and even very useful. IBM research management was very happy to see me start in something very different and new, although I had quite a reasonable record in critical phenomena.

I looked for a new research staff member and finally found Gerd Binnig.

K: So, you got to find Dr. Binnig as a coworker.

R: Yes. Tunneling came in, through the important problem of the Josephson tunnel junctions of our engineers. But growth mechanisms and quality of thin oxide layers were *en mode* in any case. I happen to listen to a lecture of Dr. Karl-Heinz Rieder, our surface science specialist on the growth of nickel oxide.

Gerd and I discussed how to solve the problem. Actually, we thought of buying a scanning Auger for about a million Swiss francs, and IBM agreed. But Gerd said "everybody can buy a scanning Auger. Let's take a more original and unique approach." And since tunneling through a very thin oxide layer was the problem guideline, "why not use tunneling in a way?" he continued. That is how the scanning tunneling microscope (STM) emerged. With the finest tips available in field emission of some hundred Angstrom radius of curvature, we estimated a resolution of 2-3 nm, as good as the scanning electron microscopes at that time, much better than a scanning Auger and having a direct access to the inhomogeneity of electron transport through the oxide layers, not just the surface structure.

Somehow we took the most complicated approach. Using a field emission tip means ultrahigh vacuum, of which we both were laymen. But Dr. Karl-Heinz Rieder helped us with abundant advice, which we, however, missed sometimes. We took an UHV chamber from another research staff member who abandoned physics. Even more of a problem was our background in low temperature physics and superconductivity. We had a fixed idea to work at low temperatures for doing tunneling spectroscopy and using sc-levitation for vibration damping. Low temperature and UHV is the most difficult approach you can take, in particular in the exploring stage. Whenever something did not work the way it should, e.g. the piezoelectric drive of the "mouse" for rough approach the system had to be warmed up, the faulty parts repaired, baked out for UHV, and cooled down. But also the wrong belief that UHV is needed in order to obtain credibility in the surface science community was not helpful. After close to two years Gerd had enough and decided to use a desiccator instead. He had abandoned the field emission tip already a while ago and instead used unprepared W tips sharpened on a "grinding" wheel. Ingenious of him, such a rough tip always has an atom at the end, and finally yielded atomic resolution. This was a year and a half of "frustration," in particular for Christoph Gerber, my long time associate, whom I assigned to work full time on the project and he did it with great enthusiasm. This gave Gerd and me the opportunity to work on our own transition projects. Gerd built himself a top loading dilution refrigerator and studied two band superconductivity in $SrTiO_3$: Nb, I myself continued with the random field problem in critical phenomena. Although our full involvement with the STM might have sped up its development somewhat, our so called "PRL" projects – Gerd published one on the two band uperconductivity of Nb doped $SrTiO_3$, I one on the random field critical point. – were quite helpful ammunition at management reviews and no questions asked and no exposure on the STM work.

The first successful experiment came soon afterwards, the exponential dependence of the tunnel current as a function of the tunnel gap, with an acceptable work function. Phys. Rev. Lett. did not appreciate the significance of this first quantitative experiment in an STM configuration and refused its publication. Such is life, we made it nevertheless a step higher than most accepted papers!

For obtaining atomic resolution you need an atomically sharp tip and proximity to the object, the resolution contains the geometry factor $(r+d)^{1/2}$ where r is the radius of curvature of the tip part closest to the object and d the tip-object distance – which has to be in the 1-2 nm range anyhow in order to have an easy and fast measurable tunnel current. The stethoscope of the medical doctor works according to the same principle.

K: Quite a different approach.

R: You are right. Unfortunately some of the American scientists did not see it this way and their strong lobby misled even the Nobel committee to a shaky citation: ".. for the design of the STM" and not " …for the invention and design…" But the Nobel committee made up for it by awarding E. Ruska (Fritz-Haber-Institute, Berlin) who designed the first electron microscope the prize together with us. Ruska died a year later. It would have been most regretful if one of the pioneering and most important instrumental developments of the 20^{th} century would have missed the recognition. It took over 50 years because of some intellectual property issues. Some high ranking

industrial R&D leader had embezzled the patent from the young student Ruska back in the beginning of the thirties. But these are tempi passati.

K: Getting tunneling. This is critical.

R: That is true. Tunneling by its exponential dependence on distance selects automatically the atom closest to the sample as the probe.

K: But how did you come across to the measurements of the micro-topography? First of all, you wanted to measure some tunneling current, but tunneling current measurement and topographic measurement are not equal.

R: In principle you are right, in practice, however, constant tunneling current means constant distance from the surface, at least in metals. Semiconductor surfaces are a bit more delicate, adsorbates can even mimic an indentation instead of a protrusion. That is then the domain of scanning tunneling spectroscopy.

K: From the beginning, you wanted to measure micro-topography of the surface or not in intention?

R: No. At the beginning, we were fixed on the properties of very thin oxide layers. But this turned out to be more involved. So, after a month or so talking back and forth, we realized, that by scanning the tip at constant current over a metal surface, we get the topography and had created a novel microscope, the scanning tunneling microscope (STM).

K: You had the idea of just making microscope.

R: To start with, yes. But Gerd made drawings for an internal physics presentation of combining Frankenstein's fingers and tunnel tip already shortly after the first successful experiments with the STM, anticipating the STM as a manipulation and modification tool on the atomic scale.

K: So, your idea of scanning of sporadic measurement came later. "Why don't you scan?" And by this scanning process, you found something like topography.
R: Yes, we did not come from the field of microscopy and so it took about a month. But when we set out to build the microscope, we used a three dimensional piezo- drive, both for rough and fine motion of the tip.
K: So, topography was not your intention at the beginning.
R: Serendipity.
K: The first intention was the measurement of the work function.
R: One of the first ones. Work functions are an interesting material property, but not the only ones.
K: And then why don't you scan to measure the work function? In the meantime, you found topographic data?
R: Who would not get carried away when seeing the first time surface structures with atomic resolution. Work function measurements were done soon later by many others. Priorities change.
K: Yes, I have seen that the Helvetica Physica Acta, you wrote a beautiful scheme.
R: Yes, that was a contribution to the 60th birthday of my thesis advisor, Prof. J. L. Olsen.
K: I remember your paper about the step height measurement of some $CaIrSn_4$.
R: This was the first surface topography with clearly resolved steps better than 1 nm. Since we had abandoned UHV for a while, we tried the very shiny Ir faces to please the surface science community. The success was not overwhelming.
K: But it was not atomic scale at that time.
R: Not yet. But the Si(111) 7x7 structure – actually already with the Au(110) 2x1 we had nice atomic resolution, but deferred its publication because of the first success on the Si(111) – changed all this with a bang. From a few relentless believers like Prof. Quate of Stanford, a refreshing STM community with a lot of youngsters started to grow rapidly and the opposition dwindled even more rapidly. Nevertheless, there still remained some die-hard reactionaries going as far as invoking the Heisenberg uncertainty principle as argument against STM.

Let me summarize some of the important points made in the interview, your "message for the younger generation" at the beginning the interview (but the older one is welcome to take it to heart too).

1) When you set out for something new, ask first: "what would change if I could do this." The best way not to get discouraged is to tell as few people as possible, e.g. we did not expose ourselves to the IBM management by making presentation. That

creates expectation pressure by others which prevent you from taking extraordinary approaches.

2) Do not pay too much attention to others, otherwise you remain on their level. Something is new and novel, because nobody thought it thinkable and doable, found it uninteresting or not important enough or did simply not think of it.

3) Be aware of the unforeseen developments. Your friend serendipity might turn up.

4) You can start anywhere and reach any place.

5) Use your young, unbiased mind, the greatest asset of youth, for the selection and solution of problems. And when you get older, you might be able to regain the lightness of being unbiased by starting something way beyond in what you have become an expert.

Interviewers: Prof. H. Komatsu and Mrs. M. Sakurai
in the sunroom of Dr. Rohrer's house, October 30, 2008

Dr. Rohrer with his friends (from left: M. Aono, H. Rohrer, K. Aono, M. Sakurai, & C. Quate) in front of his childhood house in Buchs.
The street was renamed after him a few years ago (this photo was taken in July 2006).

Interview with Dr. J. Georg BEDNORZ, 1987 Physics Nobel Laureate, Fellow, IBM Zurich Research Laboratory and International Advisory board member, WPI-AIMR

"Always be adventurous with full of curiosity!"

Prof. Komatsu (K): What made you decide to become a scientist? Is this by tradition in your family?

Dr. Bednorz (B): My father was a primary school teacher, and my mother was a piano teacher and therefore I guess I got some artistic blood in my veins. My sister and two brothers, in contrast, were more practical people. As a young boy at the age of five or six, I watched my brother repair motorcycles, but soon started to help him as an assistant. I liked to discover how technical things work and find out the reasons for their failure. This curiosity was preserved throughout my career. As a student, I learned to repair my cars and, for a long time, it became a hobby to reassemble a completely dismantled old-timer car. I was very satisfied and proud when the work was done, and the car worked perfectly and even passed the official vehicle inspection.

So very early on I gained some confidence in myself and lost my fear of diving into a field which I had not learnt from scratch or of starting a new activity and not knowing where it would lead.

K: What are your experiences from school time?

B: At secondary school, I enjoyed the activities in arts, like painting and sculpturing with wood and clay. This activity was carried out on a voluntary basis in late evening hours. We had an excellent teacher in arts, who was committed to nurturing creativity and guided us to discover new techniques, and encouraged us to explore our own ways when working with different materials. In retrospective, he was the person who had the greatest influence in fostering my desire to make things in a different way.

The second person who strongly influenced me was our chemistry teacher, who with exciting experiments fostered my interest for science. So my favorite topic at that time was chemistry. I even set up a chemistry laboratory in the basement of my parents' house.

K: You asked this to your father and your father prepared for you?

B: No, I only asked my father for his permission, and I did shopping of the equipment and bought the chemicals which I could not get from my classmates. I did many experiments with reactions I had read about in books, until the day when a mixture of chemicals I had prepared caught fire and ended up with a lot of smoke. I had to escape and close the door to wait until the reaction had completed – before I could return to extinguish the fire. After this experience, I closed down this private laboratory forever. But I got plenty of opportunities to continue – sometimes with explosions that were far from harmless – after I had started my studies in chemistry at the University of Münster in Germany. But after pursuing chemistry for a while, I made a major decision to change the topic, and joined the Institute of Crystallography. As always, it was the opportunity to discover new things and to test and explore my talents that excited me.

K: Who was your Professor?

B: There were two professors, both from the famous school of Professor Laves at the Swiss Federal Institute of Technology (ETH) in Zurich: Professor Wolfgang Hoffman was a crystallographer and Professor Hans Ulrich Bambauer was teaching Mineralogy. At the Institute, advanced students were given special tasks, such as either to provide assistance in exercises for younger students or help in the laboratory with real scientific experiments. I was lucky to work with a physicist, Dr. Horst Böhm, as supervisor, and was able to enjoy real everyday science projects, an interesting combination of chemistry and physics.

In 1972, after my pre-diploma examination, I was selected by Professor Hoffmann and Dr. Böhm to spend three months at the IBM Zurich Research Laboratory (ZRL) as a summer student. It was a real challenge for me to experience how the education I had obtained so far could be applied in the reality of an unknown environment. Could I really make use of what I learnt? But this was also a good opportunity to test how I would get along with new colleagues in a completely new world – and survive when exposed to new tasks. The decision to go to Switzerland set the course for my scientific future.

K: What was your impression when you joined the ZRL?

B: The physics department which I joined, was headed by Professor K. Alex Müller, whom I met with deep respect. I realized that I had come to a very special place with fascinating research projects and was very proud that soon I was accepted by my colleagues as a real member of the new community. And concerning my activities in everyday laboratory work, I was really impressed by the freedom even I, a student, was given to work on my own, to learn from mistakes and thus lose the fear of approaching new problems and solving them in my own way.

K: What in particular were your assignments?

B: I was working under the guidance of Hans Jörg Scheel, who was responsible for growing crystals for use in the solid-state physics projects in the department. He introduced me to the different methods of crystal growth, materials characterization, and solid-state chemistry for the synthesis of new materials. Concerning experiments, I was forced to jump into cold water because Hans J. Scheel was very busy writing a book about crystal growth from high-temperature solutions and therefore spent most of his time in his office. So I worked closely together with a technician, from whom I learned lots of technical tricks. Besides operating the X-ray analysis, my task was to grow single crystals using different methods. But soon I concentrated on flame fusion (Verneuil method using a H_2-O_2 burner, originally developed in Swiss) or the Czochralski method (pulling a crystal from the melt using a seed). For the latter work, the inductive heater system of the crystal puller was equipped with a huge RF generator, a truly impressive piece of equipment. I had never operated such a big machine before, and now I had to do it in own responsibility, which took some courage. But I said to myself, "OK, since everybody seems to have sufficient trust in me; I just try it." With the opportunity to make my own decisions, i.e., when to start the growth process or stop it if a possible failure was apparent, how to modify the growth parameters or even the experimental setup, I also got numerous chances to make mistakes. But I learned that mistakes made because of lack of experience – unless you make the same mistake twice – can and will be forgiven and can be regarded as an opportunity to learn from them. This three-month period at the ZRL helped build up my confidence and made me lose any fear of starting something new with unpredictable outcome. Thus, I think, this student's experience was decisive for the rest of my career.

K: How was this experience at the IBM Laboratory transferred to your later career at the university?

B: Back at the university, I started as a student assistant with the responsibility of designing and setting up new experiments for advanced student exercises. I found myself in a role in which the academic leader of the course gave me specific topics to prepare for the course. These topics ranged from the determination of the sound velocity in metals to optical experiments to study light propagation and interference effects and thermal analysis to determine phase diagrams as well as diverse crystal-growth experiments. Each task started with the study of the relevant literature, followed by the design of the experiment and the assembly of the appropriate equipment. Finally, prior to given them as an exercise to my classmates, I had to test the experiments by myself to make sure that they would lead to reliable results. For me, each of these experiments represented a miniature research project, allowing me to experience a new aspect in solid-state physics.

K: What about your further developments, besides these more educational activities?

B: After a second visit at ZRL in 1973, I was invited again for six months in 1974 to do the experimental part of my diploma thesis under the guidance of Hans J. Scheel.

K: On what material and subject?

B: This time the topic was very specific, namely "Crystal Growth and Characterization of Strontium Titanate $SrTiO_3$," for which I had to employ either the Flame Fusion or a modified Czochralski method (TSSG). At the ZRL physics department, perovskites were of interest for the study of structural, ferroelectric, and magnetic phase transitions. In particular, $SrTiO_3$ was the workhorse for investigations of transition-metal (TM) dopants in a perovskite host lattice. For his studies of local structural properties and optically induced charge-transfer processes, Alex Müller was very eager to get his own in-house supply of TM-doped crystals.

Soon after the start of the growth experiments, I realized that this task was a real challenge, and many failures created some doubts as to whether at the end I would be successful at all. No wonder that at that time there were only two suppliers of $SrTiO_3$ crystals in the world, a Japanese company (Nakazumi) producing jewelry and an American company (National Lead) as source of doped crystals.

K: But I know from meeting you 25 years ago, that at the end you were successful.

B: Yes, of course, but only because I pursued two alternate routes. The TSSG (Top Seeded Solution Growth) from non-stoichiometric melts turned out to be a complete disaster, although I did not give up the hope it would succeed until I had completed my thesis work. The growth temperature, thermal gradients and growth velocity were extremely hard to control. Tedious adjustments during the day, sometimes lasting until way after midnight, did not succeed in overcoming the problems. When returning to the laboratory in the morning, I found that usually either the seed crystal was gone or a big polycrystalline lump had formed, which meant that the experiments had failed. In the flame fusion experiment, the evaporation of strontium oxide (SrO), which continuously changed the composition in the melt, turned out to be the major problem. Together with Hans J. Scheel, I studied the phase diagrams, and by X-ray powder analysis of the melt compositions we were able to determine the exact SrO amount needed to compensate the evaporation losses. A few weeks before the end of my stay at ZRL, we had adjusted all the parameters to achieve reproducible growth of large and perfect single crystals. The day when the first successful experiment was done, Alex Müller, who within minutes had heard the news, got so excited that he immediately came running down to the laboratory to congratulate. After this successful completion the experiments, I returned to the University of Münster to write my thesis and to work as a scientific assistant.

K: But this wasn't the end or your relationship with IBM, wasn't it?
B: By no means. Alex Müller, having followed my work, encouraged me to continue my research on perovskites and offered me the opportunity of making my Ph.D. at the ETH in Zurich, with the support of IBM. So in 1977, I started my Ph.D. at the famous Solid State Physics Laboratory of the ETH with Alex Müller (at IBM) as one of my supervisors.
K: What was the topic of your Ph.D. work?
B: This time I worked on iso- and hetero-valent substitution in single-crystal solid solutions involving $SrTiO_3$, $CaTiO_3$, $BaTiO_3$ and $LaAlO_3$. During these studies, I gained my first experience in low-temperature experiments to determine structural and ferroelectric phase transitions in solid-solution perovskite crystals. I continued learning about the fascinating variety of properties of these compounds and how they change with doping.
K: That was Alex Müller's concern? Not yet related to high temperature superconductors?
B: Yes, this was exactly Alex Müller's traditional field of interest. And 1977 was long before the first ideas about high-T_c superconductivity came up. But interestingly enough, during my time at the ETH, I had a first encounter with superconductivity, which turned out to be a decisive experience for the future.

One day I got a phone call from Heinrich Rohrer at the ZRL, where he – as one of the managers in the physics department – had administrative responsibility for me as an IBM-sponsored Pre-Doc. Whenever I needed some materials that had to be bought, I sent the request to Heini. So one day, he called me and asked: "You are growing strontium titanate crystals; we have a new colleague here, Gerd Binnig, who would like to have a crystal doped with niobium. Can you provide that?" I said "Well, if Nature allows these crystals to be grown, I will provide them."
K: That means you didn't know the phase diagram at that time.
B: I knew the phase diagram of pure $SrTiO_3$ but I didn't know whether Nb would easily substitute Ti or whether upon doping impurity phases would be created. So I said "OK, in any case I'll try." And two days later, I brought the first crystal to the laboratory.
K: Really? How you could do that?
B: Well, the Flame Fusion method I used is fast method. Immediately after the phone call, I mixed the powders and started a three-hour growth experiment the next day. Two days after the phone call I met Gerd Binnig in his laboratory at IBM and, much to everybody's surprise, handed over the first single crystal. Gerd, who with Heini, would later become known for the development of the Scanning Tunneling Microscope (STM)

had earlier worked on superconducting SN_x and oxygen-deficient $SrTiO_3$, which was a superconductor at 0.3 K. He was curious to know whether the carrier density and the T_c could be enhanced by suitable doping. This handing-over of a Nb-doped $SrTiO_3$ crystal marked the beginning of a two-year collaboration. It soon turned out it was indeed possible to tune the T_c by varying the carrier density, and a maximum of 1.2 K was obtained for optimum doping. However, as from 1980 on, Gerd concentrated his efforts on the development of the STM, I lost my partner at IBM for continuing the superconductivity project. It was Alex Müller, who three years later, would step in and provide continuity.

K: How did this happen?

B: Well, as I said this took three years. Meanwhile, in 1982, I had joined the IBM Zurich Research Laboratory to work on both insulating and conducting oxides.

K: Aiming at what thin films?

B: The main goal was to produce a conducting material to study a metal-insulator transition. The first approach was a titanium oxide with lower oxygen stoichiometry, like Ti_4O_7, which was known to have a transition with a huge change in conductivity in going from polaron to bipolaron conductivity. It again was Alex Müller, who suggested to look at these conduction phenomena.

While learning to grow these thin films by sputter deposition, I was gently directed to combine this method with the STM as an analytical tool. The idea was to have complex vacuum system in which thin films could be grown, and in which, without breaking the vacuum, it would be possible to monitor the different stages of film formation with the atomic resolution provided by the STM. This was an excellent idea, but also a very ambitious goal, as it came at too early a stage of the STM development. I have to admit at that time it was bound to fail. So, no wonder that the project was not suitable to create satisfaction, neither for myself and nor for the management, and I came to the conclusion to look for a new project.

K: What did you have in mind?

B: I was lucky that there was an open position to strengthen the effort on new bulk materials. So I took the chance and jumped into the new project, with all the risks you take when you start something new. The safest approach, however, was in a first phase to continue the research on perovskites, which remained my main activity for a while. But when in 1983 Alex Müller approached me to ask whether I would be interested in a collaboration to search for new superconducting oxides with high T_c's, I immediately agreed – much to his surprise. It was the brief experience with the Nb-doped superconducting $SrTiO_3$ of course, which rendered my decision easy.

“Let s look at conducting perovskite oxides” he said.

K: Why oxides at all and in particular perovskites?

B: Alex has read the signs set by the existence of two other superconducting oxides with surprisingly high T_c’s of 13.7 K and 13 K, reported for $Li_{1+x}Ti_{2-x}O_4$ with spinell structure and for a $BaPb_{1-x}Bi_xO_3$ perovskite, respectively. The high values in these mixed-valent compounds were reached, despite a low carrier density of $n = 4\times10^{21}/cm^3$ and a comparatively low density of states $N(E_F)$ at the Fermi level. Therefore, according to BCS theory, the electron-phonon coupling constant should be large in these materials. We expected to find other metallic oxides in which even higher T_c’s could be reached by increasing the electron-phonon coupling by either polaron formation or introducing mixed valency or by increasing $N(E_F)$.

K: What made you confident that this could happen?

B: More than two decades of research on insulating oxides had established a significant expertise in structural and ferroelectric phase transitions at the ZRL. In particular by electron spin resonance experiments on TM impurities in perovskite host lattices ($SrTiO_3$ and $LaAlO_3$), substantial insight has been gained regarding the local symmetry of these crystals, i.e., the rotation and distortion of the TM-oxygen octahedra, the characteristic building units of the structure. Hereby, the insights gained from studies of isolated TM impurities, such as Ni^{3+}, Fe^{4+} or Cu^{2+} in insulating oxides, where 3d electrons are in partially filled e_g orbitals, and which exhibit a strong Jahn-Teller (JT) effect, i.e., a spontaneous distortion of the oxygen cage, were of particular importance for our concept.

K: So how did the concept look like?

B: There were essentially two components which we considered as the basis for our concept. A phase diagram proposed for polaronic contributions showed the existence of three phases in a plot of T versus the electron phonon coupling constant λ. At small λ a metallic phase would be stable down to low temperatures, whereas at large λ a metal-insulator transition to an insulating bipolaronic phase would occur with decreasing temperature. A high-T_c superconductor was expected to exist for intermediate λ.
K: Wasn't the idea about this polaron –bipolaron transition around, when you started with your Titanium Oxide thin films?
B: Yes, Alex didn't communicate this, but for me it is clear that when he suggested the Ti oxide project, he had the model of bipolaronic superconductivity in mind, which was proposed by the Grenoble group lead by Chakraverty. But as I had been redirected from a materials-based effort towards the STM-related project, a revival of the ideas became possible only after I had started a new materials project. Moreover, at that time, an important component for a concept to modify the electron-phonon interaction in a given material was still missing.
K: And this was available in 1983 when Alex Müller specifically asked you for collaboration?
B: Yes, at least it became apparent as a possibility. This second component was the Jahn-Teller (JT) polaron model, developed for intermetallic compounds, which directed us to a specific class of materials. The JT effect is known in the chemistry of complex compounds that contain specific TM ions with a special valence. A nonlinear molecule or a molecular complex exhibiting an electronic degeneracy will spontaneously distort to remove or reduce the degeneracy. The studies on a linear chain model for narrow-band intermetallic compounds showed that the presence of a small JT distortion with a stabilization energy E_{JT}, smaller than the bandwidth of the metal will only cause a slight perturbation for a moving electron. An increase of E_{JT} would enhance the probability of localization, and finally lead to the formation of JT polarons when the magnitude of the bandwidth is reached. In our opinion, the model could realize the phase diagram describing the effect of the variation in the electron phonon coupling and it should also be applicable to oxides if they were metallic.
K: So how did you start your search?
B: $LaNiO_3$ became our compound of choice to start our experimental effort because it is a metallic conductor and contains a JT ion. The high metallic bandwidth however suppresses the JT distortion. To reduce the metallic bandwidth to a magnitude comparable to the JT stabilization energy E_{JT} of Ni^{3+}, we started to partially replace Ni^{3+} by Al^{3+}. With increasing Al substitution, the metallic characteristics of $LaNiO_3$

gradually changed, resulting first in a general increase of the resistivity, and finally with high substitution leading to semiconducting behavior with a transition to localization. As it turned out, the idea did not work the way we had thought.

K: Did you consult the Goldschmidt's ion radius for selecting other elements?

B: Yes, we considered introducing internal strain into the host to modify the bandwidth by replacing La^{3+} by the smaller Y^{3+} with the Ni site remaining unaffected. Although the absolute value of the resistivity for the new solid solutions increased, the metallic temperature dependence was preserved; however the perovskite structure became unstable at high Y-concentrations. At that point, our concern that the target we were aiming at might not exist and that we had possibly embarked on a path which would end in a blind alley started to increase. The project entered a critical phase in 1985, when our optimism and energy had reached a low, and I frequently had to take a break, and only occasionally resumed my work on the subject. This can be regarded as a type of self-protection from continuous disappointment.

K: So you were not at all producing any valuable result during that time. How did your management react on this?

B: At the time when Alex and I decided to start this search, we also decided not to talk to anybody else about this.

K: Why that?

B: Well, to explain this, I have to describe a little bit the superconductivity research environment at that time. In 1983, the field of superconductivity research had already celebrated its 70th anniversary. Since the discovery of the phenomenon by Kammerlingh-Onnes in 1911, the materials efforts in the field had concentrated on the development of intermetallic compounds and on pushing their T_c's to the limit. All attempts, however, to further raise the transition temperatures beyond the record of 23.3 K obtained in 1973 had so far failed. The search for new superconductors had strongly been influenced by B. Matthias, who for decades knew how to guide his group in an intuitive way to discover numerous new compounds.

It was his firm belief that high-T_c superconductivity in the 25 K range, would, at best, be possible in a "relatively unstable intermetallic compound which is cubic." In accordance, the majority of specialists in the field categorically refused to take into account any new mechanism or exotic system as a serious approach to obtain novel superconductors. Indeed, the predictions of T_c's at 200 K or even at room temperature for materials exhibiting new mechanisms seemed to be far beyond the imagination of any experimentalist with a long history in superconductivity. In such an environment, our decision to start a search with new, for the community apparently exotic materials, was a high-risk enterprise. As there was no definite answer to the question whether only intermetallic compounds would show superconductivity close to the upper limit, which was thought to be 30 K, we had to accept that there was a fair chance of failing, including the risk of losing the scientific reputation.

K: But how could your effort remain secret?

B: To perform work on the superconductivity project was very easy because my main activity was to work on insulating oxides with structural and ferroelectric phase transitions. The equipment I needed for the synthesis and materials characterization was the same. The only difference was that the materials looked black instead of white. But nobody realized that I was suddenly starting on black materials.

One problem I had, however, was that I did not have a setup for measuring conductivity as a function of temperature. So I asked colleagues who studied the transport properties of semiconductors whether I could use their equipment. They said "OK, we are usually done with our experiments at 6 pm, so after that, you are free to use it." So for this "side" project, I always had to work at late evening hours to do the conductivity tests.

K: But at some time you were close to give up, you said.

B: Yes, somehow I was getting tired of having these disappointing experiences every evening, and I had to step back from the work at least temporarily, to gain some distance and rethink the problem. But having spent so much time on this idea, I found it hard to accept that the enterprise would probably find an inglorious end. Suddenly I realized that so far we had concentrated only on Ni as JT ion. I asked myself "Why didn't I consider other candidates, like Cu, to partially replace Ni?" So I started with what I though would be the last approach, but what would soon become a key experience….

Although the replacement of the JT Ni^{3+} by the non-JT Cu^{3+} did not cause structural problems, no significant change occurred in the transport properties as in all samples the metal-like temperature dependence of the resistance was preserved down to 4 K. Again there was no indication whatsoever for a transition to superconductivity. The time had come to study the literature and to reflect on the past.

It was in late 1985 when the turning point for the project was reached. While browsing through an issue of the Materials Research Bulletin one title immediately caught my eye: "The Oxygen Defect Perovskite $BaLa_4Cu_5O_{13.4}$, A Metallic Conductor," which reminded me of my last attempt. This compound containing Cu in two different valences (2+ and 3+) seemed to fulfill all requirements of our concept. A Ba replacement for La would be the means to fine-tune the ratio of the JT- distorted (Cu^{2+})

and non-distorted (Cu^{3+}) polyhedra without creating chemical disorder on the TM site of the crystal lattice. Electrified, I immediately went to the ground-floor laboratory to prepare a series of solid solutions with different Ba/La ratios. The next day the powder of the

new material was ready, and only had to be formed into a ceramic bar and measured.

K: What was the result of the resistivity test this time?

B: There were no measurements done at this point, because an unforeseen event had changed my schedule. I learned that I had been chosen to present my activity to the Director of Research, who had announced his visit for mid-December. He was known to be a critical person asking sharp questions and I got pretty nervous. While concentrating on the preparation of the presentation I neither had the time nor any intention to look at the new powder. So I left it on the shelf. The presentation of my main and official project went well as I could deduce from the reaction of our Director of Research. I presented examples of materials with tuned physical properties obtained through chemical substitution, but avoided talking about the adventurous search for superconductivity. However, I did conclude that, although the research on perovskites is a field with a long tradition at ZRL, the examples I had presented would show that this field was far from exhausted and that the perovskites were still good for surprises. After the presentation I needed one week to relax, not thinking about the superconductivity any more. Then came the Christmas period, I went on vacation.

K: But I guess after that you have started with new energy.

B: When I returned after the New Year, I first had to remember what I had planned when I had stopped. I recalled to have these materials on the shelf and decided. "Let me start the search for high-T_c superconductivity again." But first there came the routine work, pressing and sintering powders to make the ceramic samples and prepare the electrical contacts for the 4-probe resistivity measurements. Then came the first run of cooling the sample down – and it looked promising, because the resistance started to decrease like in a metal. But then I saw the resistivity was going up below 100 K, as I had seen it happen many times before. So I was mentally prepared to leave and go home with just another disappointment. Nevertheless I decided to complete the measurement down to 4 K. At 11 K, I didn't trust my eyes: suddenly the resistance started to decrease with a sharp drop and upon reaching 4 K, it had changed by 50%. Although for years I had been waiting for an event like this, it came to me as a shock. Suddenly it became hard to believe that our dream should have become reality. I immediately warmed the system again to repeat the measurement. It was already late in the evening, but I needed to have certainty. When I saw the effect coming back at the same temperature, with the same magnitude, I was convinced "This cannot be an artifact or a failure in the experimental setup, this is a real transition." Already at the first resistivity drop of only 50%, I intuitively felt "OK, you made it – this could be the breakthrough."

K: Intuitively you can have a big confidence, "Yes, this is it!"

B: Well, although the resistance did not drop to zero in the first sample, I felt that I had observed the first sign of superconductivity in this La-Ba-Cu-Oxide system. But gaining confidence is a different story. If you measure a new and unusual effect, it will require a series of experiments through which you will get your own experience regarding the stability, reproducibility and systematic trends that show up as a change in chemical composition to have trust in the results. Following the first measurement I started changing the compositions, i.e., the La/Ba ratio, and within two weeks, some samples showed a full transition to zero resistance with an onset starting as high as 35 K.

K: I see. I guess you were quite excited then.

B: Yes, indeed we were very excited because if our interpretation of a transition to the superconducting state was correct, this was an enormous jump as compared to the highest T_c of 23 K in the intermetallic compounds. And this occurred in materials that – in the view of superconductivity experts – would be regarded as too exotic to believe. On the other hand, Alex Müller and I had a lot of discussions in which we ourselves played the "devil's advocate" and tried to find all possible arguments against our interpretation. Our concern was that we observed the zero resistance state, but could not deliver the required second test for superconductivity, which is the measurement of the Meissner-Ochsenfeld effect. In the past numerous reports on high-T_c superconductivity had turned out to be irreproducible, in contrast to our transitions, which were reproducible with a systematic dependence on composition. We were convinced that we indeed did observe the onset of superconductivity typical of a granular material such as our multicomponent ceramics. So we decided to publish the results without the second test.

K: The Meissner-Ochsenfeld effect describes a transition from para – to diamagnetism when, under exposure to a magnetic field, a normal conductor goes to the superconducting state, right? Why didn't you perform the required measurements?

B: Simply because we did not have the equipment at the time. But with great foresight, when asked by the management at the end of 1985 what to do with some capital money which was left, we decided: "Let's buy a SQUID Magnetometer." So the order was placed, although there was no indication for a success in our search for superconductivity. It's funny (amazing) that shortly after that, in January 1986, the first resistive transition was measured. But it took a long time until the equipment was delivered, and only in September 1986 were we able to start the test for the Meissner Effect.

K: Your article which you submitted in April was published in September. What did you do in the meantime?

B: After our first article had been submitted for publication, we decided to enlist the help of a visitor from Japan. Dr. Masaaki Takashige, had been invited to ZRL by Alex Müller, and actually had arrived in February, only a few days after the discovery of the first resistive transition. His research interest was ferroelectricity, and I was supposed to support him and to introduce him to the method of growing thin films, in particular films of lead titanate. So in April, we urgently needed to increase the experimental effort for the synthesis of new compositions in the La-Ba-Cu-Oxide system, for electrical measurements and X-ray analysis. The latter was important, because our ceramic was a multiphase mixture and we needed to identify the pure superconducting compound. We had a first meeting to introduce him to our results.

K: How did he react when you told him that you had discovered Superconductivity in this new and unusual material at the incredibly high temperature of 35K?

B: Well, although he had no special experience in superconductivity I assume he more or less knew the state of the art in that area of research. His careful comments, displaying skepticism, told me that he did not feel very comfortable being asked to join this new effort, in which two "newcomers" in the field claimed that an exotic material could do better than the traditional intermetallic compounds. Somehow I understood his reaction, because he could not comprehend how confident we had gotten through the many experiments performed over the last months.

K: I see, he was feeling very uneasy.

B: That's the right description, but as soon he participated in the experiments, his confidence was growing rapidly. He was impressed "This is a very reliably effect."

In the following Alex Müller, Takashige and I had a lot of discussions addressing a key point, namely, which is the superconducting phase in our ceramic mixture. My favorite was a layered structure based on La_2CuO_4, a Ruddlesden Popper Phase, which

is derived from the 3D perovskite structure. Together with Takashige, I made annealing experiments under different conditions to find out what the superconducting phase was, until one day we had achieved a very sharp resistive transition. The X-ray data showed a pure phase, and Takashige also was convinced: "OK, now everything is clear. It is the layered cuprate with potassium nickel fluorite structure." Fortunately also in mid September, the SQUID magnetometer had arrived and we were now able to measure the Meissner effect. This pure sample exhibited the largest diamagnetic signal, so everything was clear. Within three weeks, we had collected the necessary data for the final proof of the existence of superconductivity, and we rushed to get the new results published.

K: This was in October, right?

B: Yes, on quite a memorable day. On October 14, 1986, we were just sitting together to make the final correction on our second paper "Susceptibility Measurements Support High T_c Superconductivity in the BaLaCuO System" which we intended to submit to Europhys. Lett. on that day. It was around noon, when we heard a loudspeaker announcement from the director's office to all employees, which said: "We pleased to make an important announcement: Heinrich Rohrer and Gerd Binnig just received Nobel Prize in Physics." People on the corridors were applauding and started to shout, and we rushed out to share their excitement and felt very proud to be colleagues of the two Laureates. But after a while, we came down to earth, and Alex seemed to be concerned: "I'm afraid, now Heini Rohrer will be gone for a while and we will not get any signature from him for the next couple of weeks."

K: A signature?

B: Yes, Heini was the Physics Department manager at that time, and we had to get his signature on the clearance form, prior to the submission of a paper. And this particular paper was urgent. I told Alex that I would find a solution, and called our receptionist to learn where in the building I could find Heini. The lady told me that he had been called to the Director's office, and I asked her to call me when he was coming back. When I heard that he was about to leave from the Director's office, I rushed to the reception with the clearance form ready and said: "Heini, congratulations on the Nobel Prize! Can I have your first signature as a Nobel Laureate?" He did not look at what he was signing, so he was giving his autograph on the form which we needed to submit our paper.

K: Heini was saying later, he handed the Nobel Prize to you.

B: Indeed his signature was important although he did not know what he was signing.

K: Funny story! It's interesting. Eventually I would like to have important references related to your findings, background and also French paper which gave you a hint.

B: The role this French paper played for the success of the project was indeed crucial. The resistance the French group had measured between +300 °C and – 100 °C was decreasing like in a metal and followed a straight line. In our search for superconductivity such a behavior was the main requirement for the choice of a compound to start with, before it was chemically modified. And the French material was a cuprate with no additional TM. I intuitively felt that working with Cu instead of a Cu-Ni mixture could be the solution.

K: Because you did so many experiments and failures, you know, that hint worked.

B: Well, I thought I had discovered a possible reason why all the experiments had failed so far. The lesson I took from the French article was, not to modify the center of the octahedron. A plausible way to go was to modify the Cu^{3+}/Cu^{2+} ratio by variation of the La/Ba ratio on the A site of the lattice. And we know by now, that the copper oxygen polyhedra play a key role in the cuprate superconductors and that whenever they are chemically modified by Ni, Zn or Al etc. the superconducting properties will be degraded. But having the key is only one part of the game, the other is finding the lock on the right door. And here we also had a bit of luck, which definitively helped to shorten the time of uncertainty. The way I prepared the new materials by wet chemistry immediately led us to the layered $(La/Ba)_2CuO_4$ with K_2NiO_4 structure and a La/Ba doping ratio that just hit the border of the stability range for superconductivity. So this was more or less the story of our discovery, and I would like to mention here a quote of Szent Gyorgi, the discoverer of vitamin C, who said. "A discovery is a lucky event meeting a prepared mind." I wonder what somebody else would have done with our first sample that showed such a strange resistance drop.

As we can only speculate about this, let's talk about something else.

K: So let's talk about your principles for your research work, is it confidence or something else?

B: My principles are manifold. First of all, I do not like to swim with the main stream. If necessary I'd rather take a certain risk, and even swim against it. I am stubborn enough and I can be persistent. The search for high-T_c superconductivity, I guess you will agree, is a good example for this. But it also explains why a few years after the discovery, when thousands of scientists in the world had begun to work on the cuprates, I started to feel uncomfortable to do what all others did.

I like to work on topics that have the potential of making a difference and giving rise to new scientific enterprises. For sure, with such a goal in mind, one has to accept that at the end not each project will be successful. When together with a Ph.D. student I looked for other materials systems and possible superconductors, we decided to investigate specific titanates and niobates. We developed a new method, which by just modifying the oxygen composition at a fixed cation ratio allowed us to produce and study 3D or 2D crystal structures that exhibited magnetic ordering, metallic or semiconducting behavior or ferroelectricity. We produced beautifully new superstructures constructed from semiconducting and ferroelectric structural subunits showing appealing properties. This work ended with the completion of the Ph.D. thesis, and did not cause too many people to raise their eyebrows – which is a pity, because these materials deserve closer investigation, which may come if the right ideas about applications appear.

What I learned during the years was that if somebody says "This is impossible." I should continue to ask myself "why not?" and "what's the reason to be that explicit. Isn't there a way around the problem and this apparent obstacle?"

K: So what are the criteria you follow when you start a new topic?

B: I try to get inspired by apparent fundamental limits or the limits of technology. Sometimes the engagement in technological problems can bring you to a completely different phenomenon with a new scientific challenge. As an example, I can describe how we arrived at another project which is related to nonvolatile memories, a field which I believe we here at ZRL were among the first to reactivate.

There is a long story behind this. In the microelectronic industry, people tried to improve the storage capacity of DRAM - Dynamic Random Access Memory. So far the only dielectric used was SiO_2, with a fairly low dielectric constant. People were looking for materials with a higher dielectric constant. I remember already in the 1980's we in IBM discussed about how to introduce higher-dielectric-constant materials for DRAM. At that time I told my colleagues that bulk $SrTiO_3$, has a dielectric constant ε of 300 at room temperature instead of 3.9. At that time the question was asked by people "How do you get strontium titanate into the deep trenches to make an efficient capacitor?" I did not know because at that time nobody was able to grow strontium titanate thin films. Even if this had been possible, nobody would consider the change of the trench technology as a realistic option. But soon after the discovery of high-Tc superconductivity, with the science community looking for materials with high structural perfection, new deposition methods were developed to grow oxides films. As important side effect – with the success to grow perfect superconductors – a new activity started to investigate thin films of dielectrics, ferroelectrics magnetic and metallic oxides. This whole class of perovskite oxides with its fascinating properties suddenly became available for a broad spectrum of applications. This technological advance was triggered by the high-T_c superconductors.

Suddenly the DRAM community was getting interested in barium strontium titanate with its much higher dielectric constant at room temperature. But in the process of preparing DRAM device structures, you need to anneal the entire assembly in argon hydrogen atmosphere. Everybody working with titanates knows that these compounds when annealed in such an atmosphere start to lose oxygen and start to conduct and will no longer be capacitors. As we also knew how to grow thin films, insulators, and conductors, we thought maybe we can contribute to sort out these leakage currents. During this project, I recalled my own Ph.D. work, in which I added chromium strontium titanate. Although through the reduction process, oxygen vacancies were introduced into the material, it remained insulating. We investigated the influence of an electric field on the behavior of thin Cr-doped films and realized that after a while the insulator became conducting. If we then scanned the voltage range between positive and negative voltage, the current would show a hysterisis. This was the discovery of a resistive memory effect in perovskite oxides. It is with great satisfaction that I realize how many research activities have been started that cover this topic in different materials. We however went back to work on strontium titanate thin films and single crystals. For decades $SrTiO_3$ has served as a model system for numerous types of phase transitions. Somehow, we are "back to the roots," and use it again as a model, but now with the aim of studying the electric-field-induced insulator to metal transition of the insulating state and the memory behavior when it is conducting. It is amazing, to see the revival of this material that has such a long tradition in solid-state research. It still is of importance, although the formulation of scientific questions has changed through new experiments related to surprising new applications. As I said, inspiration by limits of technology sometimes helps enter into new field of fundamental research.

K: So your principle is very interesting, "Don't necessarily go the same way."

B: Yes, I tend to get suspicious if I hear somebody say: "This is the only way to go." This tempts me to swim against the current. In my view, for a scientist it is one of the most valuable talents when he or she can develop a sense of whether or not it is appropriate to ask "why not?" when faced with claims that appear to be written in stone. In this way he or she might come up with a different answer.

K: This sort of spirit was already cultivated when you were young.

B: As a child, everybody starts out as a researcher. When starting to explore our environment, we were used to hear: "Don't do that" and our standard question would be "why not?" If given an answer, say "because it's hot," which we did not understand we were tempted to make our own painful experience in touching a hot plate – and we had learnt our lesson. In getting more mature, we have certainly learnt to be more careful but we should still see the world from our own and unbiased perspective. So if someone were to tell us "This is impossible" we should still ask "Why?" And in case the answer did not convince us, we should take the risk and try the impossible ourselves to sometimes find out that we can overcome the apparent barrier, as this will increase our self-confidence. I think it is important that this natural thirst for adventure is not only preserved, but cultivated by an appropriate education, beyond childhood.

K: Very interesting. We should be always adventurous and also keeping curiosity. Curiosity is very important.

B: Yes. Curiosity is what we are given in the first minutes of our life, and this precious talent should be guarded like jewelry for the rest of our life. It is curiosity that continuously leads us to explore our environment and the field of science, important for our continuous effort to further our understanding and for making new discoveries. Even if we do not see a solution, maybe we should dare to approach a problem with a certain naivety, and follow the famous popular song of *Reamonn* ………..this life is so complicated – until you see it through the eyes of a child.

K: Thank you very much for very exciting talk today

Interviewer: Prof. H. Komatsu
in Dr. Bednorz's Office at ZRL, October 31, 2008

Art work of Dr. G. Bednorz

Interview with Professor Tsuyoshi MASUMOTO, Person of Cultural Merit, The Japanese Government, Emeritus Professor of Tohoku University

The Basics of Amorphous Materials
— From Basic Studies to Practical Science —

Prof. Komatsu (K): You focused on the amorphous state while studying the mechanical properties of metals, and discovered its excellent strength and measured it. Through joint research, you also discovered that the magnetic properties and corrosion resistance far exceed those of traditional metallic materials. This led to the practical use of amorphous materials today.

We would like to know why you were interested in the amorphous state and how you have studied it, so that we can learn something about your creative ideas in materials development.

Prof. Masumoto (M): I read an interview with Professor Greer in Volume 4 of WPI-AIMR News. In fact, he participated in an international conference on amorphous metals that I held in Sendai in August 1981 (4th International Conference on Rapid Quenching Metals, Sendai, Civic Hall, August 24–28, 1981). He was from Cambridge University and still a graduate student. While traveling by the night train on the Joban Line from Ueno to Sendai, his cecitis developed into peritonitis, and he had to get off the train at Iwanuma Station as an emergency. I received the call from at night, but could not go there to help him because I was really busy preparing for the conference, so my wife took care of him. Finally, he had an operation in a hospital in Sendai. Because of this, he was deeply grateful to the many Japanese people who helped him for their kindness.

K: Oh, really? I never knew that. When I visited Professor Greer at Cambridge University last November, he was very kind to me. I guess that is one of the reasons he was such a help.

M: Dr. Yavari in France and Dr. Rao in India are also my good friends, who I came to know through amorphous studies. I have sent some of the young researchers from my laboratory to other countries on several occasions.

K: So exchanges among many people are necessary to establish a new field and gain international recognition? But what first made you interested in amorphous metals? And when was that?

M: In 1968, I happened to read Professor Pol Duwez's Campbell Memorial Lecture

[“Structure and Properties of Alloys Rapidly Quenched from the Liquid State”, *Transaction of the ASM,* **60**. 607-633(1967)]. He was a professor at the California Institute of Technology. I introduced his paper in the seminar of the laboratory, but was reprimanded by Professor Imai (1907-1988), probably because he felt that an associate professor specializing in the steel area should not get involved in studying nonferrous metals or alloys.

In those days, there were many student movements, with students acting violently all over Japan. Young associate professors like me were at the forefront of skirmishes with students, and in one of them, I got injured and had to have ten stitches in my head. So we could never concentrate on our research.

Then Professor Hiroshi Kimura who came back from U-Penn arranged me an opportunity to go and study as a visiting research scientist in the laboratory of Professor R. Maddin at the Department of Materials Science and Engineering of the University of Pennsylvania. He is a specialist in point defects. I was assigned the theme of studying the relation between creep and dislocation of nickel single crystals. I arrived at U-Penn in August 1969 and found there was no instrument ready for studying creep, which was essential for my studies, and was told that it would be delivered a few months later. So, I was allowed to study anything I liked and I decided to conduct experiments on amorphous metals, which I had been thinking of doing for a long time but had not been able to carry out in the Institute for Materials Research (IMR), Tohoku University.

In amorphous studies, Professor Duwez (1907-1984) focused on the physical properties. He was carrying out X-ray diffraction to study the structures of fine powders obtained by hitting molten metals against a cooling plate. To study their mechanical properties, which was my objective, first I needed to make an amorphous alloy with a fixed shape. So I invented an original device that used the filament centrifugal quenching method.

It was all handmade. I poured molten metal into a quartz pipe through a small hole of 0.6 mm or smaller at the top. The melt was spurted out onto the inside of a rapidly-rotating drum can, which turned into a ribbon-shaped filament. I scavenged the motor from a vacuum cleaner and used it for the device. The stock room of the Department of Materials Science and Engineering was really convenient for getting hold of the materials needed for experiments using handmade devices. Now I use a device based on the "single-roll method" which I devised later to make wide and long tapes. Metal is fused in a high-frequency furnace, and then the fused metal is spurted from a narrow quartz nozzle under gas pressure. Then the metal liquid is spread thinly on the surface of a rotating roll for cooling to be quenched and solidified. This method allowed me to make tapes continuously in large quantity.

A device for continuously producing thin belts which are about 20 cm wide and 0.1 mm thick was developed recently. You can now wind 25 cm-wide thin belts successively from 1 ton of fused metal, and you need only one single device to complete all the processes from fusion and casting to rolling.

At U-Penn, I first devised a device using the filament centrifugal quenching method. After two months of trial and error, I succeeded in making amorphous ribbon-shaped threads of palladium-silicon (Pa-Si) alloys for the first time. When I tried to bend it, they showed excellent elastic deformation, as well as high elastic moduli. I was surprised that one of them showed a tensile strength of 135 kg/mm^2 in a stretching experiment using an Instron. I made tapes of different thicknesses by changing the amount of fused metal and the rotating speed of the drum many times, while measuring the creep. Slip lines could be seen when they were bent, but when I tried to see the slip lines using an electron microscope, I couldn't focus to or see them. When it was heated, clear radiate dendrites appeared on the surface of the tape, indicating that the crystals came out from amorphous phase. But I could not understand to deformation mechanism. I proposed that it was by slips, but Professor Maddin thought it was due to small cracks. According to Professor Maddin, amorphous metals were unlikely to produce glide planes like crystals, so it could not be a slip mechanism.

Later, the concept of free volumes showed the reason for the deformation of amorphous metals, and I was right.

We published the results of experiments of first deformation with Maddin [T. Masumoto and R. Maddin, "The Mechanical Properties of Palladium 20 a/o Silicon Alloy Quenched from The Liquid State", *Acta Metallurgica,* **19**, 725-741 (1971)]. There's a picture of an interference fringe pattern in it, clearly showing a difference in level at the slip line. At that time, many people thought fused metals turned into aggregates of minute crystals rather than amorphous metals. I wanted to prove that they were amorphous metals. So I examined the size effect to prove that they were not size-dependent. I also presented data on deformation due to temperature increase, strain rate, creep, etc. to prove that they were not crystalline metals.

After returning to Japan, I sent my papers about the deformation of amorphous palladium-silicon alloys to Professor Maddin, together with the data and explanation, and published it [R. Maddin and T. Masumoto, "The deformation of amorphous palladium-20 at.% silicon", *Mater. Sci. Eng.*, **9**, 153-162 (1972)]. During my 14 months stay at U-Penn from 1969 to November1970 I published two papers, by creating amorphous alloys and studying their mechanical properties, measuring their electrical resistance, and characterizing them.

When I presented the results about amorphous metals at a conference, no one appeared to be interested. But when I presented them at the Kaya conference (a conference organized annually for commemoration of Professor Seiji Kaya, ex-president of Tokyo University) Professor Kaya appreciated them and said to me that they were interesting. In 1971, I received the Mainichi Newspaper Incentive Research Award through the recommendation of the Science Council of Japan. My project was one of only seven projects to receive the award out of the 222 projects. Later, Professor Imai also helped publicize my results. To turn these basic studies to practical use, amorphous alloys need to be made mainly of iron, instead of precious metals like palladium. So we made amorphous ribbons using iron-phosphorous-carbon (Fe-P-C) alloys, which showed a tensile strength of 350 kg/mm^2. This is three to four times as strong as piano wire.

As well as strength, I studied magnetism jointly with Professor Hiroyasu Fujimori, and showed that amorphous alloys have better soft magnetism than ordinary crystalline alloys. Amorphous alloys have excellent properties such as low coercive force and high permeability. Therefore, by using amorphous alloys for the iron cores of transformers, iron losses can be reduced to one fifth compared with ordinary silicon steel, resulting in much less energy being lost as heat. In other words, it was proven that magnetic materials made of amorphous alloys can save energy. Today, iron cores made of amorphous alloys are widely used, and many alloys are also being used as magnetic materials for high frequency, because they have high electrical resistance. As for chemical corrosion resistance, we found, through joint research with Professor Koji Hashimoto, amorphous alloys containing a small amount of chromium, such as Fe-Cr-P-C, are one million times as acid-resistant as 18-8 stainless steel.

I first became interested in amorphous alloys because I was interested in them as structural materials. As our studies proceeded, however, we learned that amorphous alloys had excellent properties and I became interested in them as functional materials.

Such progress would not have been possible without the good tradition and environment of the Institute for Materials Research where barriers between different specialties were very low and joint research projects could be promptly carried out.

K: I hear there was an argument with the U.S. about the patent for amorphous materials. What is the truth?

M: The U.S. has a first-to-invent system. But after we gave them an explanation together with our data, Japan won the case.

Unfortunately, Japan was under pressure from U.S. trade policies at that time. In March 1990, Allied of the U.S. filed a suit under Section 301 of the Trade Act (provisions about sanctions against unfair trade practices). Amorphous metals were on the list, and Japanese companies were put under pressure to import amorphous products from the U.S. The leading article of the Nikkei of September 23, 1990 also referred to the developments of the case.

It is true that Professor Pol Dewez was the first person to pay attention to amorphous metals, and his work is just like "Bible" to me even now. I found a way to fabricate

them into workable materials. To that end, making amorphous alloys with desired shapes was essential. I succeeded in making filament-shaped amorphous alloys for the first time in the world using the handmade device. Thanks to the single-roll method developed later, 100,000 tons of transformer iron cores made of amorphous iron-silicon-boron alloys are produced every year in Japan, accounting for 60 to 70% of total production in the world.

Amorphous metals, with various properties besides their three most important properties of mechanical strength, soft magnetism, and high corrosion resistance, have unknown potential. It usually takes a long time to put new functional materials into practical use. But further developments are expected to emerge in future.

For example, amorphous metals which can be processed like processed glass are attracting much attention. We have discovered since 1988 a variety of metal alloys with wide supercooled liquid regions.

K: It was fascinating to hear your explanation of the processes from discovery to practical use of amorphous materials, including the offstage ones which I knew nothing about from reading your theses. Thank you very much for this interesting interview, which will hopefully stimulate young researchers.

A list of references follows:

(Mg alloy)

・A. Inoue, M. Kohinata, A.P. Tsai and T. Masumoto, "Mg-Ni-La Amorphous Alloys with Wide Supercooled Liquid Region", *Mater. Trans. JIM.*, **30**, 378-381 (1989).

(Al alloy)

・A. Inoue, T. Zhang and T. Masumoto, "Al-La-Ni Amorphous Alloys with a Wide Supercooled Liquid Region" *Mater. Trans. JIM.*, **30**, 965-972 (1989).

(La alloy)

・A. Inoue, K. Kita, T. Zhang and T. Masumoto, "An Amorphous $La_{55}Al_{25}Ni_{20}$ Alloy Prepared by Water Quenching" *Mater. Trans. JIM.,* **30**, 722-725 (1989).

(Zr alloy)

・A. Inoue, T. Zhang and T. Masumoto, "Zr-Al-Ni Amorphous Alloys High Glass-Transition Temperature and Significant Supercooled Liquid Region" *Mater. Trans. JIM.*, **31**, 117-183 (1990).

Interviewer: Prof. H. Komatsu
at Director's Office, Research Institute for Electric and Magnetic Materials, January 21, 2009

文化功労者　東北大学名誉教授　増本（ますもと） 健（つよし） 先生にきく

アモルファス材料ことはじめ
― 基礎研究から実学へ ―

小松：増本健先生は金属の機械的性質を研究する過程でアモルファス状態に着目され、世界で最初にそのずば抜けた強度を見出して実測されました。さらにアモルファス金属の磁性と耐食性が、これまでの金属材料とはケタ違いに優れていることも共同研究で発見され、今日のアモルファス材料の実用化の先鞭をつけられました。

今日は先生がアモルファス状態に興味を持った動機、研究方法などを想い出していただき、独創的な材料開発のヒントを得たいと思います。

増本：WPI-AIMR News 第 4 巻の Greer さんのインタビューを読みました。実は 1981 年 8 月に仙台で私が開催したアモルファス金属の国際会議（4^{th} International Conference on Rapid Quenching Metals, Sendai, Civic Hall, 1981-8.24～28）に、彼はケンブリッジ大学から参加しました。まだ大学院生でした。常磐線の夜行列車で上野から仙台に来る途中、盲腸炎をこじらせて腹膜炎になり、夜中に岩沼駅で急患として下車し、夜中に電話があったのです。私は会の主催で動けなかったので、私の家内が世話をしました。最後は仙台の病院で手術をうけました。そのような次第で彼は世話になった何人もの日本人の親切に感謝していました。

小松：ええ！そうだったのですか、そんなエピソードがあったことを全く知りませんでした。昨年 11 月にケンブリッジ大学に Greer 先生をお訪ねした折は、大へん親切にしていただきました。今日、その理由の一端がわかった気がします。

増本：フランスの Yavari さんやインドの Rao さんらも古くからのアモルファス研究の知人です。私の研究室の若手を外国にたびたび派遣しました。

小松：なるほど。一つの新分野が確立し、国際的に認められるその裏には大勢の人々との交流があるのですね。ところで増本先生がアモルファス金属に興味を持たれた動機は何だったのですか？それはいつ頃のことでしょうか？

増本：それはカリフォルニア工科大学の教授だった Pol Duwez（1907-1984）の論文 [Pol Duwez, “Structure and Properties of Alloys Rapidly Quenched from the Liquid State”, *Transaction of the ASM*, **60**, 607-633(1967)] に、1968 年に偶々出会ったことからです。この論文を研究室の雑誌会で紹介したら、今井勇之進先生（1907-1988）にしかられました。鉄鋼部門の助教授が非鉄金属や合金の研究に迷いこむことはないという先生の信念からでしょう。

当時は学生運動が盛んで、全国的に大学で学生の暴力沙汰が起きていました。私たち若手の助教授は学生との小ぜりあいの最前線に立たされました。そこで私は頭を 10 針も縫うケガをしました。そんな状況で、落ちついて研究をすることからはほど遠い雰囲気でした。

そのような折、金研の木村宏教授からご紹介があって、ペンシルバニア大学材料科学部の R. Maddin 教授のところへ客員研究員として留学することになりました。

Maddin 教授は点欠陥の専門家です。与えられたテーマはニッケル単結晶のクリープと転位の関連を調べることでした。1969 年 8 月にペンシルバニア大学へ行ってみると、肝心のクリープの装置は無くて、数ヵ月後にはいるとのことでした。それまで好きな研究をしてよいことになったので、金研ではやれなくて、ずっと暖めてきていたアモルファス金属の実験をすることに決めました。

Duwez さんのアモルファスの研究は物性を調べることが中心でした。溶融金属を冷却盤にぶっつけて得られる微粉末の構造を、X 線回折で調べていました。私の目的とする機械的性質を調べるためには形状一定のアモルファス合金をつくることが先決です。そこで、独自に「フィラメント遠心急冷法」装置を考案しました。全て手作りです。先端に 0.6mm 以下の小孔をつけた石英管の中に溶融金属を入れ、高速回転する缶詰かんのドラムの内側に融液を噴き出させて、リボン状フィラメントを得ました。そのモーターには掃除機のモーターをはずして使いました。材料科学部の部品のストアーはなんでもあって、手作りで実験をするための材料を得るのに大へん助けになりました。

今では、その後に考案した「単ロール法」の装置を使って幅の広い長いテープをつくっています。これは高周波炉で金属を溶解し、細い石英ノズルからガス圧で噴出させ、回転する冷却用のロール表面上に金属液体を薄く引きのばして急冷凝固する方法です。これで連続的に大量にテープをつくることが出来るようになりました。

最近では、幅 20cm、厚さ 0.1mm 程度の薄帯を連続製造する装置も開発されています。1 トンの溶融金属を 25cm 幅の薄帯にして連続的に巻き取ることができます。これで溶解―鋳造―圧延の全行程を 1 つの装置でできるようになりました。

ペンシルバニア大学ではじめた研究は、最初に「フィラメント遠心急冷法」装置を考案し、試行錯誤のうえ 2 ヶ月かかって、はじめてパラジューム―シリコン（Pa-Si）合金の非晶質リボン状細線を作ることに成功しました。これを曲げるとよく弾性変形し、弾性率も大きく、インストロンの引張り試験で、135kg/mm^{2}の引張強さが出て、驚きました。融液量とドラムの回転速度を様々に変えて、いろいろな厚さのテープを作り、クリープの測定もやりました。曲げると、すべり線

が出ました。しかし、電子顕微鏡ですべり線を見ようとしても焦点が合わなくて、見ることができませんでした。加熱するときれいなデンドライトがテープの表面に放射状に出ました。アモルファスから結晶が出たわけです。しかし、変形の理由は不明でした。私は「スリップ」と主張したのですが、Maddin 教授は「クラック」によると考えていました。教授の理由はアモルファスは結晶のようなスベリ面は期待できないからスリップではあり得ないということでした。

その後アモルファス金属の変形は自由体積という考えなどを使って次第に明らかにされてきています。

最初の変形の実験結果は Masumoto and Maddin の連名で Acta Metallurgica に発表しました[T. Masumoto and R. Maddin, "The Mechanical Properties of Palladium 20 a/o Silicon Alloy Quenched from The Liquid State", *Acta Metallurgica,* **19**, 725-741 (1971)]。その中に載せてある干渉縞による写真を見ても、スリップラインのところで段差が生じていることは明らかです。当時は溶融金属はアモルファスにはならないで、微細結晶の集合体になると考える人が大勢でした。私はアモルファスであることを実証しようとして、サイズ効果を調べて、サイズ依存性が無いことや、昇温による変化、歪速度とクリープなどで、結晶性の金属ではないことをデータとして出しました。「パラジユウム―シリコンのアモルファス合金の変形」の論文は帰国後、Maddin 教授にデータと説明をつけて送り、連名で発表しました[R. Maddin and T. Masumoto, "The deformation of amorphous palladium-20 at.% silicon", *Mater. Sci. Eng.*, **9**, 153-162 (1972)]。

1969.8〜1970.11 の 1 年 2 ヵ月の留学期間中に、アモルファス合金の作成から機械的性質や電気抵抗の測定とキャラクタリゼイションをして、論文を 2 つ出したことになります。

帰国後、アモルファス金属のことを学会で発表しても、関心をもたれませんでした。ただ茅コンファレンスで発表したら茅誠司先生（1898-1988, 日本学術会議会長もつとめた元東大総長）に面白い研究だと評価していただき、1971 年、日本学術会議の推薦で、毎日学術奨励賞を受けました。222 件の応募の中から選ばれて 7 件受賞したうちの一つです。その後、今井先生も成果を広めて下さいました。

このような基礎研究が実用化されるためには、パラジウムのような貴金属でなく、鉄を主体にしたアモルファス合金が必要です。そこで私たちは鉄―リン―炭素（Fe-P-C）系の合金でアモルファスリボンを作り、引張強さ 350kg/mm² を得ました。これはピアノ線の 3〜4 倍の強さになります。

強さだけでなく、磁性の研究も金研の藤森啓安教授と共同研究の結果、通常の結晶合金よりもアモルファス合金の方が優れた軟磁性をもつことを明らかにしました。低い保磁力、高い透磁率など優れた特性をアモルファス合金は持っ

ています。したがってこれらのアモルファス合金をトランスの鉄芯に使うと鉄損が通常のケイ素鋼の 5 分の 1 以下になり、熱として失われるエネルギーがぐんと小さくなる。即ちアモルファス合金の磁性材料は省エネ材料になり得ることが示されました。現在アモルファス合金の鉄心は広く使われています。さらに電気抵抗が高いので高周波用の磁性材料としてすでに大量に使用されています。化学的耐食性については金研の橋本功二教授との共同研究で、クロムを少し加えた Fe-Cr-P-C 等のアモルファス合金は 18-8 ステンレス鋼の 100 万倍の耐酸性をもつことが見出されました。

アモルファス合金の特性に関して、研究のとっかかりは「構造材料」としての興味でしたが、研究が進展するにつれて、アモルファス合金が優れた機能を持つことがわかりはじめ、「機能材料」への関心が高まりました。

このような進展は異種専門間の壁が薄く、共同研究がすぐに行われる金研の伝統とよい雰囲気があったからです。

小松：アモルファス材料の特許で米国と論争があったとおききしたのですが、真相はどうなんでしょう。

増本：米国は先発明主義なので、私たちのデータを持ってゆき説明することで、日本側が勝訴しました。

ただ残念なことに、米国の通商上の政策で当時は日本たたきがありました。1990 年 3 月に米国のアライド社が米通商法 301 条（不公正貿易慣行に対する制裁条項）に基づき提訴してきたことで、アモルファスもその対象の一つに組み入れられ、米国のアモルファスの製品を日本が輸入するように圧力をかけられました。1990 年 9 月 23 日の日本経済新聞の社説でもその間の事情がとりあげられています。

Pol Dewez 教授が最初にアモルファス金属に注目されたのは確かなことで、今でもバイブルです。私はそれを材料化する糸口を見出したのです。それには一定の形をもったアモルファス合金が必須でした。私は手作りの装置で世界で最初にフィラメント状のアモルファス合金を得ることに成功しました。その後開発した「単ロール法」で、今では鉄―ケイ素―ボロン系アモルファス合金で年間 10 万トンのトランス鉄心が日本で作られています。これは世界の 7～6 割に当たります。

アモルファス金属は機械強度、軟磁性、超耐食性の 3 大特性以外に様々な特性を備えているので未知の可能性を秘めた材料といえます。新機能材料の実用化までには時間がかかります。これからも新展開が期待されます。

たとえば最近では加工がガラスのように可能なアモルファス金属が話題になっています。これも私たちは 1988 年から過冷却域の大きい金属合金を多種発見してきています。ご参考のために文献*を上げておきましょう。

小松：今日は、アモルファス材料の発見から実用までにわたって、論文では知ることができない舞台裏のお話までおききすることができて、わくわくしました。若手の研究者に刺激を与える興味深いお話に感謝致します。

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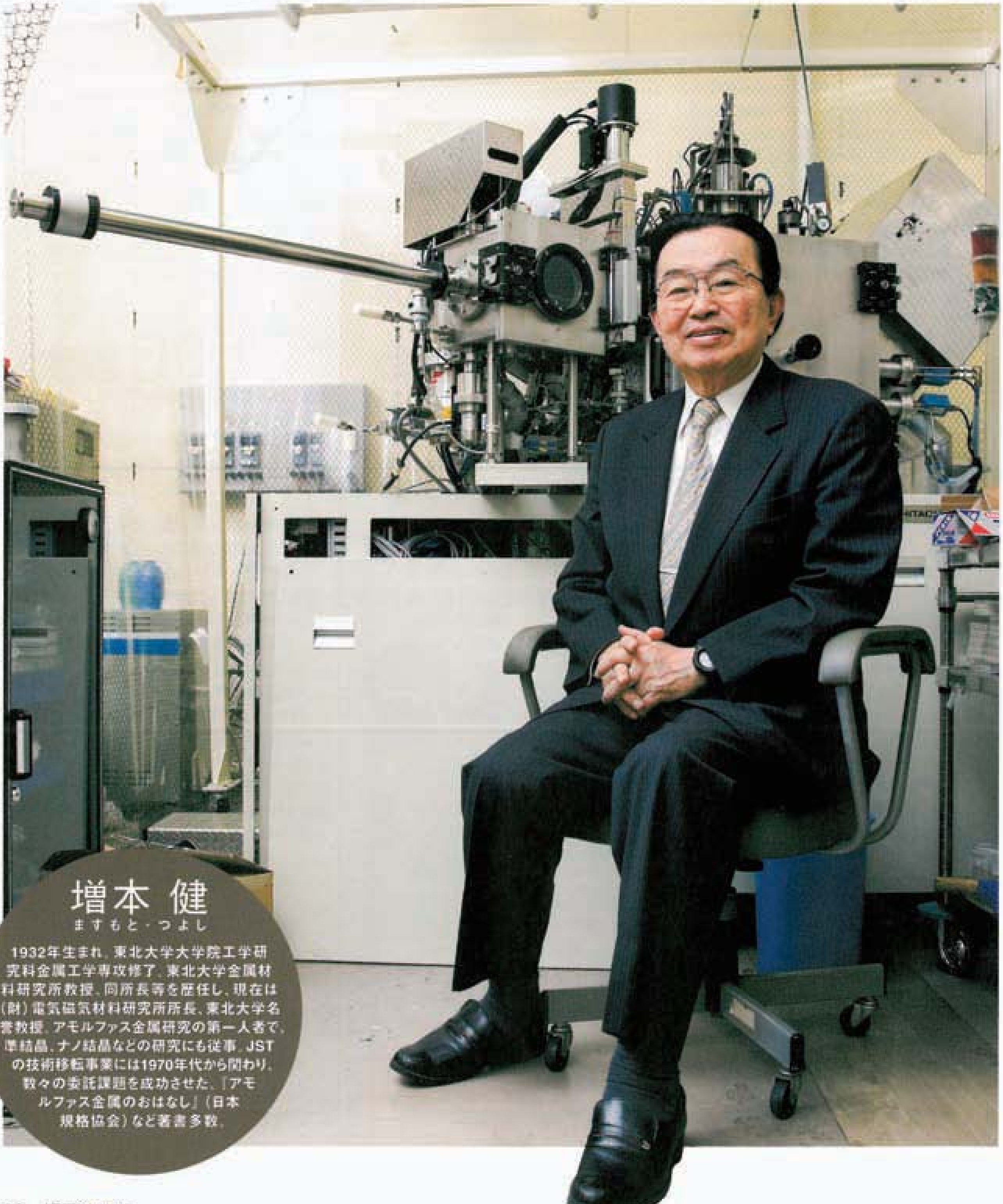
2009 年 1 月 21 日

電気磁気材料研究所　所長室にて

ひろし
小松　啓

Close up 増本健が語る「技術移転」の50年

実用材料をつ



増本 健
ますもと・つよし

1932年生まれ。東北大学大学院工学研究科金属工学専攻修了。東北大学金属材料研究所教授、同所長等を歴任し、現在は（財）電気磁気材料研究所所長、東北大学名誉教授。アモルファス金属研究の第一人者で、準結晶、ナノ結晶などの研究にも従事。JSTの技術移転事業には1970年代から関わり、数々の委託課題を成功させた。『アモルファス金属のおはなし』（日本規格協会）など著書多数。

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くる！

大学などの研究成果を企業へと橋渡しするJSTの「技術移転」事業が、
今年で50周年を迎える。
アモルファス金属を中心に、この事業に深くかかわってきた先駆者の言葉から、
今では当たり前になった技術移転の意義を、改めてかみしめたい。

"失望と反省"の時代から"再起と希望"の時代へ。

1960年〜70年ころ

憧れの「金研」に入るも設備と装置の古さに失望。

「金研（東北大学金属材料研究所）で材料工学を研究したい」――少年の頃からそんな夢を抱き、希望に胸をふくらませて研究者になった増本青年だったが、夢をかなえた先に待っていたのは失望だった。

私が材料工学を研究したいと思ったのは、父（増本量〈はかる〉・同研究所第6代所長）の師だった金研の初代所長・本多光太郎先生への憧れからです。父から、「金研にはこんなすごい先生がいる」と繰り返し聞かされ、小学生の頃から研究者になると決めていました。一途に夢を追って東北大学工学部に入学し、念願の金研で大学院生活を送った後、1960年からは特殊鋼部門で助手として働くことができました。

ところが、研究を進めるうちに、私の心には失望が広がっていきました。設備も装置も古く、貧しい。こんな環境でいい研究ができるはずがないという疑念が募っていったのです。与えられた研究テーマも、欧米の研究を追いかけたものばかりで興味を持てませんでした。決定的だったのは、日本の鉄鋼メーカーが相次いで基礎研究所を設立したことです。当時、日本は粗鋼生産が1億トンを越え、世界一の鉄鋼国となっていました。勢いのある企業の研究所には最新の設備がそろい、優秀な人材が集まって大規模な研究を始めます。これではまったく太刀打ちできない、何か新しい研究に取り組みたいと考えはじめました。

たまたま読んだ雑誌で「アモルファス金属」と出合う。

失望の淵で出合ったのが「アモルファス金属」。金属は結晶構造をしているという常識に反し、原子が不規則に並んでいるというこの不思議な物質の魅力に、急速に惹かれていった。

たまたま読んだ雑誌で、アメリカの研究者の論文に目を留めたのがきっかけです。アモルファス構造をしているのは有機材料やガラスで、金属は結晶であるというのが常識でしたから、容易には信じられませんでした。でも、「もしもあるとすればおもしろい！」と直感したのです。

材料は、安定した強度と形状を保たせることを目的にした構造材料と、それ自身が高度な機能を示す機能材料に分類できます。私のそれまでの研究は構造材料でしたが、アモルファス金属との出合いをきっかけに、機能材料の研究へ転換したいと考えるようになりました。

アメリカへの留学が研究者生活の転機に。

ちょうど学生運動の全盛期であり、金研も学生たちに妨害され、満足に研究生活ができない日々が続く。しかし、それがかえって、大きな転換を実現する契機となった。アメリカへの留学だ。

当時、私は助教授で若かったので、「大学本部を奪回しろ」と学生との対立の最前線に立たされていました。そんななか、学生との衝突で大怪我をしてしまいます。それをきっかけに、「このままここにいても仕方がない」との意を強くしました。そして、1年間、アメリカのペンシルバニア大学材料科学科の客員研究員として、留学させてもらえることになったのです。

同大学では当時、アモルファス金属の研究は行っていなかったのですが、「好きな研究に取り組んでいい」と言われ、いよいよ念願の研究に取り組みはじめました。それが私にとって、本当の意味での研究者生活のスタートとなりました。

JST技術移転事業 50年の歩み

湯川秀樹がノーベル物理学賞を受賞した昭和24年、わが国の経済再建のため、科学技術の産業への導入推進が叫ばれ、新技術開発機構設立の構想が生まれる。

1958（昭和33年）
新技術開発事業団の前身、理化学研究所開発部が誕生。昭和36年に新技術開発事業団が設立されるまで、国産技術の委託開発業務を実施

1959（昭和34年）
★人工水晶の製造［研究者:国富稔・滝貞男（山梨大学）/委託企業:東洋通信機㈱］

注）以下、★印は「委託開発」成功事例、カッコ内は［研究者/委託企業］を示す。

1961（昭和36年）
新技術開発事業団発足。委託開発および開発あっせんを柱とする新技術開発事業を開始

1963（昭和38年）
★地熱発電用蒸気の生産技術［早川正巳・中村久由（工業技術院地質調査所）/日本重化学工業㈱］

1967（昭和42年）
開発あっせん事業、国有特許あっせん業務の開始

1973（昭和48年）
特許指導・助言サービス事業を発足

1977（昭和52年）
★電磁材料用アモルファス金属の製造技術およびアモルファス金属の応用技術［増本健（東北大学）/㈱日立製作所、日立金属㈱、松下電器産業㈱（現パナソニック㈱）、ソニー㈱］

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"実学への挑戦"の時代。 1970年～90年ごろ

「実学」を求める血が騒ぎ出す。

帰国してすぐ、1971年に教授となり、アモルファス金属の研究を進めた結果、興味深い特性を次々と発見。それをきっかけに、金研の伝統である「実学」へ挑戦したいという思いが頭をもたげてきた。

数年で、「強度が高い」「錆びにくい（耐食性）」「磁化しやすい（軟磁性）」という、アモルファス金属の３大特性を発見できました。しかも、アメリカで研究していた貴金属ではなく、鉄の合金で確認できたのです。これは、非常に重要なことでした。

もともと、金研には「実学」の伝統があります。「学問は、使えるという実証がなければ価値がない」という本多先生の信念から生まれたもので、積極的に誘致などを行った結果、戦前には研究所周辺に10社ほどの企業があったと聞きます。ところが戦後になると、そんな伝統に対し、企業との癒着につながると敵視する声が高くなってきました。学生運動で金研が標的にされたのも、そのためです。こうした声に押されて、伝統は薄れ、実学など許されない雰囲気になっていました。今では考えられませんが、当時は金研に限らず、全国的に企業と接触するのが難しく、大学で特許を取ることもできなかったのです。

しかし、本多先生の信念を父から繰り返し聞かされていた私には、実学を求める血が流れていました。アモルファス金属も、最初は純粋な学問的な興味から研究を始めたのですが、研究を進めるうちに、そんな血が呼び覚まされました。

帰国してすぐ、鉄の合金の研究に着手したのも実用化への思いからです。３大特性を発見した次のテーマも、やはり実用化には欠かせない大量生産でした。研究の結果、液体急冷却法による薄帯、細線、粉末の製法の原理を確立でき、所内・所外との共同研究、さらにはアモルファス材料研究会の設立と、研究の輪を広げていきました。

アモルファス金属の「懐の深さ」が実用化の幅を広げた。

実学への挑戦は、JSTの前身である新技術開発事業団の技術移転事業などを通じて次々と実を結んでいく。アメリカとの競争など、実用化への展開の難しさも認識しながら、確かな実績を残した。

アモルファス金属の実用化に手応えを感じていたとはいえ、本当にそれが企業にとって魅力的なのか、自信がありませんでした。そんなとき背中を押してくれたのが、新技術開発事業団の担当者です。

私の講演を聞いて興味を持ち、研究所まで訪ねて来て、「おもしろいから、ぜひ、委託開発課題に応募しましょう！」と熱心に誘ってくれました。少しでも反応があればいいなと思っていたところ、30社以上から申し込みがあり、驚きました。選ばれた４社と1978年から開発に取り組んだのですが、刺激的でしたね。会議で意見を出し合うのはもちろん、基礎実験も企業が行い、実用化にはこういう視点が必要なのかと勉強になることがたくさんありました。

こうした研究の結果、実用化への道が見えてきた矢先に、思わぬ障害が現れました。

1979（昭和54年）
開発あっせん事業、有用特許取得費を新設
1980（昭和55年）
開発あっせん事業、技術加工費を新設
★携帯型電子走査超音波診断装置［伊藤健一（東京農工大学）/アロカ㈱］
1982（昭和57年）
★ヒト尿由来白血球増殖因子製剤の開発［高久史麿（東京大学）他/森永乳業㈱、㈱ミドリ十字（現 田辺三菱製薬㈱）］
1983年（昭和58年）
★組換えDNAによるB型肝炎ワクチンの製造技術［松原謙一（大阪大学）、大友信也（㈶化学及血清療法研究所）/㈶化学及血清療法研究所］
1986（昭和61年）
先端的研究成果展開事業を発足
★窒化ガリウム（GaN）青色発光ダイオードの製造技術［赤﨑勇（名古屋大学）/豊田合成㈱］

1987（昭和62年）
★高感度薄膜を用いたガスセンサーの製造技術［増本健（ERATO増本特殊構造物質プロジェクト）他/㈱リケン］
1989（平成元年）
新技術事業団に改称
1990（平成2年）
★走査型X線分析顕微鏡［中沢弘基（科学技術庁無機材質研究所）/㈱堀場製作所］
1991（平成3年）
★電子線照射による高耐熱性炭化けい素繊維の製造技術［瀬口忠男・岡村清人（日本原子力研究所）/日本カーボン㈱］

●アモルファス軟磁性合金薄帯

さまざまな特性を持つアモルファス金属の応用分野は、強度を生かした細線ワイヤー、スプリング、釣り糸、耐食性を生かした化学装置部品、医療機器部品、軟磁性を生かしたオーディオ機器のヘッド、磁気シールド材など多岐にわたっている。

1980年代

●アモルファス合金の応用製品

Reproduction of 「Close up 増本健が語る『技術移転』の50年　実用材料をつくる！」JST News Vol.5, pp. 6-9 (2008).

アメリカの圧力です。アモルファス金属の製造が特許に侵害すると訴えられました（1982～1984年）。国際裁判の結果、勝訴したのですが、次は、日米経済摩擦から、アモルファス金属はアメリカから買えと圧力がかかりました（1989～1990年）。結局、日本がこれに屈したため、せっかく開発した技術も、実用化への道はきわめて制限されることになってしまいました。

当時は非常に神経をすり減らし、こんなことなら実学への挑戦などせず、純粋な研究の道を進んでいればよかったと弱気になったこともありました。結果として、実用化への道の難しさを知らされることにもなりました。しかし、あれだけたくさんの企業が興味を示し、成果を上げられたのですから、実学の伝統を復活させることはできたと自負しています。大きかったのは、アモルファス金属の「懐の深さ」でしょう。3大特性以外にもさまざまな長所があり、元素の組み合わせも自由です。4つの企業と同時に開発を進められたのも、ある企業がこの特性を利用するなら、こちらは別の特性と、住み分けられたからでした。こうした視点を持つことは、実用化を目指すうえで非常に大切だと思います。

1990年～現在

“実学の実践への再挑戦”の時代。

「少量で高性能の材料を」という発想の転換。

実用化の難しさを思い知らされた後も、再び挑戦心を奮い立たせ、新たな道へと進み始めた。1989年から金研の所長を勤めた後、1996年に東北大学を退官。電気磁気材料研究所で新分野の研究を開始した。

金研の所長になってからは、自分自身ではあまり研究ができなくなりましたが、研究所全体として、新しい材料の実用化への道に取り組みました。鉄ではなくチタンやマグネシウム、アルミニウムの合金や準結晶、ナノ結晶など、さまざまな材料を開発し、実用化しています。

この頃から私の頭の中で発想が変わりました。資源は限られているのだから、少量で高性能の材料を開発するべきだと。そこで今は、主に薄膜での機能性材料の開発に力を入れています。

アモルファス金属は、私自身、今は研究テーマとしてはいません。しかし、大切に育てた子どものような気持ちは持っています。じつは、アモルファス金属の生産に関して、すでにアメリカは手を引いています。今では日本が、世界の7割を占めるまでになりました。かつて企業とともに委託開発課題で取り組んだ研究が、数十年を経て、役に立っているのです。だから、当時はアメリカに負けたような気がしていましたが、長い目で見れば私たちが勝ったのだと思っています。

ステイタスではなく本当に意味のある技術移転を。

アモルファス金属の開発をきっかけに実学への挑戦を始めて40年近く。大学から企業への技術移転が白い目で見られた当時と比べ、産学連携に国を挙げて取り組んでいる現状は、まさしく隔世の感がある。

世の中が大きく変わったのは、1980年代の終わり頃だと思います。研究室をほったらかしにして企業をまわっていた研究者がいたことに対する反発が、それだけ根強かったということでしょう。これからは、そんな疑いをかけられないように心しなければいけません。技術移転はとても意味がある制度ですし、企業は、以前と違ってある種のステイタスととらえているようにも感じられます。大学と企業の連携にとって、本当に意味のある研究テーマに取り組むための制度であってほしいですね。 ■

1993（平成5年）
★リン脂質極性基を有するポリマーの製造技術［中林宣男・石原一彦（東京医科歯科大学）/日本油脂㈱（現 日油㈱）］
1995（平成7年）
生活・社会技術開発事業を発足
新技術コンセプト・モデル化推進事業を発足
★ハイドロゲル剤型創傷被覆材［吉井文男（日本原子力研究所）/ニチバン㈱］
1996（平成8年）
地域研究開発促進拠点支援（RSP）事業を発足
日本科学技術情報センターと合併し、科学技術振興事業団（JST）設立
1998（平成10年）
★車載用磁気インピーダンスセンサ［毛利佳年雄（名古屋大学）/愛知製鋼㈱］
1999（平成11年）
新規事業指向型研究成果展開事業（プレベンチャー事業）を発足
★杉間伐材を原料とする家畜粗飼料の製造技術［寺田文典（農林水産省畜産試験場）他/宮崎みどり製薬㈱］
★機能性甘味料アラビノースの製造技術［檜作進（鹿児島大学）/三和澱粉工業㈱］
2000（平成12年）
J-STOREサービス開始
★酸素18安定同位体標識水の製造技術［浅野康一（東京工業大学）/日本酸素㈱（現大陽日酸㈱）］

2001（平成13年）
★痒み鎮静作用を有する機能性繊維［小宮山淳・白井汪芳（信州大学）/ダイワボウノイ㈱］
2002（平成14年）
研究成果最適移転事業を発足
2003（平成15年）
技術移転支援センター事業を発足
独立行政法人化、科学技術振興機構（JST）に改称
2004（平成16年）
大学連携版新技術説明会を新設、イノベーション・ジャパン（大学見本市）初開催
2005（平成17年）
独創的シーズ展開事業を発足
2006（平成18年）
産学共同シーズイノベーション化事業を発足

「JST技術移転事業50周年記念シンポジウム」開催！ 2008年11月14日（金）東京 経団連ホール、詳細はhttp://www.jst.go.jp/tt/50th/

TEXT：十枝慶二／PHOTO：大沼寛行

News Update

2009 WPI-AIMR Annual Workshop Report

The 2009 WPI-AIMR Annual Workshop was held from March 1, Sunday through March 6, Friday, 2009, at the Miyagi-Zao Royal Hotel in Zao Town of Miyagi prefecture. One of the main purposes of the workshop was to stimulate immediate and/or future possible Fusion Researches among WPI researchers and between WPI and outside WPI researchers. The Program Committee, Mingwei Chen, Tomihiro Hashizume, Akihisa Inoue, Yukihisa Kitamura, Terunobu Miyazaki, Toshio Sakurai, Masatsugu Shimomura, Masaru Tsukada, and Yoshinori Yamamoto have set up the plenary sessions, Mini workshops, and Poster sessions aiming for that task.

The total participants of the workshop counted to 180 and the total presentation at the workshop was 164, which included 32 invited talks in the eleven plenary sessions and two Special sessions, 50 talks in eight Mini workshops, and 82 Poster presentations. We had two special guests of the International Advisory Board members, J. Georg Bednorz (1987 Nobel Prize in Physics), Fellow, IBM Zurich Research Laboratory, and Heinrich Rohrer (1986 Nobel Prize in Physics), Chair of the International Advisory Board.

In the opening session, Professor Toshio Kuroki, WPI Program Director, kindly addressed Congratulatory remarks and introduced World Premier International Research Center (WPI) Initiative and missions of WPI Research Centers, which are (1)Top Quality of Science, (2)Fusion of Research, (3)Globally Visible Research Center, and (4)Break of Administrative Limitations. For the Opening remarks, Yoshinori Yamamoto, Institute Director, summarized researches of the four thrusts of WPI-AIMR, Physical Metallurgy, Physics, Chemistry, and Device/System.

In the plenary sessions, we had invited talks by the world reading scientists on the most advanced sciences of the hottest topics. In the special sessions, Georg Bednorz talked about oxide-based resistive memories and showed advanced researches to critically understand the future nonvolatile memories. Tetsuro Higashi, Tokyo Electron Limited, and Tadahiro Ohmi, Tohoku University, showed current status of silicon industry and discussed about importance of green technologies and science based new silicon technology for the information and communication technology.

The Mini workshops were designed so that the organizers of younger generation can stimulate Fusion Researches and also allow younger researchers to present and discuss on their daily researches. All the Poster presentations were coupled with Short presentations again for giving chance for younger researchers to discuss their sciences with world reading scientists.

On behalf of the Program Committee of the 2009 WPI-AIMR Annual Workshop, we would again like to express our warmest appreciation to all the participants of the workshop. Now we are looking forward to having proceedings of the Workshop, a book publication by Springer of "Advances in Materials Science" Series.

(by T. Hashizume, program chair)

Attending 2009 APS March Meeting

The 2009 APS March Meeting was held in Pittsburgh, PA from March 16, Monday to March 20, Friday, in a new convention center, David L. Lawrence Convention Center. The headquarters hotel was the Westin Convention Center Hotel, connected to the David L. Lawrence Convention Center by a skywalk.

The APS March Meeting is one of the biggest and most important scientific meetings in US, partially because American Applied Physics Society does not exist and thus the APS March Meeting is a kind of mixture of Physics and Applied Physics Societies. This year, there were attendees of more than 7,000, first and co-authors of 20,000, and papers of about 5,000 in five days. Each day has three major slots for three hours each and 40 parallel sessions were set for each slot, counting 560-570 sessions in total. Among these sessions, there also were focused sessions of about 240. Four major topics in the Focused sessions this year were (1) Physics of spins including magnetic semiconductors (32 Focused sessions), (2) Graphens (22 Focused sessions), (3) New Fe based superconductors (Fe Pnictides) (17 Focused sessions), and (4) Bio-related topics.

In one of the Focused session on Monday, Hideo Hosono of Tokyo Institute of Technology, who is also one of the JSPS site-visit Committee Members of WPI-AIMR, presented his invited talk as a pioneer of the Fe Pnictides and discussed general experimental features, such as crystal structures, crystallographic transition accompanying anti-ferromagnetic to paramagnetic state, Fe square lattice, new insulating layer, high pressure synthesis, and epitaxial thin films of Pnictides.

In one of the Award celebration sessions on Monday afternoon, Akihisa Inoue (WPI-AIMR) presented the James C. McGroddy Prize Talk entitled "Development and Applications of Bulk Metallic Glasses." One of the lists he used in the presentation was showing a list of bulk metallic glasses (BMG) and was very impressive demonstration because most of the BMG materials were developed by the Inoue Group. In another Award celebration invited session, Terunobu Miyazaki (WPI-AIMR) presented Oliver E. Buckley Prize Talk entitled “Birth of tunnel magnetoresistance and its development.” He introduced the history of the research in the tunnelling magnetoresistance, which was pioneered by him among others and then he focused on the Heusler electrode junctions and applications of tunnel magnetoresoistance junctions.

Another important aspect is the APS March Meeting Show, where the March Meeting offers companies opportunities for exhibiting their products and one-to-one interaction with participants. There were about 120 booths this year and the exhibition hall was full of researchers during the 3-day Show period.

T. Hashizume

The American Physical Society

JAMES C. McGRODDY PRIZE
FOR NEW MATERIALS

endowed by
IBM
is awarded by the American Physical Society to recognize outstanding achievement in the science and application of new materials.

This Diploma certifies that the 2009 Prize has been presented to

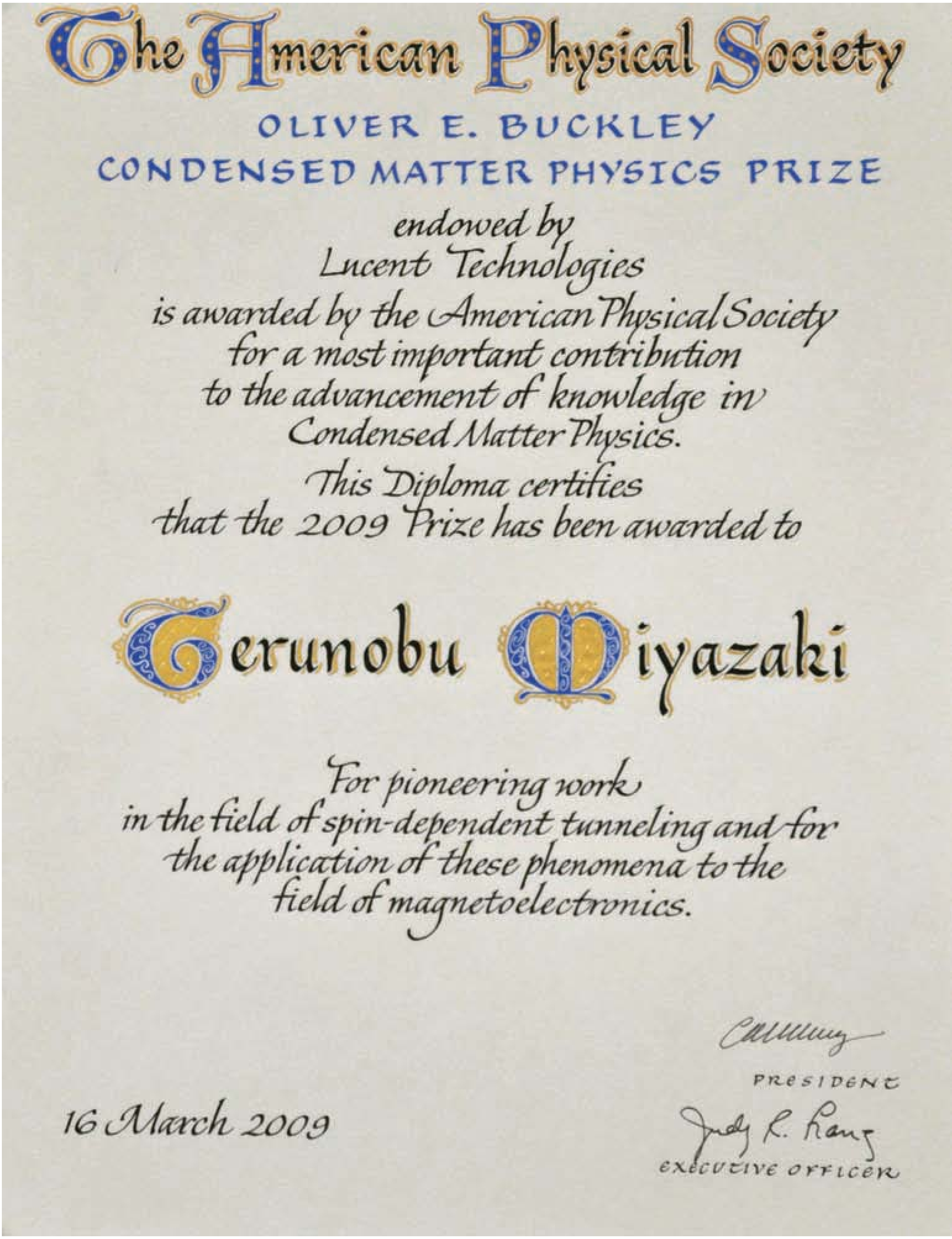
Akihisa Inoue

For the development of slow cooling methods for the fabrication of bulk metallic glasses with remarkable mechanical properties and the characterization and application of these materials.

PRESIDENT

16 March 2009

EXECUTIVE OFFICER



The American Physical Society

OLIVER E. BUCKLEY CONDENSED MATTER PHYSICS PRIZE

endowed by
Lucent Technologies
is awarded by the American Physical Society
for a most important contribution
to the advancement of knowledge in
Condensed Matter Physics.
This Diploma certifies
that the 2009 Prize has been awarded to

Terunobu Miyazaki

For pioneering work
in the field of spin-dependent tunneling and for
the application of these phenomena to the
field of magnetoelectronics.

PRESIDENT

16 March 2009

EXECUTIVE OFFICER

Award Ceremony Session

Surface Kinetics International (SKI) Conference
March 20-22, 2009　Salt Lake City

The first surface kinetics International (SKI) conference was organized by Dr. Feng Liu in Materials Science & Engineering, University of Utah in Salt Lake City on March 20-22, 2009. This meeting was arranged for the weekend directly following the APS March meeting in Pittsburgh.

About 50 researchers from Austria, China, Germany, Hong Kong, Japan, Korea and USA gathered together in a well designed convention hall of Commander House in the university campus.

Altogether twenty two talks and more than ten posters were presented. The topics covered a wide range of surface sciences such as nucleation, epitaxial growth of quantum dots, thin films, interface diffusion, step propagation kinetics and computer simulations. The understanding of the 2D nucleation kinetics has been much advanced by AFM observations with computer simulation.

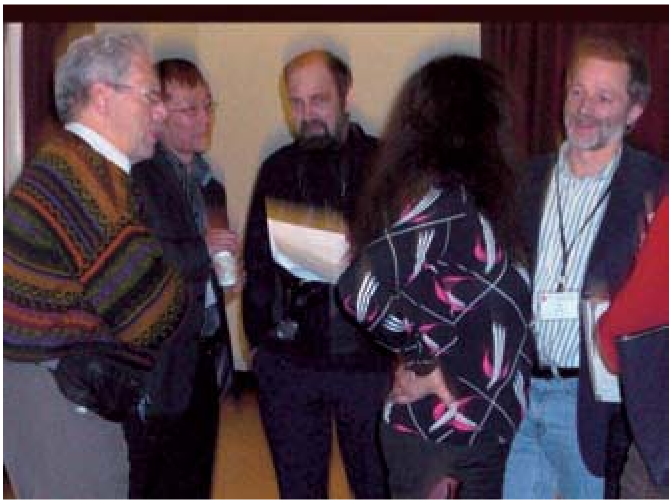
President A. Inoue gave a talk on "Development & Applications of Bulk Glassy Alloys—in conjunction with use of glassy alloy surface." Dr. K. Akiyama talked about "Nano-scale lithography with frequency modulation AFM." Professor Y. Hasegawa presented "The screened potential and the Friedel oscillation observed by LT-STM."

This was an enjoyable meeting to obtain a panorama view of surface kinetics.
Recreation time for skiing or walking was arranged by the members of Dr. Feng Liu's group. All the participants are thankful to their help throughout the conference.

(It was regrettable Professor Sakurai could not come to SKI meeting in spite of Dr. Feng Liu's invitation because of an important WPI meeting in Tokyo.)

Reported by Hiroshi Komatsu
Photographs taken by Akiyama and Komatsu

Surface Kinetics International (SKI) Conference
March 20-22, 2009 Salt Lake City



Note on 2009/2010 WPI Joint Seminar Series

M. Tsukada and T. Hashizume

The WPI-Joint Seminar Series between theory and experiments (hereafter the first stage WPI Joint Seminar Series) started in September 2008, and ended in February 5, 2009. The list of the lectures given in the Seminar series is shown in Table 1. The aim of the WPI Joint Seminars is to provide an excellent forum of communications of younger research staffs with exchange of ideas of different research fields to catalyze new fusion researches. It has been very successful so far, and to promote further activities of the WPI-Joint Seminar Series, we decided to invite younger research staffs as co-organizers, and enhance the functions of the WPI-Joint Seminars. The new stage of the WPI Joint Seminar Series will be started from April, 2009 with the newly assigned young co-organizers.

The newly assigned Organization Committee members (Co-organizers) consist of the young research staffs of WPI as listed below;

K. Akagi (Theory, Tsukada G) akagi@wpi-aimr.tohoku.ac.jp
A. Kuzume (Nano-Chemistry, Itaya G) kuzume@atom.che.tohoku.ac.jp
K. Nakajima (Nano-Chemistry, Nishi G) knakaji@wpi-aimr.tohoku.ac.jp
T. Hitosugi (Nano-Phys., Hashizume G) hitosugi@wpi-aimr.tohoku.ac.jp
S. Mizukami (Device, Miyazaki G) mizukami@wpi-aimr.tohoku.ac.jp
D. Louzguine (BMG, Inoue G) dml@imr.tohoku.ac.jp
T. Trevethan (Theory, Shluger G) t.trevethan@ucl.ac.uk
R. Pierre (Nano-Phys., Takahashi G) p.richard@arpes.phys.tohoku.ac.jp
K. Akiyama (Device, Esashi G) akiyama@mems.mech.tohoku.ac.jp

Professors T. Hashizume and M. Tsukada remain as the Organization Committee members (Organizers), and Professor H. Komatsu will join to the Committee members as an Adviser.

The first Organization Committee meeting was held on March 24th 2009 and various issues including the general principles of the WPI Joint seminar, managing philosophy, operations guideline were discussed.

The general guideline of the first stage of WPI Joint Seminar, which were held from September 2008 to February 2009, is summarized in the announcement delivered last year (see the Table 2 below). The item 1), 2) and 3) of the guideline are confirmed again to be the important general principles for the new Seminar Series. The summary of the discussions in the first Organization Committee meeting is as follows;

1) About the style of the Seminar talks

Three types of the seminar talks should be considered;

I) General over all lecture by senior researchers (e.g., PI). We can also invite appropriate speakers even outside WPI-AIMR.
(Allotted time is 45min with 15min for discussions)

II) Lectures of hot topics with ample discussions and mutual communications by younger staffs
(allotted time is 30min with 30min discussions)
These types of the talks (I and II) should include enough introductory part for non-specialists audience as stated in the announcement (see Table 2).

III) Tutorial lectures with allotted time for each 45min with 15min for discussions.
Examples of the subjects are
a) Philosophy for scientific or technological researches
b) How to make good communication among different fields
c) Statistical analyses of measurement errors

2) The immediate candidates for the Speakers for the type I or II

a) As the candidates of the speakers we can consider PIs who have not yet given a talk at the First Series of the WPI-Joint seminars (see the lineup, Table 1). If the PI is not available, any persons from his group should be considered.

b) For the foreign PI candidates, their seminars are better to be associated with some appropriate occasions, e.g., visiting for the fusion research.

c) For the list of the speakers for further Seminars, we will nominate at next and further Organization Committee Meetings.

2) Examples of other expecting topics for the Seminars

a) Fuel cell, Solar cell, b)Coordination chemistry, c)Bio-science
d) Novel superconducting materials

In the Committee meeting, we also discussed how to nominate the good candidates of the speakers for the Seminars in future, as well as the first special seminar on April by the Institute Director. As for Tea-break, and mixers, various new idea have been proposed.

Any proposals to the coming second series of the WPI Joint seminars are most welcome. Please send your opinion to any of the Organization Committee members.

WPI Joint Seminar Series of Theory and Experiments
Lineup of the first stage series

The 1st Seminar September 11

M. Tsukada (WPI-AIMR): Contribution of Theory and Computation for Surfaces, Interfaces, and Nano-Materials

M. Kawasaki (WPI-AIMR): Interface Physics and Devices by Oxide Block-Building

T. Hashizume (WPI-AIMR): Interface Issues of Organic TFT and Scanning Probe Microscopy Analysis

The 2nd Seminar October 9

T. Nishi (WPI-AIMR): Application of Nano-mechanical Mapping to Polymer Alloys and Composites

T. Trevethan (UC London): Modeling the adsorption, diffusion and manipulation of molecules on insulating surfaces

The 3rd Seminar October 22

K. Tanigaki (WPI-AIMR): Atoms and Molecules in Nano Spaces and Interfaces with their Applications for Materials Science and Electronics

R. Saito (Dept. Physics, Tohoku Univ.): Dark exciton and phonon softening phenomena of single wall carbon nanotubes

The 4th Seminar November 18

T. Miyazaki (WPI-AIMR): Recent Progress of Large Tunnel Magnetoresistance Junctions and their Applications

S. Maekawa (IMR): Spin Current, Charge Current, Heat Current and Spin-Electronics

The 5th Seminar Dec. 9

Y. Ikuhara (WPI-AIMR): Interface Characterization by Cs corrected STEM

K. Itaya (WPI-AIMR): Electrical Double Layer and Electrochemical Reactions with Atomic Levels

K. Akagi (WPI-AIMR), I. Hamada (Osaka Univ.): Theoretical Approaches for Solid-Water Interfaces

The 6th Seminar Dec. 16

M. Chen (WPI-AIMR): Nanoporous Metals: synthesis, structure and properties

M. Tokuyama (WPI-AIMR): Theory of Glass Transitions in diversely different Glass-Forming Systems

The 7th Seminar Jan. 9

T. Takahashi (WPI-AIMR): High-resolution ARPES study of graphene, graphite, and GIC superconductor

K. Nomura (Dept. Phys.): Quantum transport of massless Dirac fermions in graphene

The 8th Seminar Feb. 5

H. Fukuyama (Tokyo Sci. Univ.): Emergence in Condensed Matter

T. Hitosugi (WPI-AIMR): Quest for new physics in oxide nanostructure

D. Louzguine (WPI-AIMR): High-strength and ductile hypereutectic Ti-Fe system based alloys

A. Kuzume, (WPI-AIMR): Electrochemistry on single crystal surface:Reconsideration of experimental "facts".

S. Mizukami (WPI-AIMR): Pump-probe investigation of spin dynamics in magnetic thin films

Table1

Announcement from the organization committee of WPI-Joint Seminar

1)The purpose of the WPI-Joint Seminar is to enhance the mutual communication among different fields in WPI-AIMR towards fusion research

2)Speakers should start with very wide and general introduction to their fields and give the talk not for the specialists but for non-specialists. Any amateur questions by audience are most welcome, even during the talk.

3)The participation to the WPI-Joint Seminar is mandatory

i) please submit a short report (within 1 page)of the Seminar talk to the organization committee (Address; tsukada@wpi-aimr.tohoku.ac.jp)

ii) Please check your name on the name list when you come to the seminar room

4)Young research staffs at WPI-AIMR are invited to the co-organizers of the WPI-Joint Seminar to make planning and arrange forthcoming Seminars

5)Proposals for the speakers or the Seminar topics are generally most welcome. Please give us the information by e-mail

Table 2

---- Mingwei CHEN ----

Physical Review Letters 2009 in Press

X. Q. Yan, Z. Tang, C. Q. Jin, T. Goto, J. W. McCauley and M. W. Chen

"Depressurization amorphization of single-crystal boron carbide"

Strong covalent solids generally possess very high strength and are widely used as super-hard materials for the applications where strength and hardness are critical. However, the underlying mechanisms of deformation and failure of these materials subjected to extreme loading conditions, such as high pressures and shock, are less known [1]. Boron carbide (B_4C) is one of the hardest materials after diamond and boron nitride and has been widely used as an armor ceramic material. A variety of constitutive models have also been developed to model its performances at high pressures. The results have been mixed because the actual micromechanisms that lead to failure at high pressures are unclear. Recently, our HREM observations of ballistic B_4C fragments reveal that the high pressure damage of B_4C is associated with the formation of nano-sized amorphous bands [2]. Following our finding, a number of experiments including dynamic mechanical tests, mechanical scratching and nanoindentation tests have demonstrated that the damage of B_4C is related to the pressure-induced amorphization.[3] It is thus expected that more controllable high-pressure diamond anvil cell (DAC) experiments, the standard technique for high-pressure study, can provide additional insights on the phase transition. A number of DAC experiments of B_4C have been attempted with loading pressures up to 50 GPa and, mysteriously, the amorphous B_4C has not been detected by synchrotron XRD and neutron diffraction. In the recent PRL paper [4], we reported pressure-induced amorphization of B_4C investigated by *in situ* high pressure Raman spectroscopy and first-principles molecular dynamics simulations. We found that the amorphization of B_4C takes place unexpectedly during unloading from high nonhydrostatic pressures (**Figure 1**). First principles simulations suggest a novel amorphization mechanism that the irreversible local atomic/electronic structure changes caused by nonhydrostatic pressures can result in elastic destabilization and thereby amorphization during depressurization. This study provides an atomic explanation on the anomalous dynamic properties of B_4C subjected to shock loading and high pressures, which may be generically applicable to other strong covalent solids with similar atomic structures.

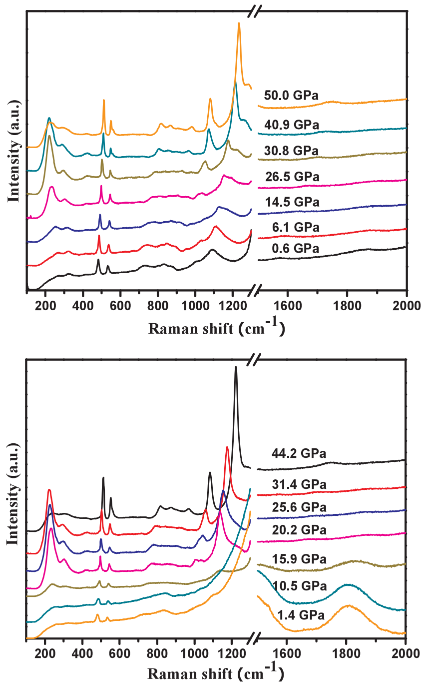


Figure 1 High-pressure Raman spectra of single-crystal B_4C. (a) B_4C loaded from ambient up to 50 GPa. (b) B_4C decompressed from 50 GPa to ~1.4 GPa. Amorphous B_4C with a characteristic peak at ~1810 cm^{-1} emerges when the pressures is below ~20 GPa during unloading.

References

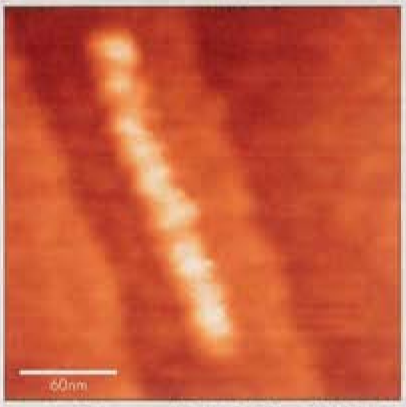
[1] M. W. Chen, *et al.*, *Nature Mater.* **5**, 614 (2006).
[2] M. W. Chen, *et al.*, *Science* **299**, 1563 (2003).
[3] X. Q. Yan, *et al.*, *Appl. Phys. Lett.* **88**, 131905 (2006).
[4] X. Q. Yan, *et al.*, *Phys. Rev. Lett.*, 102, 075505 (2009).

研究者の横顔

高精度なAFM探針で線幅10nm以下の金細線を描画し単一分子の電気伝導特性の解明に挑む

秋山 琴音
AKIYAMA, Kotone
東北大学原子分子材料科学高等研究機構
東北大学大学院工学研究科
助教　博士（理学）

現在、LSIの標準的な最小加工線幅は約65nm。最先端技術を駆使しても約22nmが限界とされている。東北大学の秋山琴音博士は、原子間力顕微鏡（AFM）を用いて線幅10nm以下の金細線を試料上に描画する画期的な研究に取り組んでいる。極めて細い金細線は、半導体デバイスの小型化やLSIの高集積化をさらに進化させるだけではなく、単一分子やナノ構造体の電気伝導特性など未だ解明されていない特性の解析にも応用できる。未知の領域を開拓する研究に迫った。



AFMリソグラフィーで描画した金細線。探針と試料との距離を精密に制御しながら金の原子を電界蒸発させ、試料上にドットを打つように細線を形成していく。線幅は20〜30nm。印加するパルス電圧の幅などパラメータを変えることで、線幅を10nm以下にすることを目指している。

曲率半径3.5nmの探針の開発

秋山博士は、東北大学大学院の博士課程に在籍中に、現在の研究テーマに出会った。当時の研究テーマは、走査型プローブ顕微鏡の探針の製作であった。走査型プローブ顕微鏡とは、レコードプレーヤのピックアップのように、鋭利な探針を検出器とし、それを物質の表面をなぞるように動かしながら分子や原子レベルにまで表面を拡大観察する装置である。探針が細く鋭いほど分解能は向上する。原子レベルの物質の構造像や電位分布図などを得るには、先端の曲率半径が数nmという細い探針が求められる。当初、秋山博士は、金属ワイヤーをカンチレバーに取り付け、そのワイヤーの先端を薬品に浸してエッチングで細く加工する化学的な方法で探針の製作に取り組んでいた。しかし、「薬品の表面張力でカンチレバーまで薬品に浸ってしまったり、制御が困難な課題も多く、研究に行き詰まりを感じていました」という。解決策を模索するなか、指導教官から集束イオンビーム（FIB）を用いて金属ワイヤーを物理的に削り出し、細く加工する方法があることを知らされた。「FIBを用いて走査型トンネル顕微鏡（STM）の探針を削る加工法は、すでに研究されていました。その方法を原子間力顕微鏡（AFM）の探針の製作に応用してみたらどうなるか。そう閃いたのです」。

STMの探針は、AFMのそれと比べると太い。「STMの探針の直径は100nm程度でした。一方、私が作ろうとしていたAFMの探針は数nmから10数nmです。この一桁細い探針を製作するために、金属ワイヤーの芯の部分を残して周囲をドーナツ状に削っていく手法を試してみました。ちょうど鉛筆を尖らせていくように加工していくんです。このようなFIB加工法をAFMの探針の製作に応用したことは新しかったと思います」。秋山博士は、タングステンを用いて先端の曲率半径3.5nmという細く鋭い探針の製作に成功した。「これがひとつのターニングポイントでした。この成果を何かに応用したいと思いました」。秋山博士はさらなる手ごたえを確信して応用展開の研究に取り組んだ。

AFMリソグラフィーへの応用

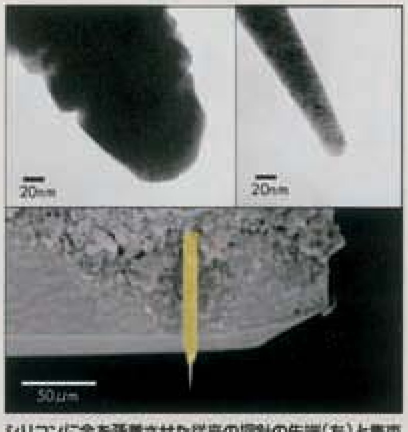
AFMは、物質表面の分子や原子を観察するのに用いられるだけではなく、探針にパルス電圧をかけることで探針の原子を飛び出させ、試料上に蒸着させて微細なドット（点）を描画する「AFMリソグラフィー」にも利用できる。たとえば、探針を金で製作すれば、微細な金のドットを描画できるようになる。「それまでのAFMリソグラフィーに用いられていた金の探針は、曲率半径10nm程度のシリコンの上に金を蒸着して作られていました。そのために曲率半径は50〜100nmと、どうしても太くなってしまいます。また金の量が少なく、すぐになくなってしまいます。この探針を金だけで、そしてもっと細く製作できれば、AFMの分解能が向上しますし、非常に細い金の細線をいくらでも試料上に描くことができます。金の細線は、半導体デバイスやLSIの配線や電極へと応用が広がっていくはずです。新たな研究テーマとの出会いでした」。

秋山博士は、さっそく金の探針の製作に取りかかった。目標は曲率半径10nm以下。「金の探針の製作では、タングステンの細い探針を製作したときと同じように金ワイヤーをカンチレバーに取り付けて、FIBで物理的に加工しました。今思うと、金属のワイヤーをシリコンカンチレバーにどういう手段で接着するのかを考え、実現するまでが一番大変でした」。秋山博士は、バイオ分野などでの研究に用いられているマイクロマニピュレータを用いて、金属探針を動かしてカンチレバーに接着する実験を繰り返した。「この作業は本当に根気のいるものでした。接着剤の温度管理も難しかったですし、ミクロン単位で金属探針を動かして接着する

操作がなかなかうまくいきませんでした。帰宅しても実験の話ばかりしているものですから、家族まで「こうしてみたら」とアドバイスをくれるほどでした」。数多くの試行錯誤ののち、文字通り「そうっと金属探針を動かしてくっつける」ことで成功した。

水晶振動子で探針と試料との距離を制御

しかし、こうして作られた金の探針を用いても、AFMリソグラフィーで金細線を描画することは容易ではなかった。「シリコンのカンチレバーでは、大きくて不揃いのドットしか打てなかったのです」。AFMリソグラフィーでは、探針と試料にバイアス電圧をかけて、探針の原子を飛び出させる電界蒸発という手法で描画する。しかし、バイアス電圧をかけるとシリコンのカンチレバーが静電気力で曲がってしまい、探針が試料表面にぶつかってしまうのだ。衝撃で針も曲がってしまうし、当然、ドットも大きくなってしまう。パルス電圧が印加されたときでも探針が試料表面に接触しないように、探針と試料との距離を精密に制御しなくてはならない。「シリコンカンチレバーが電圧印加によって大きく曲がってしまわないように、何とか解決しなければなりませんでした。そんなときに、共同研究先である東京大学物性研究所の研究者から、AFMのセンサを、カンチレバーではなく、水晶振動子にしてみてはどうかとアドバイスをいただいたことが、非常に大きなヒントになりました」。水晶振動子は、シリコンカンチレバーよりもばね定数が高い。そのため、パルス電圧印加時のセンサの曲がりを抑制でき、探針と試料との距離の精密制御も可能となる。そう考えた秋山博士は、さっそく水晶振動子をAFMのセンサとして利用した。その結果、探針と試料との距離を制御できるようになり、現在、線幅20～30nmの金細線を試料上に描画することに成功している。



シリコンに金を蒸着させた従来の探針の先端（左）と集束イオンビーム（FIB）で金ワイヤーを加工して作製した金探針（右）。従来の探針の先端の曲率半径が50～100nmに対し、FIB加工で作製した探針の曲率半径は12nm程度とはるかに鋭い。下は水晶振動子に取り付けた金探針。

分子エレクトロニクスへの応用

「金細線の幅を2009年3月までに10nm以下にまで細くすることが当面の目標です」と、秋山博士は目を輝かせる 。現在はパルス電圧などのパラメータを変えながらの研究に取り組んでいるところだ。ただし、狙うところは「10nm以下の金細線の描画」だけではない。視線の先には「分子エレクトロニクスへの応用」がある。半導体素子の微細化やLSIの高集積化は今後、さらに加速していくことが予測される。その一方で、分子や原子といった物質の最小単位を電子回路素子として、さらに高集積な回路を形成する分子エレクトロニクスにも注目が集まっている。分子エレクトロニクスでは、分子そのものが素子となるため「単一分子に電気を通したときにその分子がどのように振る舞うのか」といった基本的な物性が解明されなければならない。現在、単一分子の電気伝導性の測定は、次のような手法が用いられている。まず、金と結びつきやすくした測定対象分子を溶液中に溶かし、この溶液に表面が金の測定電極を浸す。つぎに、溶液中で金探針を測定電極に軽く衝突させた後両者を引き上げて、これらに挟まった分子を測定する。対象分子の両端に金が存在する鎖構造となるため、分子の電気伝導性を測定することが可能となる。「ただ、この手法では、金と金との間の分子が1つだけなのかどうかは厳密には特定できないので、もしかしたら分子が2個存在しているかもしれません。何度も実験を繰り返してデータを取る、統計的な手法でしか単一分子の電気伝導性を測定できません。ですから、単純な構造の分子ですら、その電気伝導性は未だに解明されていないのです」。秋山博士は、現在、この分野にこれまでの研究成果を展開しようと取り組んでいる。具体的には、測定基板上の電極と電極の間に単一分子やカーボンナノチューブなどの測定対象を固定する。次にAFMリソグラフィーで金細線を描画し、測定対象と電極を接続する。電極から電気を流すことで、単一分子やナノ構造体の電気伝導性の測定が可能となる研究である。「この方法なら、単一分子やナノ構造体のどこに接続されているか精密に観察した後にその電気伝導特性を測定できます。多くの研究者が納得できる手法で、分子エレクトロニクスの基礎となる単一分子やナノ構造体の電気伝導性などの特性を解明していけると思います」。もちろん容易に求める結果が得られるとは考えてはいない。「『研究とは長いトンネルを歩いて行くようなもの。その先には必ず"光"がある。』大学院時代の恩師の言葉です。私はスロースターターかもしれませんが、地道にコツコツと研究を進めていきたい。歩みを止めるつもりはありません」。秋山博士の研究は、極微の世界に光を照らす大きな可能性を着実に深めている。

Reproduction of 「研究者の横顔　秋山琴音」 metamorphosis no.14 pp.26-27 (2008).

2030年への挑戦
次世代産業技術

ナノファイバー（上）

「塗る太陽電池」に展望

京大ではレーザー光を液晶画面で反射させてガラスに当て、微細な光の回路を作っている

《研究小史》

1930年代	細いファイバー製造に適したエレクトロスピニング法が開発される
1990年代	直径が数十ナノメートルのファイバーが製造可能になる
2002年	米国防総省がナノファイバーで兵服を開発するプロジェクト開始
2006年	日本で新エネルギー・産業技術総合開発機構がプロジェクト開始
2008年	日本でナノファイバー学会発足。有機、無機、金属の専門家結集

肉眼では見えない「ナノファイバー」という極細繊維が注目され出した。繊維の直径や中の構造が数十ナノ（ナノは十億分の一）㍍以下なのが特徴で、様々な装置の省エネ化に有効。発電効率の高い太陽電池を安価に製造したり、燃料電池を十分の一以下に小さくしたりできるとみられ、研究が加速している。

透明な容器の底が次第に白くなった。上から降り注ぐナノファイバーが積み重なってシートができていく。東京工業大学の谷岡明彦教授らは様々な有機材料を使ってナノファイバー作りに挑んでいる。

研究室の松本英俊特任准教授は昨年末、直径百ナノ㍍の炭素繊維を敷き詰めたシート表面に直径五十ナノ㍍の酸化亜鉛のナノファイバーを生やすことに成功。酸化亜鉛の表面に太陽光で電気を発生する色素を塗って太陽電池の働きを確認した。

この電池は色素増感型の一種。このタイプはシリコン太陽電池に比べ製造コストが安いうえ、様々な場所にペンキを塗るように作れる。電極が酸化インジウムすず（ＩＴＯ）と酸化チタンからなるタイプが主流だが、課題は発電効率が七―八％程度と低いことだ。松本特任准教授は電極を炭素と酸化亜鉛の組み合わせにすれば一〇％以上に高められるとみている。

太陽電池以外にも省エネにつながる極細繊維の開発に取り組んでいる。燃料電池への応用はその一つ。燃料電池で使う二つの電極の間に挟む電解質には、一般的にフッ素系高分子のフィルムが使われている。松本特任准教授は、この高分子で直径が四百ナノ㍍で中央に二百ナノ㍍の穴が開いたマカロニ型のナノファイバーを作った。

フィルムをマカロニ型の集合体に変えると発電効率が上がり、燃料電池を現在の十分の一から二十分の一に小さくできる可能性があるという。

「様々なナノファイバーを作り、どこにでも張れたり着たりできる太陽電池など、多様な電子機能を備えた布を織るのが夢」（谷岡教授）。夢の実現に向けて改良研究に力を入れている。

ナノファイバー研究は有機材料から始まったが、無機材料を研究する専門家もいる。京都大学の平尾一之教授らは、透明なガラスにレーザー光を当て、内部に直径が数十ナノ㍍のナノファイバー構造を作り込む技術を開発した。ガラスの屈折率を変えられるほか、溶かした金属や半導体のイオンを還元すれば金属や半導体のナノファイバーをガラスの中に作れる。

この構造はレーザーを直接ガラスに当てても作れるが、平尾教授らは浜松ホトニクスなどと独創的な技術を開発した。まず液晶画面に電子回路を映し出す。ここをめがけて発光時間が百フェムト（フェムトは千兆分の一）秒と短いパルスレーザーを当てると、反射してガラスに当たる。すると画面に映った通りの立体回路がガラスの中にできる。

現在はガラスの中に様々な材質のナノファイバーでできた回路を作る技術を開発中。「電気回路を光回路に変えると超高速の情報通信デバイスなども実現できる。二〇三〇年ごろには実用化しているだろう」と平尾教授は期待する。

▼ナノファイバー　直径がナノ（ナノは十億分の一）メートルサイズか、内部にナノレベルの複雑な構造を作り込んだファイバーの総称。一九九〇年代初めに合成繊維の有機高分子材料で研究がスタート。その後、ガラスに代表される無機材料や金属でも研究が進んでいる。

ナノレベルになると通常の物質には現れない「量子効果」と呼ぶ特殊な性質を示す。電気や光に反応するようになり、様々な素子や機能性繊維を実現できる。病原体のウイルスも大きさがナノレベルで、ファイバーの太さや内部の微細構造の大きさと同程度なので、相互作用によってウイルスを分解する効果も期待できる。

Reproduction of「2030年への挑戦　次世代産業技術　ナノファイバー（上）」
『日経産業新聞 (Nikkei Business Daily)』FEB. 24, 2009, p.13

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2030年への挑戦

次世代産業技術

ナノファイバー（下）

「有機・無機・金属」一体で

昨年十二月、ナノファイバーの研究を推進する「ナノファイバー学会」が発足した。有機材料の専門家で、この領域の研究をけん引してきた東京工業大学の谷岡明彦教授が初代会長に就任した。

学会には有機材料のほか、無機材料、金属材料の専門家も加わった。異なる種類の材料の専門家が知識を持ち寄ることで、「これまでにない新材料が生み出され、新しい産業につながるだろう」と副会長となった京都大学の平尾一之教授は期待する。

有機材料や無機材料に比べると研究の例は少ないが、金属材料でも世界的に大きな成果が日本で出始めている。

東北大学の中山幸仁准教授はガラス状態にしたジルコニウム系金属材料の中に直径が十㌨（㌨は十億分の一）㍍、長さ二十㍈（㍈は百万分の一）㍍ほどのファイバーができることを発見した。

この金属ナノファイバーの合成条件も研究してきた。特性の測定も始めている。「まだ一部の材料で成功した段階だが、ガラス状態にできる他の二十—三十種類の金属でも間違いなくナノファイバーを作れるだろう」（中山准教授）と自信を見せる。

開発目標の一つは磁性を持つ金属ナノファイバー。超電導量子干渉素子（SQUID）並みに高感度の磁気センサーを開発したいと考えている。SQUIDは極低温に冷却しないと利用できないが、金属ナノファイバーを使えば室温で利用可能な高感度磁気センサーが実現すると予想している。

学術面のレベルアップを図る学会の発足に先立ち、ナノファイバーを産業につなぐ新エネルギー・産業技術総合開発機構（NEDO）のプロジェクトが二〇〇六年度から進められてきた。毎年十億円前後の予算が投じられており、一〇年度まで続く。帝人や東洋紡、日清紡、グンゼなど日本を代表する繊維メーカーを含む民間十二社が加わっている。

「しかしこれだけでは不十分。米国に負けないためにはもっと多くの業界に加わってもらいたい」と谷岡教授は訴える。NEDOのプロジェクトは有機材料を中心に進めている。学会と同様に無機や金属のナノファイバー開発を進め、総合力で新産業を生み出したいというのが谷岡教授の希望だ。

実はナノファイバーの研究は米国が早かった。国防総省（DOD）はマサチューセッツ工科大学（MIT）を中心として〇二年にナノファイバーで兵服を開発するプロジェクトを始めた。砂漠などで行動する兵士の服を軽くするほか、断熱、爆薬の検出、ウイルスを分解する機能などを盛り込む計画を立てている。

軍事研究だけではない。「エネルギー省（DOE）や全米科学財団（NSF）、国立衛生研究所（NIH）などもナノファイバー研究に力を入れ始めた。水の浄化などの環境技術、がん治療に使う薬物送達システム（DDS）などの医療技術の開発で日本は水をあけられている」と東京工業大の松本英俊特任准教授は説明する。

日本では米国のような軍事研究は進めにくい。しかし環境・エネルギーや医療などの分野では米国を早く追い越したいと関係者は口をそろえる。現在、米国は有機材料中心で研究を進めているとみられる。個々の材料の基礎研究では世界的に評価されている日本としては、有機、無機、金属の総合力が米国を追い越すカギを握っている。

（黒川卓）

東北大学では金属をガラス状態にしてナノファイバーを合成している

《実用化の将来像》

東京工業大学の谷岡明彦教授が2030年までに実現を期待する技術の１つは、半導体素子のナノファイバーを衣服に織り込むこと。体温や発汗量、汗の成分などを測定可能な服ができる。これらのデータは無線で病院に送り、医師は服を着た人の健康状態を監視できるようになる。ほかに大気中の成分を測定できるうえ、有害な細菌はナノ構造の中に捕らえられて分解する。

私の見方

高分子物性研究の第一人者である梶山千里・九州大学前総長（日本学生支援機構理事長）

有機材料のナノファイバーを例に挙げれば、単に細かったり内部にナノ構造があるというだけでは理想的な機能は得られない。「分子間」と「分子内」をいかに制御できるかがカギを握る。

同じ直径のファイバーを積み重ねても分子間のすき間が均一でないとガスや液体を分離する「分子ふるい」に使えない。ファイバーそのものの性質を利用するには分子内の配列が設計通りでないといけない。難易度の高い研究分野といえる。

Reproduction of 「2030年への挑戦　次世代産業技術　ナノファイバー（下）」
『日経産業新聞 (Nikkei Business Daily)』FEB. 25, 2009, p.13

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milsil 自然と科学の情報誌［ミルシル］

No.2 2009 Vol.2

「milsil（ミルシル）」について

'milsil（ミルシル）'の'mil（ミル）'は「見てみる」「聞いてみる」「やってみる」の「ミル」。そのような「ミル」から、新たな、そして豊かな'sil（シル=知る）'が得られるでしょう。
　この雑誌とともに、皆様が楽しい「ミルシル」体験をされることを願っています。

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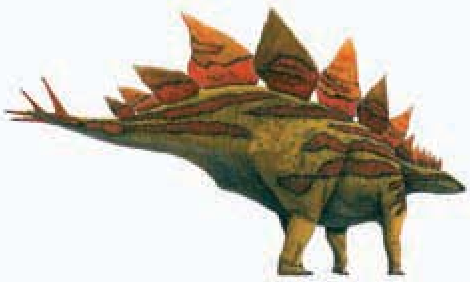

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表紙写真
史上最大級の肉食恐竜として人気の高いティラノサウルス。写真の骨格標本は、骨格の大部分が発掘され「スタン」と名付けられた個体のレプリカ（国立科学博物館に常設展示）。　©国立科学博物館

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原子・分子の制御で社会に役立つ材料科学研究を探る

さまざまな素材の性質を生かし、新しい素材を生み出すことをめざす材料科学。材料科学における世界トップの研究拠点をつくることを目標に、東北大学原子分子材料科学高等研究機構（WPI-AIMR）が組織され、文部科学省の世界トップレベル国際研究拠点形成促進プログラムに選ばれています。

機構の統括、かじ取りをする立場にある山本嘉則機構長に、ご自身の研究生活の中から、現在注目している先端研究とその研究に求めるもの、さらに、どのようにして新しい考え方を生み出していくのかをお聞きしました。

東北大学原子分子材料科学高等研究機構長
山本 嘉則 やまもと よしのり
1986年東北大学理学部教授、1997年九州大学有機化学基礎研究センター教授（併任）を経て、東北大学理学研究科巨大分子解析研究センター長、2006～2007年東北大学副学長、2007年10月より現職。2002年フンボルト研究賞、2006年紫綬褒章、2007年アメリカ化学会賞ほか。

■基礎研究から社会貢献まで

いまはどんな研究が行われているのでしょうか。

まずご紹介したいのは、東北大学総長の井上明久（いのうえあきひさ）先生自らが率いる「金属ガラス」の研究です。通常、金属は原子がきれいに並んだ結晶になっています。ガラスはガラス状構造ですから、きれいになっていない。このため金属ガラスは、引っ張り強度や塑性（そせい）特性は結晶金属以上に堅牢（けんろう）な金属材料になりえます。あとは物理と化学の融合による新しい材料をもとにした電子デバイス（電子部品）ですね。

基本的なところでは液体と固体の境、界面にせまる表面科学です。基礎面では表面科学に力を入れたいと考えています（図1）。それこそが新しい物性発現の基礎になるのではないでしょうか。金属ガラスでも内部にはクラスター構造*があり、それらのクラスター構造間の界面がおもしろいのです。

たとえば私たちのプリンシパル・インベスティゲーター（PI、研究リーダーのこと）のひとり、薄膜（はくまく）電子材料を研究している川崎雅司（かわさきまさし）先生は、「チタン酸ストロンチウム」という絶縁体に、電気を蓄える性質をもつプラスチックを貼りつけて高電圧をかけて温度を下げていくと超伝導現象が起きることを発見し、『ネイチャー・マテリアルズ』誌に発表しました。これまでは一つの物質で超伝導を起こす材料を探していましたが、二つの材料を組み合わせるとまったく考えもしなかった可能性があるわけです。

これまで研究していたのは原子がきれいに並んだ均一な系なのです。でも不均一な物性固体物理では界面が非常に重要になる。それをどのように系統立てていくのか。私たちの機構には界面を連続的に見る技術と、本当に基礎的な理論から応用までの才能が揃（そろ）っています。

もう一つ、東北大学は「出口」に強い。企業と本当に密接なコンタクトをもち、実用に結びつけられる状況にあります。それらを生かして基礎的なところから出口まで広くやっていく。評価もわかりやすく可視化する。そして材料科学の世界でトップの、つまりワールドプレミアインターナショナル（WPI）な研究機関にしたいのです。

「出口」としてはどんなものをつくりたいのですか？

金属ガラスの場合は磁性材料ですね。たとえばゴルフクラブのヘッドに金属ガラスをつけると、飛びすぎて困るくらい反発力が大きくなります。こうした特殊な能力が金属ガラスにあることがわかりました。東大阪の各企業とも連携して、この出口をさらに探っています。あとはマイクロマシンや電子材料、半導体で、製造業の発展の突破口となるようなものを探索しています（図2）。

そして、これらの探索が社会貢献につながることをめざしています。良い論文を書いてアカデミックに評価されるのは当然ですが、それでは終わらない。さらに社会貢献をする。私はイノベー

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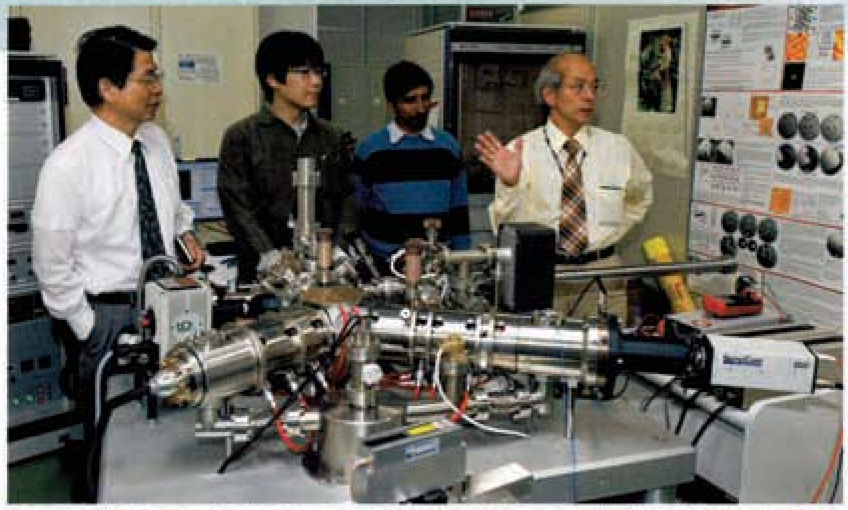

図1　表面科学の研究で用いる低速電子顕微鏡（LEEM）装置。試料表面での原子薄膜や分子薄膜の動きや成長のしかたをナノメートルの分解能で実時間観察することができるため、原子・分子の制御に役立つ。

ション（技術革新）の中にも二つあると考えています。一つはプログレッシブ・イノベーション。たとえばパソコンの記憶容量が連続してどんどん増えるように、発展的に進歩させていく。それも非常に重要です。それからもう一つは、大きなインパクトを与える量子が瞬間的に状態を変えるような量子跳躍です。偶然の発見で引き起こされますが世界を大きく変えるようなものです。両面作戦が重要です。

偶然の発見（セレンディピティ）を引き寄せるためにはどんなしくみづくりが重要ですか。

私が若いころにしていた研究は新しい反応を見つけることでした。しかし、新しい反応なんて考えてできるものじゃない。誰かが偶然加える量を間違えたとか。ノーベル賞を受賞された白川先生の発見はそうした一つの好例ですよね。

ただ、セレンディピティというのは偶然とはちょっと違います。セレンディピティは、ちょっと視点を変えると、「あれ、自分はずっとこっちからばかり見ていたけど、ちょっとこういうふうに見方を変えて、こう考えればいいじゃないか」と思ったりすることなのです。それは融合というか、まったく違う分野の人と話しているときにポッと頭に浮かぶものなんですね。どの先生も過去に体験されていることだろうと思いますが、自分で考えているだけではなかなか出てこない。でもある人と話していると、「あ！」と出てくる。だから融合が大事なんです。本当に頭のいい人で環境が整えばいいのかもしれないけど、自分の分野だけ深く深くやっていても発見は難しいでしょう。つまり発見とは必ずしも頭脳明晰な人だけがやるのではないのです。融合からパッと出てくるようなものが重要なのです。だから私たちはさまざまな専門家を集めて一緒に機会をつくるという方法をとっています。

■原子・分子を巧みに制御する試み

先生ご自身のいまのご研究は？

WPIでは私は拠点長に専任で、研究全体の統括や方向性、事務のマネージメントをやっています。理学部の化学教室で小さいラボはもっています。いまは有機合成の中の触媒反応の研究をしていて、触媒を使ってもっとうまく合成する方法を探しています。

化学分野での研究は、AとBからCをつくる反応の研究と、できあがったCはどんな物質かを調べる研究、この二つがメジャーです。私は反応をつくる研究をやっています。たとえばCはできるけどほかにもいっぱいDができてしまうようなことがあります。Dはいらないという場合、余計なものはつくらずに、Cだけをつくるにはどうすればいいかを探る必要があります。

反応というものはすごいんです。たとえばノーベル賞を獲得したチーグラー・ナッタ（Ziegler-Natta）触媒の研究。エチレンを重合させる触媒です。結晶性の高いポリエチレンが得られる一つの触媒を見つけたことが、いまの化学産業の基礎になった。こういうものは、なかなかありませんよ。

私がいまターゲットにしているのはルイス酸です。ルイス酸とは電子を受け取るもののことです。中学校で習う酸というとH^+でしょう。塩基はOH^-。みなさんが知っているとおりです。ルイス

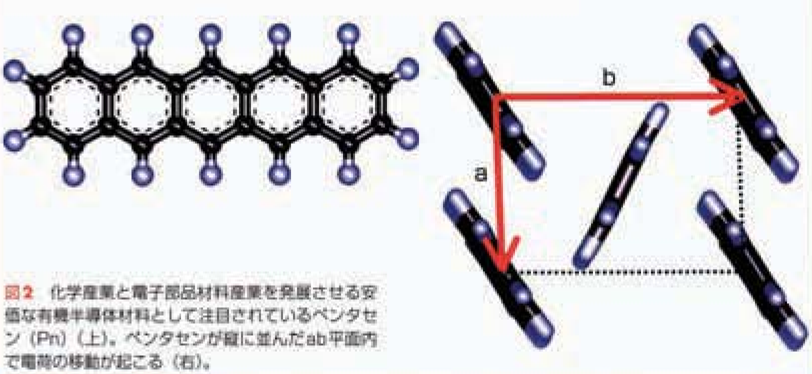

図2　化学産業と電子部品材料産業を発展させる安価な有機半導体材料として注目されているペンタセン（Pn）（上）。ペンタセンが縦に並んだab平面内で電荷の移動が起こる（右）。

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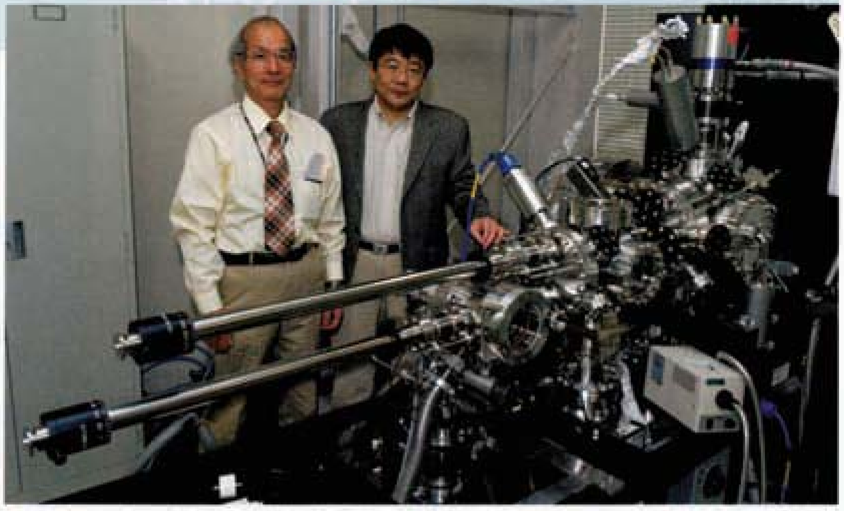

図3 アトム・プローブ電界イオン顕微鏡装置。試料の微少領域、100ナノメートル立方（1万分の1mm立方）程度にある原子の種類と位置を計測して、元素の3次元マッピングができる。

酸というのはもっと広い概念で、周期表の元素の7割か8割くらいのものがみな、酸として働き得るとわかっています。だから従前のH^+の酸ではなく、ルイス酸の性質を使って新しい反応を見つけようと思っています。概念が広がったんですよ。

何でもそうじゃないですか。金縛りでこういうものだと思って見ていたものが、概念を広げてみると違って見える。いまはルイス酸を使って新しいものを見つけようという研究が世界中で増えています。

このような研究で、たとえば、ある有機化合物をつくるために必要な工程が飛躍的に短くなりました。これまでは連続的にA、B、C、Dと反応させてつくっていたものが、Aから直接Dをつくることができるようになったのです。

化学は人に説明しづらい損なところがあって、すべてがちまちましているんですよ。「この場合はこう、この場合はこう」という話になって、ひとくくりで言えない。それがつらいところです。でもこの多様性がおもしろいところでもあります。しかも実社会と結びついています。化学のすごさは、効率性や反応の改良が現実の企業活動と直接結びついているところなのです。

WPIではこれまで以上に原子・分子を巧みに制御することを目的にしていますね。

そうです。化学は分子を制御している。物理は原子を制御しようとしている（図3）。いままでも制御していたわけです。反応とは分子を制御することです。でも、まだ完璧（かんぺき）ではありません。

ものすごく簡単な例を出しましょう。ベンゼン環には6つの手がある。この手全部にそれぞれ違うものを入れる、そんな簡単なことすらできていません。絵に描けるようなこともできてない。大学生の試験に出るようなことすら、それほど簡単ではないのです。私たちはまだ自由に原子・分子を制御したとは言えない。思いのままにはなっていない。まだ制限がある。反応を100％理解したら、すべてできるはずです。だからいまはまだ人類の理解度は60点くらいかもしれない。そこまでも到達していないかもしれない。100点満点になって、初めて何でもできるようになるでしょう。将来はできるようになります。もちろん時間はかかりますよ。化学が始まってまだ200年かそこらです。でも少しずつは進んでいます。やがて必ずすべて理解できるでしょう。 ◫

※クラスター構造―いくつかの原子や分子がひきつけあうことで、ブドウの房（ふさ、cluster）のようにひとつのかたまりになってふるまう構造。一つひとつの原子や分子と違う特徴的な性質を示す。

成功への近道は、自分にうそをつかないこと

自分が何に興味をもっているかを、できるだけ早くはっきりさせることが重要じゃないかな。そして自分にうそをつかない。自分に正直に好きなことをやっている人がやっぱり成功すると思います。若い人には自分に正直に生きてもらいたい。自分のことは自分が一番知っている。自分を見つめて自分に正直に判断することが重要なのではないでしょうか。

私自身が化学に進んだのは時代の流れでした。ですが、有機化学か無機化学かを選ぶときには、どちらが自分の肌に合っているかなと考えて選びました。

化学分野はほかの学問と違って産業界と直結しています。実験台の上でやっていることが、そのまま社会の利潤や企業活動に結びついています。私が若い人に一番言いたいのは、化学は発見するチャンスが大きい分野だということです。たとえば、素粒子の分野で発見するのは大変です。しかし、化学分野は発見することは小さいことかもしれないけれど、非常に多いんですよ。

化学は「ディスカバリー」がキーワードなのです。ディスカバリーするチャンスが大きい分野が化学であり、そしてその発見が社会的に大きな影響を及ぼすチャンスがある分野です。学生でも発見する機会はあります。

問題は発見のあとですね。発見を正当に意味づけするのは経験のある教授の仕事です。ですから広いビジョンと識見をもった教授につくことも重要です。そして学生は、正直に見つけたことを報告する。それは教科書で「Cになる」と書いてあるはずのものがCにならないことかもしれない。「Cのはずだ！」と教授が言うから、「あ、Cになりました」と言って終わってしまったら何にもならない。このコンビネーションがおもしろい。だから私は、化学は頭がいい奴だけではないよと言っています。化学はディスカバリーです。化学のおもしろさはそこ。若い人にもチャンスが転がっているんです。そして良い人につきなさい。このコンビネーションが大切です。

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取材・文：森山和道　写真：飯尾亮悟

U. S. Short-stay Report - A. Chigiri, WPI Administrative Office

I studied in Santa Barbara, California, in the U.S. for about five months, from the end of October 2008 to mid-March in 2009. I attended a language school to brush up my English first, and I worked as an intern at a local city college for the last month or so. I am now writing this report by looking back on my stay in America. I experienced a variety of things there. Of such experiences, I would like to mention one of my bitter experiences.

One day, I went to the city library because I wanted to print out some documents. I asked the person in charge at the library how to use the machine. She explained it to me, but I could not understand her very well, so I asked her again. She explained again. I still could not understand her very well, although I could catch her English this time. I mean I knew what she said, but I still could not "understand" it. "You don't seem to understand what I mean," she said in the end. I remember that I was bitterly disappointed and felt the stress of living abroad.

I later learned that other libraries, including those at schools, adopted similar systems. If it had not been my first time to use one, I probably could have understood her explanation.

This experience taught me that prior, background knowledge is as important as knowing the language to "understand" someone or something. We tend to communicate with the assumption that the person we are talking to probably has the same recognition we have. We have to be careful about this. When you explain your work to foreign researchers, even if you use English to explain in the same manner as you explain to Japanese researchers in Japanese, they might not understand you. You will sometimes need to start from the basic knowledge we Japanese people intrinsically know. In addition, they often respond in unexpected ways. They might not actually understand what you expect them to understand without explanation. Thus, it is more difficult than you think to provide correct information to others.

People from different countries have different cultures and ideas as a matter of course. Roughly speaking, Japan has only one race and one language. Compared with people in Western nations and the U.S., we Japanese seem to be conservative in accepting things that are different. On the other hand, while they have a shorter history, American people accept others and respect individuals. I felt that they had surely established a culture that Japan does not have.

Life in America was not so difficult for an outsider like me. (However, I could never get used to their eating habits until the end of my stay.) I am sure that it was thanks to the background of the country accepting immigrants and the cheerfulness that is characteristic of the people.

I had many pleasant, difficult, and frustrating experiences during my stay in the U.S. I also realized some of the merits of Japan, which I noticed for the first time because of living away from Japan. It is impossible to inscribe all of them here, but each of them was something I could not have experienced in Japan, and they helped me discover something important. I believe all of the experiences will be useful for my work in the future.

アメリカ留学体験記　WPI 事務室　千木里　温子

2008 年 10 月末から 2009 年 3 月中旬まで、約 5 ヶ月間に渡りアメリカ・カリフォルニア州サンタバーバラで海外研修を行ってきた。初めは、語学力向上のため、語学学校へ通い、最後の 1 ヶ月程は、地元の City College でインターンとして働いてきた。アメリカでの生活を振返りながら、この文を書いているが、思い起こせば、様々なことがあった。ここでは、アメリカでの苦い体験を紹介したい。

ある日、市の図書館へ行った。プリントアウトしたいものがあったからだ。図書館で係員へ使い方を尋ねた。すると、彼女は使い方を説明してくれた。しかし、私はよく理解できなかったので、もう一度尋ねてみた。すると、彼女は、また説明してくれた。それでも、私はよく分らなかった。彼女の英語は、聞き取れた。彼女の言っていることは分かったのだ。けれども、“理解”ができなかったのだ。結局、「あなたは、私の言っていることが分からないみたいね。」と言われた。その時は、悔しかったことと海外で生活することのストレスを感じたことを覚えている。

後になって分かったことだが、学校などの他の図書館でも同じ様なシステムになっており、おそらく初めて利用するのでなければ、説明されたことが理解できていたであろう。

理解は、言語＋予備知識／背景知識＝理解　ということを、身をもって体験した出来事であった。普段、無意識のうちに自分の持っている認識を相手も持っているだろうという共通理解を前提に話を進めがちであるが、実は、気を付けなければいけない。仕事で外国人研究者に対して説明する場合、日本人に対して説明することと同じことをすべて英語で説明すれば、相手が理解してくれるとは限らない。時には、私たち日本人なら前提として当然持っているであろう知識から、説明しなければならないこともあるはずだ。また、自分が思ってもみないような答えが相手から返ってくることも、しばしばある。さらに、「この程度のことなら、説明しなくても分かるだろう」と思うことも、実際は伝わらなかったりと、自分が思っている以上に相手に何かを正確に伝えることは難しいことだ。

国が違えば、文化も違うし、考え方も当然異なる。正確に言えば違うが、日本は、同一民族・同一言語いう環境のため、西欧米諸国に比べ、自分とは異なるものを“受け入れる”ことに不寛容になりがちである。その点、アメリカは、歴史こそ浅いが、他者を受け入れ、しかしながら、個人を重んじる、日本にはない文化が確かに根付いていると感じた。

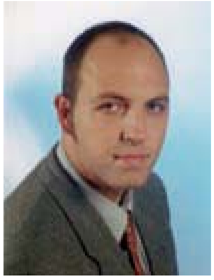
アメリカでの生活は、余所者の私でも、さほど難しいものではなかった。(ただし、アメリカの食生活には、最後まで馴染めなかったが。) きっと、移民を受け入れてきた土壌と、あの人陽気な国民性がそう感じさせるのかもしれない。

アメリカ生活を通して、嬉しかったこと、大変だったこと、悔しい思いをしたことがたくさんあった。また、日本を出て生活をしてみて初めて感じた、日本の良さもあった。そのすべてを、ここで書ききることはできないが、その一つ一つが日本では経験し得ないことであり、またその一つ一つに何らかの発見があった。そのすべてが、これからの仕事に生きると思う。

WPI-AIMR
Newly Appointed Research Staff

Curriculum Vitae

Dirk EHRENTRAUT



Associate Professor
E-mail: ehrentraut@wpi-aimr.tohoku.ac.jp

ACADEMIC:

1995 Diploma (German equivalent to M.S.), Humboldt-University to Berlin, Germany
2003 Dr. Sci. in Swiss Federal Institute of Technology Lausanne (EPFL), Switzerland

PROFESSIONAL EXPERIENCE:

1995-1997 Researcher, the Leibniz-Institute of Crystal Growth, Germany
1997-2003 Researcher, EPFL, Switzerland
2004-2007 Visiting Associate Professor, Tohoku University
2007-2008 Visiting Professor and Research Professor, Tohoku University
2008-present Advisor to companies
2009 Invited Senior Researcher, Institute of Materials Research and Engineering, A-STAR, Singapore
2009-present Associate Professor, WPI-Advanced Institute for Materials Research, Tohoku University

CURRENT RESEARCH:

✦Solvothermal technology to fabricate high-quality group-III nitride crystals
✦ZnO bulk and film as semiconducting scintillator

Akihiko HIRATA

Assistant Professor
E-mail: hirata@wpi-aimr.tohoku.ac.jp
URL: http://www.wpi-aimr.tohoku.ac.jp/chen_labo/

ACADEMIC:

1998 B.E. in Science and Engineering, Waseda University, Japan
2000 M.E. in Science and Engineering, Waseda University, Japan
2003 Dr. Eng. in Science and Engineering, Waseda University, Japan

PROFESSIONAL EXPERIENCE:

2003-2006 Research Associate, Institute of Scientific and Industrial Research, Division of Advanced Materials Science and Technology, Osaka University, Japan
2006-2009 Assistant Professor, Institute of Scientific and Industrial Research, Division of Advanced Materials Science and Technology, Osaka University, Japan
2009-present Assistant Professor, Advanced Institute for Materials Research, Tohoku University

CURRENT RESEARCH:

✦ Structural studies of alloys and intermetallics using transmission electron microscope
✦ Studies on local atomic structure and crystallization process in metallic glasses

Keith MCKENNA

Assistant Professor
E-mail: k.mckenna@ucl.ac.uk
URL: www.cmmp.ucl.ac.uk/~kpm/people/keith.htm

ACADEMIC:

2001 M. Phys in Physics, University of Leeds, UK
2005 Ph.D., University of Leeds, UK

PROFESSIONAL EXPERIENCE:

2005-2009 Research Fellow, University College London, UK
2009 Visiting Senior Research Fellow, Ångstrom Laboratory, Uppsala University, Sweden
2009-present Assistant Professor, WPI-Advanced Institute for Materials Research, Tohoku University

CURRENT RESEARCH:

My research aims to understand the properties and functionality of nanoscale and nanostructured materials by developing and applying a range of theoretical and computational approaches. Current research topics include:

✦ Defect segregation and charge trapping at grain boundaries in polycrystalline materials
✦ Electronic and optical properties of metal-oxide nanocrystallites
✦ Dynamics, structure and properties of metallic nanoparticles in liquid and gaseous environments
✦ Electronic properties of supported metal clusters

Takeo OHSAWA

Assistant Professor
E-mail: ohsawa@wpi-aimr.tohoku.ac.jp
URL: http://www.wpi-aimr.tohoku.ac.jp/hashizume_labo/home.html

ACADEMIC:

2001 B.E. in Engineering, Tokyo Institute of Technology, Japan
2003 M.E. in Engineering, Tokyo Institute of Technology, Japan
2006 Dr. Eng., Tokyo Institute of Technology, Japan

PROFESSIONAL EXPERIENCE:

2006-2007 Postdoctoral fellow, National Institute for Materials Science (NIMS), Japan
2007-2009 Post Doctoral Research Associate, Pacific Northwest National Laboratory (PNNL), USA
2009-present Assistant Professor, WPI-Advanced Institute for Materials Research, Tohoku University

CURRENT RESEARCH:

✦ Epitaxial growth and properties at surfaces and interfaces of widegap oxides
✦ Atomic-scale studies of epitaxial oxides using scanning probe microscopy

Curriculum Vitae

Toyoto SATO

Assistant Professor
E-mail: toyoto@imr.tohoku.ac.jp

ACADEMIC:

2001 B.E. in Mechanical Engineering, Shibaura Institute of Technology, Japan
2006 Ph.D. in Chemistry, Stockholm University, Sweden

PROFESSIONAL EXPERIENCE:

2006-2007 Postdoctoral Fellow, Department of Chemistry and Biochemistry, Arizona State University, USA
2007 Postdoctoral Fellow, Institute for Materials Research, Tohoku University, Japan
2007-2009 COE Fellow, Institute for Materials Research, Tohoku University, Japan
2009-present Assistant Professor, WPI Advanced Institute for Materials Research, Tohoku University

CURRENT RESEARCH:

✦Hydrogen storage materials
✦Structural investigations by powder x-ray/neutron diffraction

Jun TANG

Assistant Professor
E-mail: tangjun@sspns.phys.tohoku.ac.jp
URL: http://sspns.phys.tohoku.ac.jp/

ACADEMIC:

1996 B.S. in Inorganic Chemistry, Chongqing University of Science and Technology, P.R.China
2003 M.S. in Inorganic Chemistry, Sichuan University, P.R.China
2006 Dr. in Particle Physics and Nuclear Physics, Institute of High Energy of Physics, Chinese Academy of Sciences, P.R.China

PROFESSIONAL EXPERIENCE:

1996-2000 Assistant engineer, South-west Bureau of Geological Prospecting, P.R.China
2007-2009 JSPS Fellow, Department of Physics, Tohoku University, Japan
2009-present Assistant Professor, WPI Advance Institute for Materials Research, Tohoku University

CURRENT RESEARCH:

✦Clathrate-structured thermoelectric materials, single crystal synthesis of iron-based superconductor, synchrotron radiation x-ray photoelectron spectroscopy.

Curriculum Vitae

Thomas TREVETHAN

Assistant Professor
E-mail: tomt@wpi-aimr.tohoku.ac.jp

ACADEMIC:

2002 M.S. in Physics, University College London, UK
2005 Ph.D. in Physics, University College London, UK

PROFESSIONAL EXPERIENCE:

2005-2008 Research Fellow, University College London, UK
2009-present Assistant Professor, WPI Advanced Institute for Material Research, Tohoku University

CURRENT RESEARCH:

✦Long-timescale simulation of complex systems
✦Electronic structure of adsorbed molecules and clusters
✦Multi-scale modeling of scanning probe microscopy experiments

Aleksandr CHEKHOVSKIY

Research Associate

ACADEMIC:

1996 Specialist in Physics, Novosibirsk State University, Russia
2004 Ph.D. in Physics, Institute of Semiconductor Physics, Russia

PROFESSIONAL EXPERIENCE:

2004-2005 Postdoctoral Researcher, Advanced Telecommunication Research Institute, Japan
2005-2008 Postdoctoral Researcher, Institute of Industrial Science, The University of Tokyo, Japan
2008-2009 Senior Researcher, Institute of Physics and Applied Math., Ural State University, Russia
2009-present Research Associate, WPI-Advanced Institute for Materials Research, Tohoku University

CURRENT RESEARCH:

✦Materials and their nano-fabrication for new system integration of MEMS/NEMS.

Na CHEN

Research Associate
E-mail: chenn@wpi-aimr.tohoku.ac.jp

ACADEMIC:

2001 B.S. in Mechanical Engineering, Tsinghua University, P.R. China
2005 M.S. in Materials Science and Engineering, Tohoku University
2008 Ph. D. in Materials Science and Engineering, Tsinghua University, P.R. China

PROFESSIONAL EXPERIENCE:

2008-present Research Associate, WPI advanced Institute for Materials Research, Tohoku University

CURRENT RESEARCH:

✦ Preparation of phase separating bulk metallic glasses and research on their mechanical

Arunabhiram CHUTIA

Research Associate
E-mail: arun@wpi-aimr.tohoku.ac.jp

ACADEMIC:

2001 B.S. in Chemistry, Gauhati University, India
2004 M.S. in Chemistry, Gauhati University, India
2008 Ph.D. in Theoretical and Computational Chemistry, Tohoku University

PROFESSIONAL EXPERIENCE:

2009-present Research Associate, WPI-Advanced Institute for Materials Research, Tohoku University

CURRENT RESEARCH:

✦ Theoretical and computational materials science
✦ Parameterization of empirical potentials for metallic alloys using quantum based methods

Takeshi HIGUCHI

Research Associate
E-mail: higuchi@mail.tagen.tohoku.ac.jp
URL: http://poly.tagen.tohoku.ac.jp

ACADEMIC:

2003 B.S. in Applied Chemistry, Tokyo University of Science, Japan
2005 M.S. in Chemistry, Hokkaido University, Japan
2008 Dr. Sci., Hokkaido University, Japan

PROFESSIONAL EXPERIENCE:

2006-2007 Junior Research Associate, RIKEN, Japan
2007-2008 JSPS Research Fellow (DC2), Hokkaido University, Japan
2008-2009 JSPS Research Fellow (PD), Tohoku University, Japan
2009-present Research Associate, WPI Advanced institute for Materials Research, Tohoku University

CURRENT RESEARCH:

✦Investigation of the polymer nanoparticles prepared from various kinds of polymers, which include polymer blends and block copolymers.
✦Fabrication of the novel nano-materials by using self-organization and self-assembly processes based on polymer science.

Takeshi KOMINO

Research Associate
E-mail: komino@atom.che.tohoku.ac.jp

ACADEMIC:

2004 B.S. in Chemistry, Gakushuin University, Japan
2006 M.S. in Chemistry, The University of Tokyo, Japan
2009 Dr. in Chemistry, The University of Tokyo, Japan

PROFESSIONAL EXPERIENCE:

2009-present Research Associate, WPI Advanced Institute for Material Research, Tohoku University

CURRENT RESEARCH:

✦Fabrication and characterization of organic semiconductor devices

E. P. SAJITHA

Research Associate
E-mail: sajitha@wpi-aimr.tohoku.ac.jp

ACADEMIC:
1997 B.S. in Physics, University of Calicut, India
1999 M.S. in Physics, Kannur University, India
2007 Ph. D. in Physics, Indian Institute of Science, India

PROFESSIONAL EXPERIENCE:
2007-2008 Research Associate, Institute Nanoscience Initiative, Indian Institute of Science, India
2009-present Research Associate, WPI Advanced Institute for Material Research, Tohoku University

CURRENT RESEARCH:
✦ Magnetotransport properties of magnetic thin films and multilayers.

Katsuaki SUGAWARA

Research Associate
E-mail: k.sugawara@arpes.phys.tohoku.ac.jp

ACADEMIC:
2004 B.S. in Applied Physics, Tokyo University of Science, Japan
2006 M.S. in Physics, Tohoku University
2009 Dr. Sci. in Physics, Tohoku University

PROFESSIONAL EXPERIENCE:
2007-2009 JSPS Fellow, Department of Physics, Tohoku University
2009-present Research Associate, WPI Advanced Institute for Material Research, Tohoku University

CURRENT RESEARCH:
✦ Electronic structure of graphene and related materials (graphite, graphite intercalation compounds, fullerene, and carbon nanotubes)
✦ Electronic structure of one-dimensional quantum metal wires and ultrathin metallic film

Newly Appointed Adjunct Professors

Curriculum Vitae of Adjunct Professor

Tsuguo FUKUDA

PRIMARY AFFILIATION:

President

Fukuda Crystal Laboratory Co. Ltd.

6-6-3 Minami-Yoshinari

Aoba-ku, Sendai 989-3204

TEL: +81-22-303-0170

FAX:+81-22-303-0171

E-mail: ts-fukuda@wpi-aimr.tohoku.ac.jp

ACADEMIC:

1964	B.S. in Earth Science, The University of Tokyo, Japan
1971	Dr. Sci., The University of Tokyo, Japan

PROFESSIONAL EXPERIENCE:

1964-1987	Researcher, TOSHIBA Co. Ltd., Japan.
1987-2002	Professor, Institute for Materials Research, Tohoku University
2002-2007	Guest Professor of Tohoku University
2002-2009	Emeritus and Research Professor of Tohoku University
2002-present	President, Fukuda Crystal Laboratory Co. Ltd., Japan
2008-present	Adjunct Professor, WPI-AIMR, Tohoku University

RECOGNITION:

- ✦The Kahoku Culture Awards (2008)
- ✦Jan Czochralski Gold Medal (2007)
- ✦The Japanese Association for Crystal Growth Technological Contribution Award(2004)
- ✦Medal for the acknowledgements of scientific value and research activities Awarded by Rhône Administrative Department’s Council(2000)
- ✦Medaille de la ville de Lyon (Medal of Lyon city)(2000)
- ✦The Japanese Association for Crystal Growth Paper Prize(1987)
- ✦The Science and Technology Agency of Japan Minister Prize(1985)
- ✦The Crystallographic Society of Japan Prize(1984)
- ✦The Okochi Memorial Prize(1980)

Curriculum Vitae of Adjunct Professor

Hiroshi JINNAI

PRIMARY AFFILIATION:

Associate Professor

Department of Macromolecular Science and Engineering

Kyoto Institute of Technology

Matsugasaki, Sakyo, Kyoto 606-8585 Japan

Office: +81-75-724-7846

Fax: +81-75-724-7770

E-mail: hjinnai@kit.ac.jp

ACADEMIC:

1988 B.S. in Polymer Chemistry, Kyoto University, Japan

1990 M.S. in Polymer Chemistry, Kyoto University, Japan

1993 Ph.D. in Polymer Chemistry, Kyoto University, Japan

PROFESSIONAL EXPERIENCE:

1993 JSPS Research Fellow, Kyoto University, Japan

1993-1998 Group Leader, ERATO Hashimoto Polymer Phasing Project, Japan Science and Technology Corporation, Japan

1998-2002 Lecturer, Kyoto Institute of Technology, Japan

2002-present Associate Professor, Kyoto Institute of Technology, Japan

RECOGNITION:

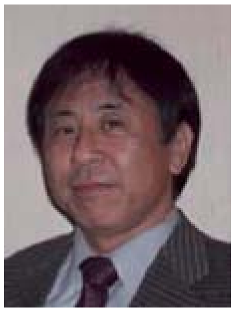
✦SPSJ (The Society of Polymer Science, Japan) Wiley Award (2006)

✦Ernst-Ruska-Prize 2007 (International Award, The German Society for Electron Microscopy) (2007)

✦Award for Persons of Merit in Industry-Academia-Government Collaboration in FY2008 (Minister of Education, Culture, Sports, Science and Technology Award)(2008)

Curriculum Vitae of Adjunct Professor

Hidemi SHIGEKAWA



PRIMARY AFFILIATION:

Professor

Institute of Applied Physics,

University of Tsukuba,

Tsukuba, 305-8573 Japan

E-mail: hidemi@ims.tsukuba.ac.jp

ACADEMIC:

1978	B.S. in Applied Physics, The University of Tokyo, Japan
1980	M.S. in Applied Physics, The University of Tokyo, Japan
1986	Ph.D. in Applied Physics, The University of Tokyo, Japan

PROFESSIONAL EXPERIENCE:

1980-1989	Assistant professor, Department of Applied Physics, The University of Tokyo, Japan
1987-1988	Resident Visitor, AT&T Bell Laboratories, Brookhaven National Laboratory, USA
1989-1994	Lecturer, Institute of Materials Science, University of Tsukuba, Japan
1990-2002	Lecturer, Department of Applied Physics, University of Tokyo, Japan
1990-1991	Visiting researcher, Institute for Materials Research, Tohoku University, Japan
1994-2003	Associate professor, Institute of Materials Science, University of Tsukuba, Japan
1994-present	Lecturer, Department of Physics, Chuo University, Japan
1995-1998	Project leader, Nanology, TARA Center, University of Tsukuba, Japan
1997-2000	Associate professor, Department of Chemistry and Biology, The University of Tokyo, Japan
2003-present	Professor, Institute of Applied Physics, University of Tsukuba, Japan

CURRENT RESEARCH:

Scanning probe microscopy and related techniques, Surface science, Molecular science, Nanotechnology, Nano-structured materials, Ultrafast phenomena, Semiconductor physics, Phase transition.

RECOGNITION:

✦Paper Award, The Surface Science Society of Japan, Japan (1991, 1998)

✦Project leader, CREST-JST, Japan (2004-2009)

✦Chief, Thin Film and Surface Division, The Japan Society of Applied Physics, Japan (2006-2008)

✦Director, The Japan Society of Applied Physics, Japan (2007-2009)

✦Editor-in-Chief, OYO BUTSURI, The Japan Society of Applied Physics, Japan (2008-2009)

✦Depute Editor-in-Chief, HYOMEN KAGAKU, The Surface Science Society of Japan, Japan (2008-)

Curriculum Vitae of Adjunct Associate Professor

Yukiko YAMADA-TAKAMURA

PRIMARY AFFILIATION:

Tenure-track Lecturer

School of Materials Science

Japan Advanced Institute of Science and Technology (JAIST)

1-1 Asahi-dai, Nomi, Ishikawa 923-1292, Japan

Tel: +81-761-51-1581/ Fax: +81-761-51-1149 (MS office)

E-mail: yukikoyt@jaist.ac.jp

ACADEMIC:

1993 B.E. in Materials Science, The University of Tokyo, Japan

1995 M.E. in Metallurgy, The University of Tokyo, Japan

1998 Ph.D. in Metallurgy, The University of Tokyo, Japan

PROFESSIONAL EXPERIENCE:

1995-1998 Research Fellow of the Japan Society for the Promotion of Science (DC1)

1998-2001 Research Fellow of the Japan Society for the Promotion of Science (PD)

1999-2000 Guest Scientist at Max-Planck-Institut für Plasmaphysik, Germany

2001-2002 Research Associate of the Japan Society for the Promotion of Science

2002-2006 Research Associate, Institute for Materials Research, Tohoku University

2006-present Tenure-track Lecturer, School of Materials Science and Research Center for Integrated Science, JAIST, under the project "Development of Personnel System for Young Researchers in Nanotechnology and Materials Science" supported by Special Coordination Funds for Promoting Science and Technology (MEXT)

RECOGNITION:

✦ Honda Memorial Foundation: The 46th Harada Young Research Award (2006)

WPI-IFCAM

WPI-IFCAM

International Frontier Center for Advanced Materials (IFCAM) was inaugurated in Institute for Materials Research (IMR), Tohoku University, in October 2001 to function, simply stated, as the world-first “materials science think-tank.” With wise steering by past directors, IFCAM has been performing well in its mission until now by 1) bringing in many world-renown researchers to IMR in the greater field of materials science/engineering, 2) enlightening / encouraging young scientists, post-doctoral fellows and graduate students by organizing and supporting workshops/summer schools, and 3) establishing and coordinating IFCAM branch offices around the world, including those in Cambridge University, Harvard University, Stanford University, and Institute of Physics, P.R. China.

We were also so fortunate to have had a government initial-equipment fund in 2002 to acquire several advanced tools, such as 3-dimention atom-probe tomography (3-D AP) and low energy electron microscope (LEEM). With untiring effort of able faculty members and their staff / graduate students, the LEEM program of IFCAM, for instance, has quickly become one of the most active and successful research centers in the world.

Realizing that IFCAM and newly established WPI-AIMR have essentially the same mission: namely, further promote international collaboration and cooperation in innovative research on advanced materials on a global bases, IFCAM was transferred from IMR to WPI-AIMR, effective of April 2008.

Briefly stated, WPI-IFCAM has following function and service.

1 . Visiting Professorship

2. Workshops / Summer schools

Ⅰ. Visiting Professorship

Qualified researchers who may be interested in IFCAM visiting professorship should first contact the WPI principal investigator(s) of the related research fields. Your contact PIs will initiate the further process to materialize the joint research.

(1) Tenure: For a period of minimum one month to a maximum of 3 months.

(2) Financial: The salary varies, depending on the qualifications, based on the Tohoku University regulations. Roughly speaking, “full professor” receives Y600,000 per month and “Associate Professor” receives Y500,000 per month.

Ⅱ. Workshops / Summer schools

WPI-IFCAM will financially support the workshops and summer schools, if the scientific aims are along the WPI-AIMR missions. For more information, please contact WPI Administrative Office.

2009–2010 WPI-IFCAM Visiting Scholars

	Candidate	Host	Position	Term							Term	Affiliation	Position	Graduated from	Diploma	Nationality	Age	Research Proposal
				From			～	Through										
1	**CHALLAPALLI, Suryanarayana**	A. Inoue	Visiting Professor	H21	**4**	13	～	H21	**7**	12	3m	University of Central Florida	Professor	Banaras Hindu University, India	Ph. D	USA	64	Synthesis, Processing, and Characterization of Nanomaterials and Bulk Metallic Glasses
2	**LEE, Seung-Hun**	K. Yamada	Visiting Professor	H21	**5**	28	～	H21	**7**	15	1m18d	University of Virginia	Professor	Johns Hopkins University, USA	Ph. D	South Korea	45	Novel quantum phenomena in complex transition metal oxides
3	**TEIZER, Winfried**	T. Adschiri	Visiting Associate Professor	H21	**6**	1	～	H21	**8**	31	3m	Texas A&M University	Associate Professor	University of Massachusetts, USA	Ph. D	Austria	38	Biophysical applications of magnetic nanoparticles
4	**JIA, Jinfeng**	M. Chen & T. Hashizume	Visiting Professor	H21	**7**	1	～	H21	**9**	30	3m	Tsinghua University	Professor	Peking University, P.R. China	Ph. D	P.R. China	43	Growth mechanism of Pb film by LEEM and LTSTM
5	**ORESHKIN, Andrei Ivanovich**	T. Hashizume	Visiting Professor	H21	**9**	3	～	H21	**12**	2	3m	Moscow State University	Research Fellow	Moscow State University, Russia	Ph. D	Russia	42	
6	**CHEN, Ho Sou**	A. Inoue	Visiting Professor	H21	**10**	1	～	H21	**11**	30	2m	Bell Laboratories, Lucent Technologies	Adviser	Harvard University, USA	Ph. D	USA	76	Development of mechanical properties by fundamental approaches of fragility and relaxation control in metallic glasses.

WPI Activity Report of Professor Dong-Ryul Jeon
January 16th, 2009- March 15th, 2009
Host: Professor Tomihiro Hashizume

I got on the train at the airport bound for Sendai station, and the familiar voice of the recorded announcement reminded me that I was in Sendai. The streets, the stores, the trees, the people of Sendai looked familiar as I got out of the bus. Since this was my first visit to the World Premier Institute after it had been established last year, I felt more exciting. When I saw the completely renovated WPI building, I could hardly believe that it used be the oldest faded university building in the *kitamon* area and I could read the strong will of the WPI leaders. The old but entirely new building was where I was going to work for two months.

The research I planned to conduct during this visit was about the organic thin film, especially the organic film/metal interface for application to electronic devices in mind. The structural and electrical properties of the metal/organic interface are a key issue for successful control of organic devices such as organic solar cells, organic light emitting diodes and organic thin film transistors. I liked to narrow my interest in the study of interface using electric force microscopy (EFM) and simultaneous measurement of *I-V* characteristics in order to investigate the charge accumulation and depletion at the Al/pentacene/Au interface. This system exhibits a diode characteristic due to the Schottky barrier forming at the Al/pentacene interface and the ohmic contact at the pentacene/Au interface. Interestingly, we found that the current kept increasing in the forward direction even though the bias voltage is fixed. If we can use EFM to measure the charge at the interface during the current change, it would help understand how the interface affects the device performance and we can control the device by modifying the interface.

Although the lab was not fully ready because moving to the new facility was not completed yet, I could manage to conduct experiments. The EFM phase image of the cross section of the sample showed the amount of interface charge which changed with time and was dependent on the bias voltage polarity. When negative bias voltages below -2 V were applied to the Al electrode, the current decreased momentarily and then increased. The EFM phase image in this case showed a strong initial charge accumulation at the Al/pentacene interface which depleted slowly with the increase of the current. On the other hand, when the positive bias voltages was applied to the Al electrode, the current decreased immediately and stopped, and the EFM phase image did not show any change. The overall results indicated that the barrier deformation due to the charge accumulation at the Al/pentacene interface is the reason for the current increase with time for the forward bias.

Cross sectional EFM was utilized in the past for the study of dopant density of Si and compound semiconductors, but not for the organic/metal interface. Our cross sectional EFM for the study of Al/pentacene/Au was the first such attempt.

The initial success I achieved from this visiting research at WPI is very encouraging for it confirmed a possibility that cross sectional EFM can be a useful diagnostic tool in the development of organic electronic device by interface modification. For all this, I am deeply indebted to professors Sakurai, Hashizume, Hitotsugi who invited me and who became my host. Every member of Hashizume group was a great help. Without them, I could not have done anything. I also thank Ms. Sato at the WPI office for the administrative work. I look forward to visiting WPI-AIMR again in the near future. It always stimulates me and creates something out of me.

WPI Activity Report of Dr. Satyabrata Raj
January 19th, 2009- March 30th, 2009
Host: Professor Takashi Takahashi

I visited WPI-AIMR, Tohoku University, Sendai Japan from 19th Jan, ~ 30th March, 2009. I strongly collaborated with Prof. Takahashi's group to investigate few very interesting unsolved problems. It could be possible only because of the availability of world-class high-resolution angle-resolved photoemission (ARPES) spectrometer at Tohoku laboratory.

We investigated evolution of Charge Density Wave (CDW) state in monophosphate tungsten bronze, *namely* $P_4W_{12}O_{44}$ and $P_4W_{14}O_{50}$, a very interesting low-dimensional class of compounds. Phosphate tungsten bronzes, (PO2)4(WO3)2m have created enormous interest due to its strong anisotropic structure builds up with chains. The essential building blocks are perovskite-type layers made up of WO6 octahedral and linked across PO4 tetrahedral. Due to this layer structure, all the phosphate tungsten bronzes have a 2D character. We have carried out high-resolution angle-resolved photoemission spectroscopy on mono-phosphate tungsten bronze $P_4W_{12}O_{44}$ and $P_4W_{14}O_{50}$, $[(PO_2)_4(WO_3)_{2m}$, $m = 6$ and 7] at well below and above the CDW transition temperature ($T_{CDW1} = 60$ K). The experimentally determined band structure from ARPES is compared with the band calculation. We mainly observed three bands in the vicinity of E_F. A good agreement has been found between these electronic bands and the theoretical calculation. We found that the intensity at E_F is reduced at well-nested part whereas the other part of FS contributes to its conductivity, making the system overall metallic even below T_{CDW}.

Another very interesting system we investigated is the metal-insulator transition on sodium tungsten bronze, Na_xWO_3. We carried out high-resolution angle-integrated measurements on Na_xWO_3 ($x = 0.075$) with variation of temperature. We found that the insulating behavior in low Na_xWO_3 doping arises from the Anderson localization of all the states near E_F due to the strong disorder caused by inserting Na in a WO_3 lattice. Simultaneously a soft Coulomb gap arises at E_F and consequently the density of states (DOS) vanishes at E_F. This gap arises due to the long-range interaction of the electrons trapped due to the strong disorder caused by Na doping. We fit the experimental DOS near E_F with a function of $(E - E_F)^{\alpha} + C$, where C is a constant, and found that α is close to 2. Hence, we conclude that the presence of disorder together with long-range Coulomb interactions leads to the formation of a soft Coulomb gap at E_F in this system, this being responsible for its insulating properties.

Announcement

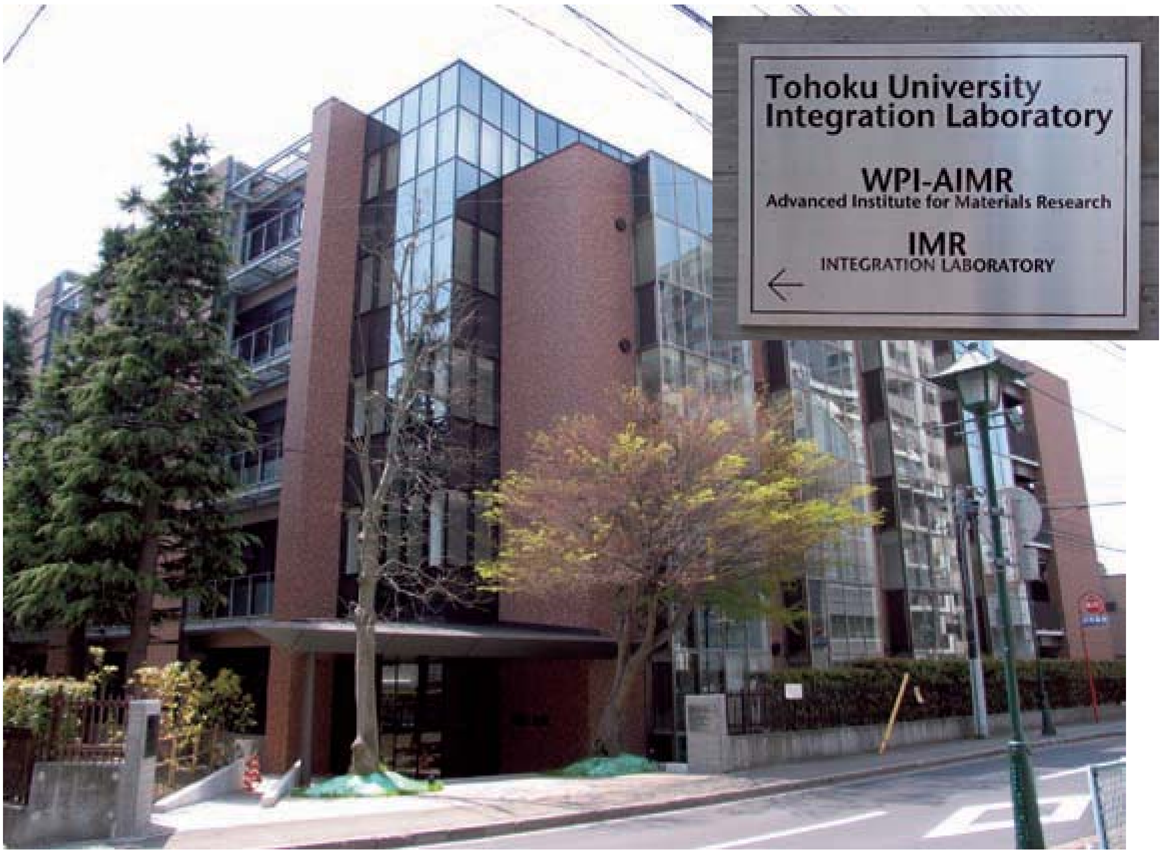
Open House of Integration Laboratory of the WPI-AIMR On May 22, 2009

The first and second phase construction of the WPI-AIMR laboratories consisting of five-story with some 9,000 square meters has been completed recently and we will be having an opening ceremony on the afternoon of Friday, May 22, 2009. The new facility is located on the north-east corner of IMR (Institute for Materials Research) campus.

The first part was completed in August 2008 with 3,650 square meters and a large portion of it has already been occupied by WPI-AIMR research groups. Newly acquired advanced research tools, such as UNISOKU low-temperature, high-magnetic field scanning probe microscope (SPM) and JEOL Field-emission type transmission electron microscope (TEM), have been installed and are in operation.

The second part of the project involving 5,350 square meters connected to the first phase building was completed at the end of March 2009. Currently, the relocation of instrumentation and offices is occurring and this transition will be completed by the second week of May, the end of the Golden Week.

Professor Venky Narayanamurti, founding Dean of the School of Engineering and Applied Science at Harvard, and WPI-AIMR Advisory Board member, will be a featured speaker among other distinguished speakers at the ceremony.



WORKSHOP WPI-INPG-EUROPE

Grenoble, 25 - 28 August 2009

Metallic glasses are currently at the cutting edge of metal research and will be the subject of a meeting of experts in Grenoble August 25-28, 2009 (WPI-EUROPE-INPG). Leading European, Japanese and international scientists will present keynote lectures and young scientists will report on the latest results. All scientists active in the field are cordially invited to participate. In order to enhance focused interactions, we plan to limit the number of participants to 100.

If needed a quota will be set up to promote participation of female scientists and students.

For more information, please visit the following website :

http://wpi-europe.inpg.fr/index_files/WPI_Europe_Home.htm

Keynote (tentative):

A.Inoue (WPI-Japan)- President of Tohoku University

H. Rohrer (Switzerland)- Chairman of WPI International Advisory Board

H. Gleiter (Germany)- Member of WPI International Advisory Board

W.L. Johnson (USA)	D. Miracle (USA)	J. Eckert (Germany)	J. Loeffler (Switzerland)
A.R. Yavari (WPI-France)	B. Cantor (UK)	C. A. Schuh (USA)	W. H. Wang (China)
A.L. Greer (WPI-UK)	K. Samwer (Germany)	W.J. Botta (Brazil)	L. Battezzati (Italy)
A. Argon (USA)	E. Ma (USA)	M. Atzmon (USA)	P. Liaw (USA)
F. Spaepen (USA)	T.G. Nieh (USA)	T. Kulik (Poland)	C. Kiminami (Brazil)
T. Hashizume (WPI-Japan)	C. Volkert (Germany)	G. Evangelakis (Greece)	T. Rouxel (France)
J. Perepezko (USA)	H. Fecht (Germany)	T. Zhang (China)	Do Hyang Kim (Korea)

Oral (invited-tentative):

M. Barrico (Italy)	D. Louzguine (WPI-Japan)	N. Lupu (Romania)	D. Dudina (France)
G. Vaughan (France)	Y. Yokoyama (WPI-Japan)	F. Dalla Torre (Switzerland)	M. Aljerf (France)
R. Portier (France)	A. Vinogradov (Japan)	K. Nakajima (WPI-Japan)	K. Hajlaoui (Tunisia)
A. Takeuchi (WPI-Japan)	E. Blanquet (France)	C. Lekka (Greece)	T. Hitosugi (WPI-Japan)
V. Keryvin (France)	J.J. Blandin (France)	J. Antonowicz (Poland)	H. Komatsu (WPI-Japan)
M-W. Chen (WPI-Japan)	M. Stoica (Germany)	K. Georgarakis (WPI-France)	K. Nakayama (WPI-Japan)

Contact:

Secretariat WPI-Europe 2009

SIMAP- INPG, 1130 rue de la Piscine, 38402 St Martin d'Hères, France

Tel/Fax:+33(0)4.76.82.65.16

e-mail: euronano@minatec.inpg.fr

Deadlines:

Abstract submission: **June 15th, 2009**

Electronic registration: **June 15th, 2009**

Junior Faculty/Post-Doctoral Positions

Tohoku University
WPI-AIMR

Effective October 1, 2007, Tohoku University created a new Research Institute, the Advanced Institute for Materials Research (AIMR), based on an initiative of the Japanese Department of Education (MEXT) for World Premier International Research Center Initiative (WPI) to bring together scientists involved in research on nano-science and technology.
In the 21st century, material science, broadly defined as the study of how complex/novel properties arise in matters/materials from the interactions of individual components, will comprise of inter-discipline collaboration.
(*HTTP://WWW.WPI-AIMR.TOHOKU.AC.JP*).
Over the next few years, as many as one hundred new appointments at the levels of post-doctoral fellows and junior faculty will be available. All innovative researchers are welcome as active promoters of basic/applied sciences in the fields of physical metallurgy, physics, chemistry, precision mechanical engineering and electronic / informational engineering.

We are continuously looking for excellent applicants throughout the year.
Please submit
1) **a curriculum vitae,**
2) **research proposal (<3,000 words),**
3) **summary of previous research accomplishments (<2,000 words),**
4) **copies of 5 significant publications, and**
5) **2 letters of recommendation**

by email to **yoshi@mail.tains.tohoku.ac.jp,**
sakurai@imr.tohoku.ac.jp, and
wpi-office@bureau.tohoku.ac.jp.
All files must be submitted electronically in pdf or Word format.

Applications from, or nominations of, women and minority candidates are encouraged. Tohoku University WPI-AIMR is an affirmative action / equal opportunity employer.

Graduate Student Scholarship In Materials Science/Engineering

WPI-AIMR Graduate Student scholarship

Effective October 1, 2007, Tohoku University created a new Research Institute, the Advanced Institute for Materials Research (AIMR), based on an initiative of the Japanese Department of Education (MEXT) for World Premier International Research Center Initiative (WPI) to bring together scientists involved in research on nano-science and technology.

In the 21st century, material science, broadly defined as the study of how complex/novel properties arise in matters/materials from the interactions of individual components, will becomes an essential and most important research topics
(HTTP://WWW.WPI-AIMR.TOHOKU.AC.JP).

TU WPI-AIMR is now looking for young motivated Ph.D. graduate student candidates in the fields of physical metallurgy, physics, chemistry, mechanical engineering and electronic / informational technology. All innovative M. S. students are welcome as active promoters of basic/applied sciences in these fields.

Applications are continuously screened throughout the year.
Please submit

1) **a curriculum vitae,**
2) **research proposal (<1,000 words),**
3) **2 letters of recommendation,**

by email to
yoshi@mail.tains.tohoku.ac.jp,
sakurai@imr.tohoku.ac.jp, and
wpi-office@bureau.tohoku.ac.jp.
All files must be submitted electronically in pdf or Word format.

WPI-AIMR

Workshop Guideline

Tohoku University's new Research Institute, the Advanced Institute for Materials Research (WPI-AIMR) solicits several applications per year for International Workshops in the field of "broadly defined Materials Science."

Guidelines:

1) Organizers

Qualified research staff of academic institutions and public or private research establishments can submit the application for an international workshop to be held at WPI-AIMR or its Satellite branches, jointly with the WPI-AIMR principal investigator(s) whose research interest overlaps with the scope of the workshop.

2) Financial support

Under normal circumstances, WPI-IMR supports up to 2/3 of the workshop budget, while the organizer is expected to cover the rest.

3) deadline

The application must be received at least four months in advance

to

yoshi@mail.tains.tohoku.ac.jp,
sakurai@imr.tohoku.ac.jp, and
wpi-office@bureau.tohoku.ac.jp.

All files must be submitted electronically in pdf or Word format.

Appendix

2009 WPI-AIMR Annual Workshop
March 1-6, 2009


of allotted time (40 mi
2. Manuscripts were due
3. Please submit your PF
Banquet
1. Please bring your ticke
2. Your spouse is welcon
ase see Prof. Tomi Ha
problem/concern.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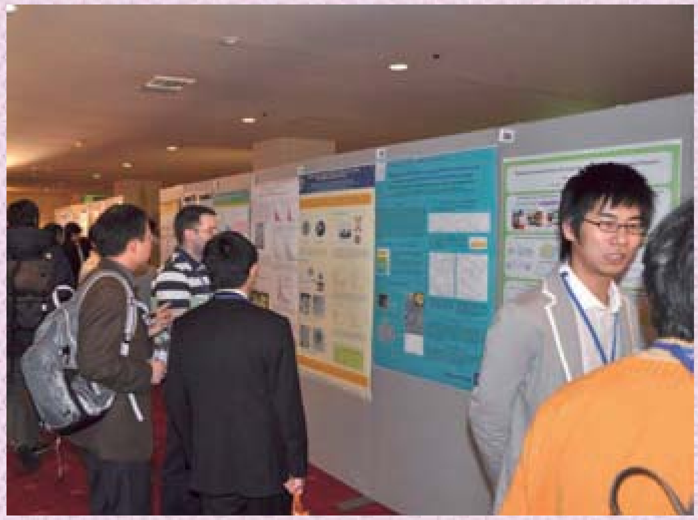

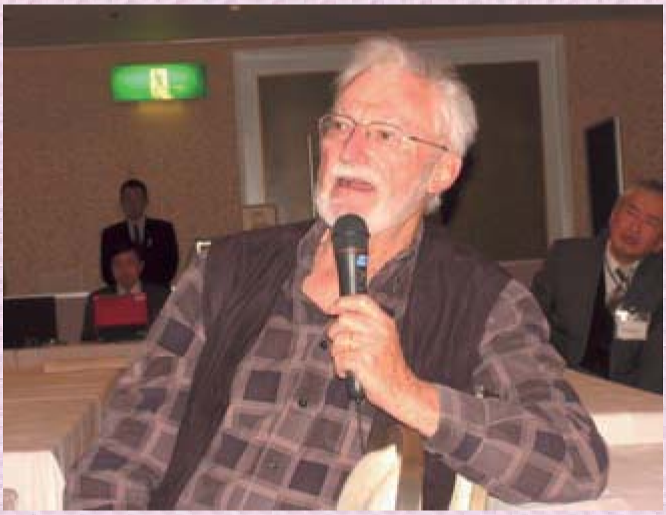


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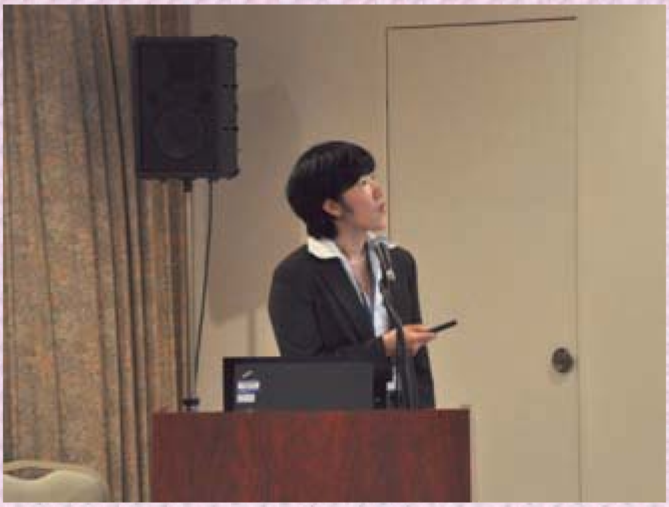


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wpi
March 5 (Thursday)
Session 9


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Summary
Spin-RAM is expected as universal memory in near future but material design and developments required to realize it.
It is of quite importance to optimize Ms, P, Ku, and α in designing materials. Natural and artificial ferrimagnets are one of candidates.
High-quality oxide barrier materials except for MgO is an open question although it is not included in this talk
Various knowledge and techniques are needed


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The Market TEL Participates in
CY2007 World Market
TEL's Mission
"Innovation"
"Growth"
"Environment"


Special session
Chair: Y. Kitamura
Thu- 7. 16:20 - 16:50
T. Higashi (Tokyo Electron Limited)
Thu- 8. 16:50 - 17:20


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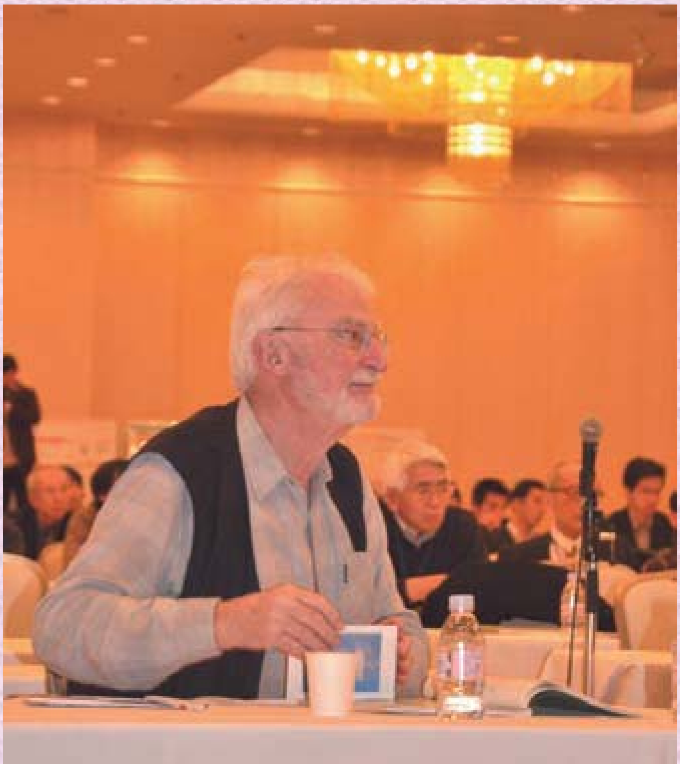
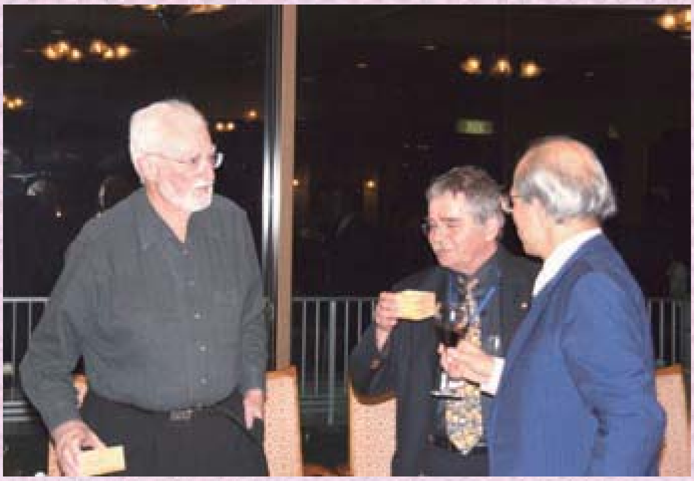



The 2009 WPI-AIMR Annual Workshop

Banquet











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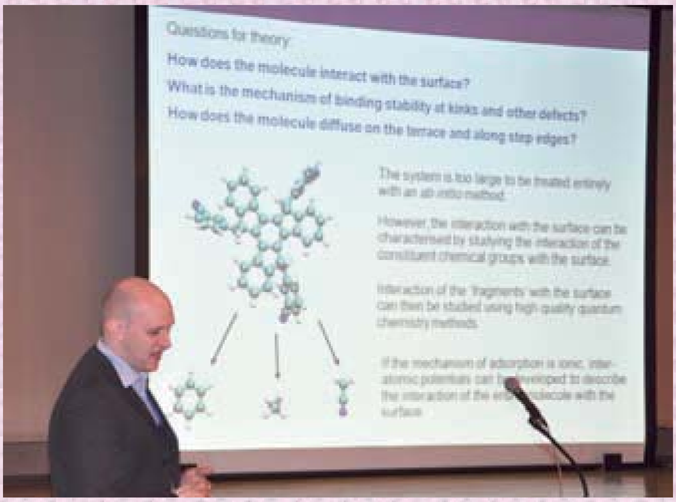
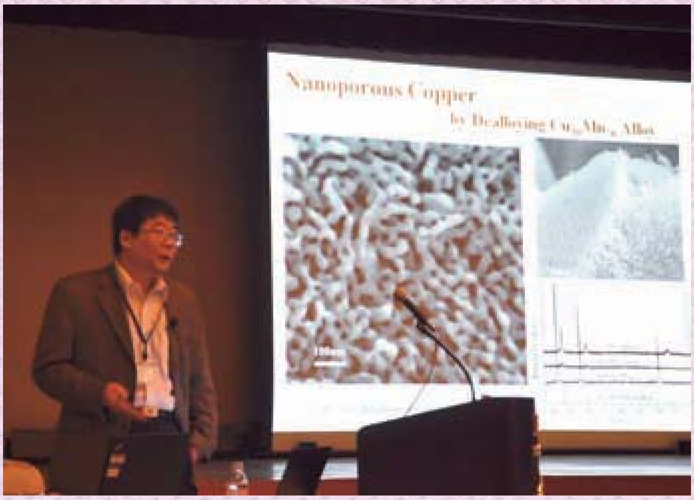
The 2009 WPI-AIMR Annual
March 1 to March 6, 2009


Questions for theory:
How does the molecule interact with the surface?
What is the mechanism of binding stability at kinks and other defects?
How does the molecule diffuse on the terrace and along step edges?
The system is too large to be treated entirely with an ab initio method.
However, the interaction with the surface can be characterised by studying the interaction of the constituent chemical groups with the surface.
Interaction of the 'fragments' with the surface can then be studied using high quality quantum chemistry methods.


Nanoporous Copper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Session 11
Chair: T. Hashizume
Fri- 4.
10:20 - 11:00
M. W. Chen (WPI-AIMR)


Acknowledgement for the support by
Professors Hitosugi and Nakajima
Hashimoto
Wagatsuma
Yoshida
Zeniya
Saito
Unoura
Sato


The 2009 WPI-AIMR Annual Workshop


The 2009 WPI-AIMR Annual Workshop

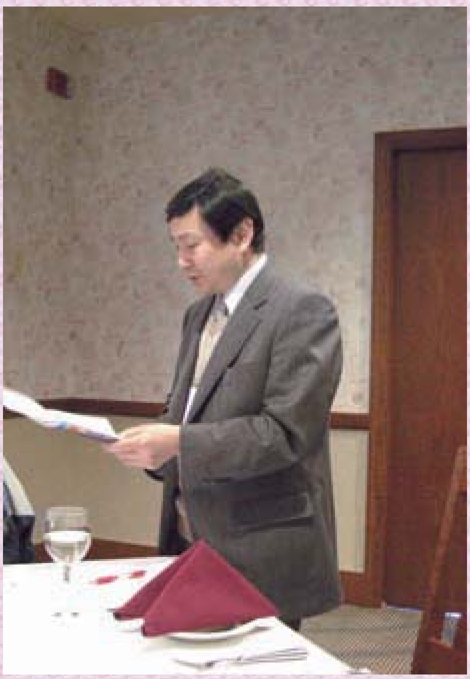
Snow Monster Excursion

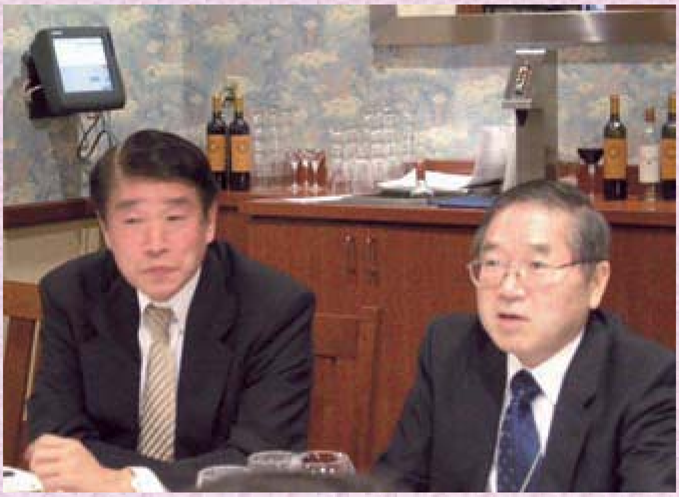
Swiss Embassy Reception
for Dr. Rohrer & Dr. Bednorz

2009 APS March Meeting

Award Party

for Prof. Inoue & Prof. Miyazaki









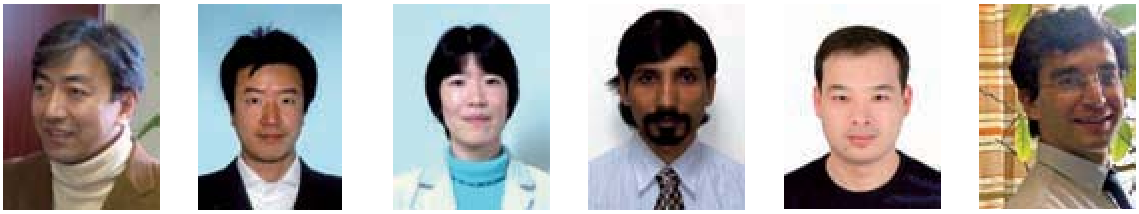

SKI Conference in Salt Lake City

March 20-22, 2009

Administration

A. Inoue | Y. Kitamura | T. Sakurai | Y. Yamamoto

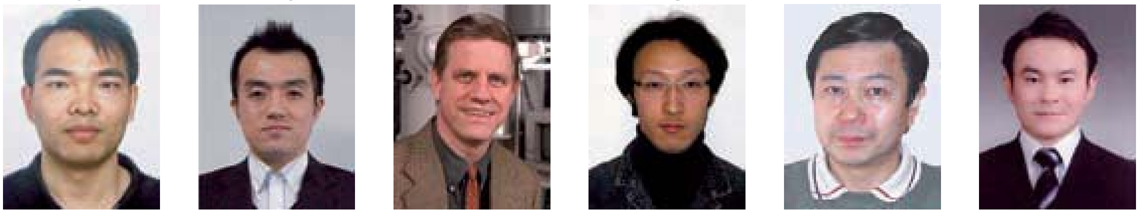
Research staff



T. Adschiri | K. Akagi | K. Akiyama | A. Al-Mahboob | Z. An | A. Chekhovskiy

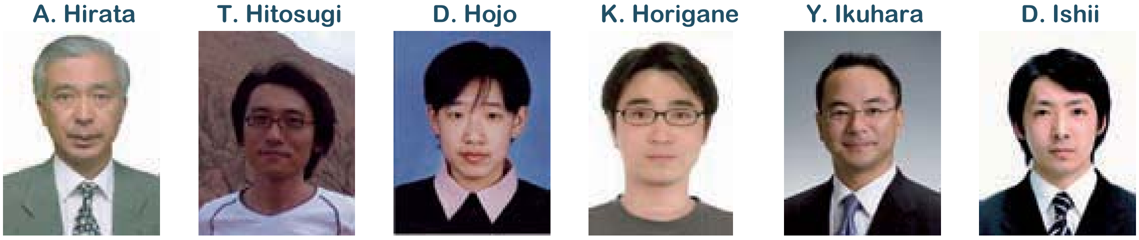
L. Chen | M. Chen | N. Chen | A. Chutia | D. Ehrentraut | M. Esashi

S. Fujinami | T. Fujita | N. Fukui | K. Georgarakis | T. Gessner | A. Greer

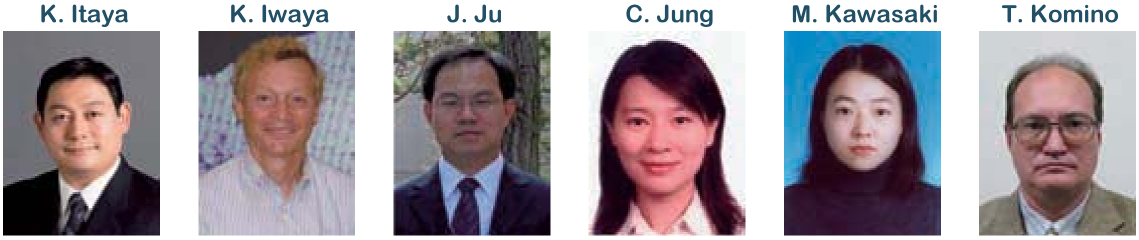


P. Guan | I. Hamada | K. Hemker | T. Higuchi | T. Hashizume | H. Hiraga

A. Hirata | T. Hitosugi | D. Hojo | K. Horigane | Y. Ikuhara | D. Ishii



K. Itaya | K. Iwaya | J. Ju | C. Jung | M. Kawasaki | T. Komino



A. Kuzume | M. Lagally | Z. Li | Y. Lin | H. Liu | D. Louzguine

S. Madge | T. Makino | A. Masago | K. McKenna | K. Minami | T. Miyazaki

S. Mizukami | M. Muroyama | K. Nakajima | K. Nakayama | M. Nakazawa | T. Nishi

R. Nouchi | T. Ohmi | T. Ohsawa | C. Qin | P. Richard | T. Russell

M. Saito | E. Sajitha | S. Sanchez | T. Sato | M. Shimomura | A. Shluger

S. Simdyankin | S. Soma | K. Sugawara | T. Takahashi | A. Takeuchi | H. Tamura

J. Tang | K. Tanigaki | M. Tokuyama | T. Trevethan | M. Tsukada | S. Tsukimoto

K. Ueno | L. J. Wan | D. Wang | Z. Wang | D. Watanabe | P. Weiss

R. Wen | F. Wu | L. Xu | Q. K. Xue | K. Yamada | M. Yamaguchi


Y. Yasui


A. Yavari


S. Yoshida


J. Yu


Q. Zhang


X. Zhang

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S. Unoura


Y. Wagatsuma


Y. Yanagawa


H. Yoshida


M. Yoshida


I. Zeniya

DIRECTORY

As of April 24, 2009

Name	Phone Number (+81-(0)22- _____)	E-mail Address
<Adschiri Group>		
1. ADSCHIRI, Tadafumi (PI, Prof.)	217-5629	ajiri@tagen.tohoku.ac.jp
2. HOJO, Daisuke (Assist. Prof.)	217-5631	dhojo@tagen.tohoku.ac.jp
3. MINAMI, Kimitaka (Assist. Prof.)	217-5631	nankim@tagen.tohoku.ac.jp
4. RANI, Varu (Research Assist.)	217-5630	chauhan@mail.tagen.tohoku.ac.jp
5. YOO, Jungwoo (Research Assist.)	217-5630	jw-yoo@mail.tagen.tohoku.ac.jp
<Chen Group>		
6. CHEN, Mingwei (PI, Prof.)	215-2143	mwchen@wpi-aimr.tohoku.ac.jp
7. FUJITA, Takeshi (Assist. Prof.)	215-2139	tfujita@wpi-aimr.tohoku.ac.jp
8. CHEN, Luyang (Research Assoc.)	215-2138	chenly@wpi-aimr.tohoku.ac.jp
9. GUAN, Pengfei (Research Assoc.)	215-2138	pf.guan@wpi-aimr.tohoku.ac.jp
10. ZHANG, Xuefeng (Research Assoc.)	215-2138	xfzhang@wpi-aimr.tohoku.ac.jp
<Esashi Group>		
11. ESASHI, Masayoshi (PI, Prof.)	795-6934	esashi@mems.mech.tohoku.ac.jp
12. AKIYAMA, Kotone (Assist. Prof.)	795-6258	akiyama@mems.mech.tohoku.ac.jp
13. MUROYAMA, Masanori (Assist. Prof.)	795-6256	muroyama@mems.mech.tohoku.ac.jp
14. YOSHIDA, Shinya (Assist. Prof.)	795-6468	s-yoshida@mems.mech.tohoku.ac.jp
<Gessner Group>		
15. GESSNER, Thomas (PI)	+49- 371-53124060	thomas.gessner@zfm.tu-chemnitz.de
16. LIN, Yu-Ching (Assist. Prof.)	795-6256	yclin@mems.mech.tohoku.ac.jp
17. CHEKHOVSKIY, Aleksandr (Research Assoc.)		
<Greer Group>		
18. GREER, Alan Lindsay (PI)	+44-1223-334308	alg13@cam.ac.uk
19. MADGE, Shantanu V. (Research Assoc.)	217-5956	shantanu.madge@wpi-aimr.tohoku.ac.jp

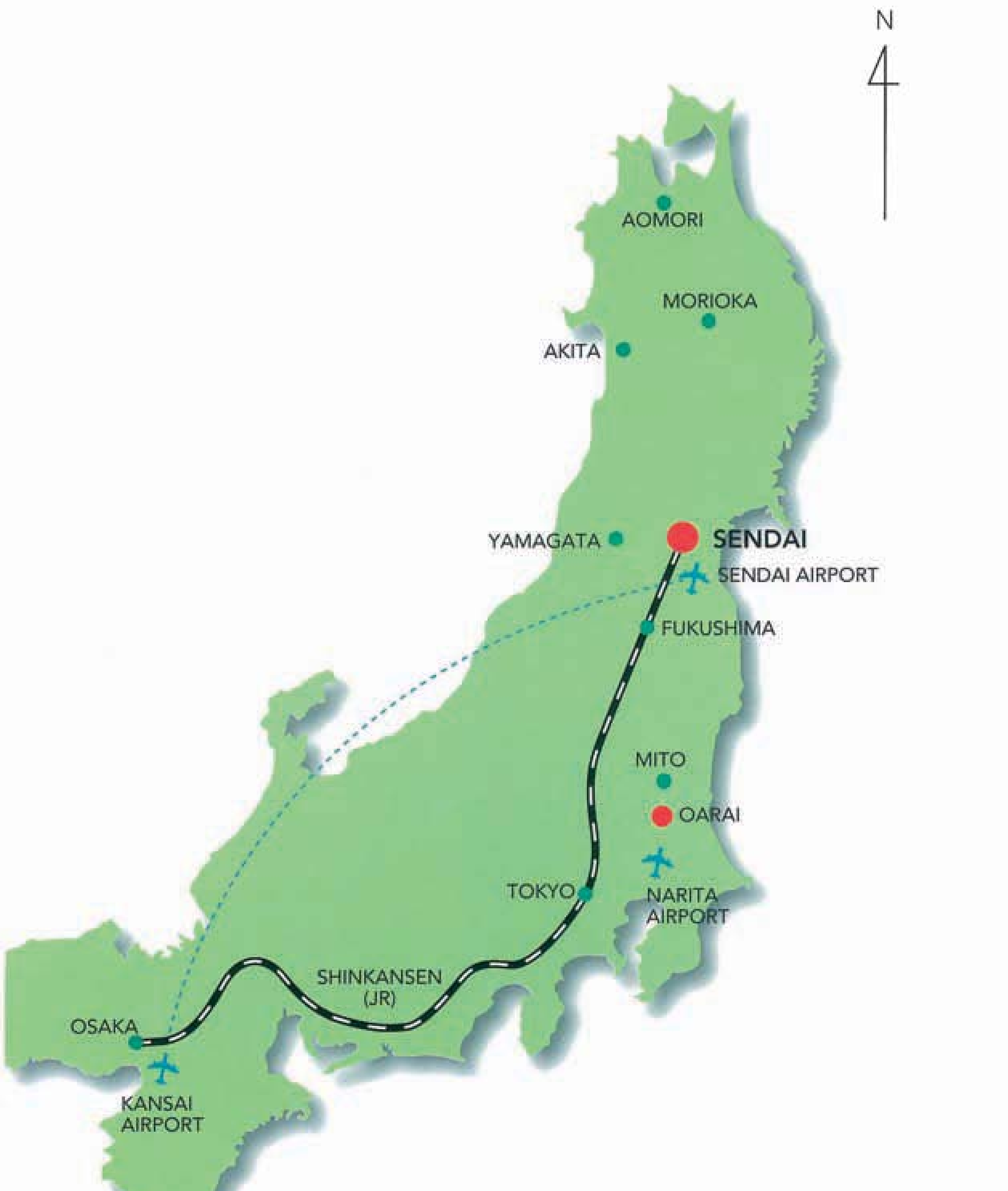
	Name	Phone Number (+81-(0)22- _____)	E-mail Address
<Hashizume Group>			
20.	HASHIZUME, Tomihiro (PI)	+81-49-2966111	tomihiro.hashizume.qb@hitachi.com
21.	HITOSUGI, Taro (Assoc. Prof.)	217-5944	hitosugi@wpi-aimr.tohoku.ac.jp
22.	AL-MAHBOOB, Abdullah (Assist. Prof.)	217-5948	aal-mahboob@wpi-aimr.tohoku.ac.jp
23.	IWAYA, Katsuya (Assist. Prof.)	217-5948	iwaya@wpi-aimr.tohoku.ac.jp
24.	OHSAWA, Takeo (Assist. Prof.)	217-5948	ohsawa@wpi-aimr.tohoku.ac.jp
25.	FUKUI, Nobuyuki (Research Assoc.)	217-5948	n-fukui@wpi-aimr.tohoku.ac.jp
26.	SHIMIZU, Ryota (Research Assist.)	217-5948	shimizu@chem.s.u-tokyo.ac.jp
< Hemker Group>			
27.	HEMKER, Kevin (PI)	+ 1- 410- 5164489	hemker@jhu.edu
<Ikuhara Group>			
28.	IKUHARA, Yuichi (PI)	+81-3-58417688	ikuhara@sigma.t.u-tokyo.ac.jp
29.	TSUKIMOTO, Susumu (Lecturer)	217-5934	tsukimoto@wpi-aimr.tohoku.ac.jp
30.	SAITO, Mitsuhiro (Assist. Prof.)	217-5933	saito@wpi-aimr.tohoku.ac.jp
31.	WANG, Zhongchang (Research Assoc.)	217-5933	zcwang@wpi-aimr.tohoku.ac.jp
<Inoue Group>			
32.	INOUE, Akihisa (PI, President of TU)	217-4800	ainoue@imr.tohoku.ac.jp
33.	LOUZGUINE, Dmitri V. (Prof.)	217-5957	dml@wpi-aimr.tohoku.ac.jp
34.	NAKAYAMA, Koji (Assoc. Prof.)	217-5956	kojisn@wpi-aimr.tohoku.ac.jp
35.	TAKEUCHI, Akira (Assoc. Prof.)	217-5956	takeuchi@wpi-aimr.tohoku.ac.jp
36.	YOKOYAMA, Yoshihiko [Assoc. Prof. (Concurrent)]	215-2199	yy@imr.tohoku.ac.jp
37.	QIN, Chunling (Assist. Prof.)	217-5956	clqin@wpi-aimr.tohoku.ac.jp
38.	ZHANG, Qingsheng (Assist. Prof.)	217-5956	qszhang@wpi-aimr.tohoku.ac.jp
39.	HIRATA, Akihiko (Assist. Prof.)	217-5959	hirata@wpi-aimr.tohoku.ac.jp
40.	CHEN, Na (Research Assoc.)	217-5956	Chenn@wpi-aimr.tohoku.ac.jp
41.	SANCHEZ, Sergio Gonzalez (Research Assoc.)	217-5956	sergiogs@wpi-aimr.tohoku.ac.jp
42.	YU, Jinshan (Research Assoc.)	217-5956	yujinshan@imr.tohoku.ac.jp
43.	LI, Song (Researcher)	217-5956	lisong@wpi-aimr.tohoku.ac.jp

Name	Phone Number (+81-(0)22- _____)	E-mail Address
<Itaya Group>		
44. ITAYA, Kingo (PI, Prof.)	795-5868	itaya@atom.che.tohoku.ac.jp
45. KUZUME, Akiyoshi (Assist. Prof.)	795-5869	kuzume@atom.che.tohoku.ac.jp
46. JUNG, Changhoon (Research Assoc.)	795-5869	cjung@atom.che.tohoku.ac.jp
47. KOMINO, Takeshi (Research Assoc.)	795-5869	komino@atom.che.tohoku.ac.jp
<Kawasaki Group>		
48. KAWASAKI, Masashi (PI, Prof.)	215-2085	kawasaki@imr.tohoku.ac.jp
49. MAKINO, Takayuki (Lecturer)	217-5600	tmakino@imr.tohoku.ac.jp
50. UENO, Kazunori (Assist. Prof.)	215-2088	uenok@imr.tohoku.ac.jp
51. HIRAGA, Hiroki (Research Assoc.)	217-5600	hiraga.hiroki@imr.tohoku.ac.jp
<Lagally Group>		
52. LAGALLY, Max G. (PI)	+1-608-2632078	lagally@engr.wisc.edu
<Miyazaki Group>		
53. MIYAZAKI, Terunobu (PI, Prof.)	217-6000	miyazaki@wpi-aimr.tohoku.ac.jp
54. MIZUKAMI, Shigemi (Assist. Prof.)	217-6003	mizukami@wpi-aimr.tohoku.ac.jp
55. SAJITHA, E. P. (Research Assoc.)	217-6003	sajitha@wpi-aimr.tohoku.ac.jp
56. WATANABE, Daisuke (Research Assoc.)	217-6003	watanabe@wpi-aimr.tohoku.ac.jp
57. WU, Feng (Research Assoc.)	217-6004	fengwu@wpi-aimr.tohoku.ac.jp
<Nakazawa Group>		
58. NAKAZAWA, Masataka (PI)	217-5522	nakazawa@riec.tohoku.ac.jp
<Nishi Group>		
59. NISHI, Toshio (PI, Prof.)	217-5926	nishi.toshio@wpi-aimr.tohoku.ac.jp
60. NAKAJIMA, Ken (Assoc. Prof.)	217-5927	knakaji@wpi-aimr.tohoku.ac.jp
61. FUJINAMI, So (Research Assoc.)	217-5928	fujinami@wpi-aimr.tohoku.ac.jp
62. WANG, Dong (Research Assoc.)	217-5928	wangdthu@wpi-aimr.tohoku.ac.jp
<Ohmi Group>		
63. OHMI, Tadahiro (PI)	795-3952	ohmi@fff.niche.tohoku.ac.jp

Name	Phone Number (+81-(0)22- _____)	E-mail Address
<Russell Group>		
64. RUSSELL, Thomas (PI)	+1-413-5452680	russell@mail.pse.umass.edu
<Shimomura Group>		
65. SHIMOMURA, Masatsugu (PI, Prof.)	217-5329	shimo@tagen.tohoku.ac.jp
66. ISHII, Daisuke (Assist. Prof.)	217-5824	dishii@tagen.tohoku.ac.jp
67. HIGUCHI, Takeshi (Research Assoc.)	217-5825	higuchi@mail.tagen.tohoku.ac.jp
<Shluger Group>		
68. SHLUGER, Alexander (PI)	217-5942 /+44-171-391-1312	a.shluger@ucl.ac.uk
69. TREVETHAN, Thomas (Assist. Prof.)	217-5942	tomt@wpi-aimr.tohoku.ac.jp
70. MCKENNA, Keith (Assist. Prof.)	217-5942	k.mckenna@wpi-aimr.tohoku.ac.jp
<Takahashi Group>		
71. TAKAHASHI, Takashi (PI, Prof.)	217-6417	t.takahashi@arpes.phys.tohoku.ac.jp
72. RICHARD, Pierre (Assist. Prof.)	795-6477	p.richard@arpes.phys.tohoku.ac.jp
73. SOUMA, Seigo (Assist. Prof.)	795-6477	s.souma@arpes.phys.tohoku.ac.jp
74. SUGAWARA, Katsuaki (Research Assoc.)	795-6477	k.sugawara@arpes.phys.tohoku.ac.jp
<Tanigaki Group>		
75. TANIGAKI, Katsumi (PI, Prof.)	795-6469	tanigaki@sspns.phys.tohoku.ac.jp
76. JU, Jing (Assist. Prof.)	795-6468	jujing@sspns.phys.tohoku.ac.jp
77. NOUCHI, Ryo (Assist. Prof.)	795-6468	nouchi@sspns.phys.tohoku.ac.jp
78. TANG, Jung (Assist. Prof.)	795-6468	tangjun@sspns.phys.tohoku.ac.jp
79. LI, Zhaofei (Research Assoc.)	795-6468	zfli@sspns.phys.tohoku.ac.jp
<Tokuyama Group>		
80. TOKUYAMA, Michio (PI, Prof.)	217-5953	tokuyama@wpi-aimr.tohoku.ac.jp
81. SIMDYANKIN, Sergey (Assist. Prof.)	217-5955	simdyankin@wpi-aimr.tohoku.ac.jp
82. XU, Limei (Assist. Prof.)	217-5955	limei.xu@wpi-aimr.tohoku.ac.jp
83. CHUTIA, Arunabhiram (Research Assoc.)	217-5954	arun@wpi-aimr.tohoku.ac.jp
84. NARUMI, Takayuki (Research Assist.)	217-5954	takayuki.narumi@wpi-aimr.tohoku.ac.jp

Name	Phone Number (+81-(0)22- _____)	E-mail Address
<Tsukada Group>		
85. TSUKADA, Masaru (PI, Prof.)	217-5937	tsukada@wpi-aimr.tohoku.ac.jp
86. AKAGI, Kazuto (Assoc. Prof.)	217-5940	akagi@wpi-aimr.tohoku.ac.jp
87. TAMURA, Hiroyuki (Assist. Prof.)	217-5938	hiroyuki@wpi-aimr.tohoku.ac.jp
88. HAMADA, Ikutaro (Assist. Prof.)	217-5938	hamada@cmp.sanken.osaka-u.ac.jp
89. MASAGO, Akira (Research Assoc.)	217-5939	masago@wpi-aimr.tohoku.ac.jp
90. ARAIDAI, Masaaki (Researcher)	217-5939	araidai@wpi-aimr.tohoku.ac.jp
<Wan Group>		
91. WAN, Li-Jun (PI)	+86-62558934	wanlijun@iccas.ac.cn
92. WEN, Rui (Research Assoc.)	217-5869	ruiwen@atom.che.tohoku.ac.jp
<Weiss Group>		
93. WEISS, Paul S. (PI)	+1-814-8653693	stm@psu.edu
<Xue Group>		
94. XUE, Qikun (PI)	+86-10-62795618	qkxue@mail.tsinghua.edu.cn
95. LIU, Hongwen (Assist. Prof.)	217-5948	liu@wpi-aimr.tohoku.ac.jp
<Yamada Group>		
96. YAMADA, Kazuyoshi (PI, Prof.)	215-2035	kyamada@imr.tohoku.ac.jp
97. SATO, Toyoto (Assist. Prof.)	215-2039	toyoto@imr.tohoku.ac.jp
98. HORIGANE, Kazumasa (Research Assoc.)	215-2038	khorigane@imr.tohoku.ac.jp
<Yamaguchi Group>		
99. YAMAGUCHI, Masahiko (PI, Prof.)	795-6812	yama@mail.pharm.tohoku.ac.jp
100. YASUI, Yoshizumi (Assist. Prof.)	795-6815	yasui@mail.pharm.tohoku.ac.jp
101. AN, Zengjian (Research Assoc.)	795-6815	zjan@mail.pharm.tohoku.ac.jp
<Yavari Group>		
102. YAVARI, Alain Reza (PI)	+ 33-(0) 4 76 82 6516	yavari@minatec.inpg.fr
103. GEORGARAKIS, Konstantinos (Research Assoc.)		georgara@minatec.inpg.fr

Name	Phone Number (+81-(0)22- _____)	E-mail Address
<Administrative Office>		
104. YAMAMOTO, Yoshinori (Institute Director)	217-5130	yoshi@mail.tains.tohoku.ac.jp
105. SAKURAI, Toshio (Director of Administrative Office)	217-5973	sakurai@imr.tohoku.ac.jp
106. HAGA, Hideko (Deputy Director of Administrative Office)	217-5980	haga-h@bureau.tohoku.ac.jp
General Affairs Section	217-5922 or 5971 FAX: 217-5129	wpi-office@bureau.tohoku.ac.jp
107. HASHIMOTO, Keiichi (Chief)		k-hashi@wpi-aimr.tohoku.ac.jp
108. SAITO, Asuka		asuka-s@bureau.tohoku.ac.jp
109. CHIGIRI, Atsuko		chigiri@wpi-aimr.tohoku.ac.jp
110. KONOHARA, Nao		konohara@bureau.tohoku.ac.jp
111. CHIBA, Yoko	217-5956	ykchiba@wpi-aimr.tohoku.ac.jp
112. UNOURA, Sayaka		unoura-0@wpi-aimr.tohoku.ac.jp
113. SATO, Kaori		sato-0@wpi-aimr.tohoku.ac.jp
Accounting Section	217-5923 / FAX: 217-5129	wpi-keiri@wpi-aimr.tohoku.ac.jp
114. WAGATSUMA, Yasushi (Chief)		wagatsu@bureau.tohoku.ac.jp
115. OKAZAKI, Tomohiro		tokazaki@bureau.tohoku.ac.jp
116. ZENIYA, Ippei		zeniya@bureau.tohoku.ac.jp
117. KOBAYASHI, Yuki		y-kobayashi@bureau.tohoku.ac.jp
Property Management Section	217-5924 / FAX: 217-5129	wpi-yodo@wpi-aimr.tohoku.ac.jp
118. YOSHIDA, Masahiro (Chief)		masa-y@bureau.tohoku.ac.jp
119. HIRAYAMA, Hirotaka		h-hiraya@bureau.tohoku.ac.jp
120. YOSHIDA, Hajime		hyoshi@bureau.tohoku.ac.jp
121. ONO, Toshio		toshi-ono@bureau.tohoku.ac.jp
122. DAIGAKU, Noriko		daigaku@bureau.tohoku.ac.jp
123. OHTOMO, Yumi		yumi-o@bureau.tohoku.ac.jp
Research Cooperation Section	217-5971 / FAX: 217-5129	wpi-kenkyo@wpi-aimr.tohoku.ac.jp
124. YANAGAWA, Yasukazu		yanagawa@bureau.tohoku.ac.jp
125. HAGITA, Etsuko	217-5978	hagita@bureau.tohoku.ac.jp
126. ONODERA, Mariko		m.onodera@wpi-aimr.tohoku.ac.jp
127. OKAMOTO, Ayumi	217-5978	a-okamoto@bureau.tohoku.ac.jp
128. SAITO, Ikuo		s-ikuo@bureau.tohoku.ac.jp
<Safety and Health Management Office>	217-5970	
129. KAMOSHIDA, Kazuyoshi (Manager)		kamoshida@bureau.tohoku.ac.jp
130. UMEZAWA, Akiko		akiko_umezawa@bureau.tohoku.ac.jp



World Premier International Research Center
Advanced Institute for Materials Research
Tohoku University

2-1-1 Katahira, Aoba-ku, Sendai 980-8577, Japan
Phone : +81-22-217-5922
FAX : +81-22-217-5129
E-mail : wpi-office@bureau.tohoku.ac.jp
URL : http : //www.wpi-aimr.tohoku.ac.jp

World Premier International Research Center
Advanced Institute for Materials Research
Tohoku University

●

2-1-1 Katahira, Aoba-ku, Sendai 980-8577
Japan
Phone: +81-22-217-5922
FAX: +81-22-217-5129