



Oral History

Pol Duwez — How It Was

Pol Duwez, professor of applied physics and materials science, emeritus, is one of the world's leading scientists in the field of metals and materials. He has conducted research leading to the development of a series of new alloys that are widely used in industry, and he pioneered in work that led to the discovery of metallic glasses. Most recently he has been involved in research on metals known for properties of superconductivity.

All this work has been recognized by a number of awards, including in 1981 the William Hume-Rothery Award from the Metallurgical Society of the American Institute of Mining, Metallurgical, and Petroleum Engineers, Inc.; and the Heyn Medal of the German Society for Metals Science. He is a member of numerous professional societies and was elected to the National Academy of Sciences in 1972. For many years he was a member of several boards and committees of government agencies concerned with national defense.

Duwez held an appointment at the Jet Propulsion Laboratory in the late 1940s and early 1950s, and he has been a full professor at the Institute since 1952. In 1979, Harriett Lyle interviewed him for the oral history project of the Caltech Archives. E&S presents here an excerpt from the transcript of those interviews.

Harriett Lyle: What are some of the first memories you have of Belgium and what it was like to be a child there?

Pol Duwez: The first thing that I cannot forget is when Belgium was invaded by the Germans in 1914; I was seven years old, and I remember the mounted German soldiers, called Uhlans de la Mort, with a skull as an insignia on their shakos. Before that, I remember that I went to the opera to see *Faust* in Brussels.

HL: Was your family interested in music?

PD: My father and mother appreciated music, but they never played any instrument. But my older brother became a professional cellist in an orchestra. He taught me when I was six, and at seven I had a quarter-size cello. I wish I could have kept it, but we lost everything when we left Belgium at the beginning of the Second World War. When I was twelve, I got my first prize in cello at the Mons Conservatory in my home city, playing the "Davidow Concerto."

HL: Do you remember any interest in science or exposure to it?

PD: When I was in high school, I was mostly interested in physics and mathematics. I don't know if I could have made up my mind between the two. Then came the question of continuing to university. At that time in Belgium we had four universities — Ghent, Liège, Brussels, and Louvain. But there was also a School of Mines and Metallurgy Engineering that was famous in Mons. Now the question of

going to the University of Brussels for science was a question of cost and distance. It was only 60 kilometers, but that meant time and money to go to Brussels and back home. So I decided I would stay in Mons and go to the School of Mines. Fortunately, the first two years there were just comparable to what you would have in science at the university. In the third year I started to study, in addition to the courses in Mons, what would be required for the science curriculum in physics and mathematics. It was possible to do this at that time. You could go to a central examining committee to be examined on what would be required to have a degree in physics in any university.

HL: I'd like to talk a little about how you as a child experienced the occupation of Belgium by the Germans.

PD: The main point, as far as I remember, was that we were lacking in food, but fortunately Herbert Hoover, who became president later, created the Commission for Relief in Belgium, and he got plenty of money from American charity. It was very well organized, and we started receiving food regularly from this country. In school, for example, we got a small piece of bread every morning to supplement what we did not have at home before leaving for school.

That particular commission had surplus money when the war ended, and that surplus became a foundation, the Commission for Relief in Belgium Educational Foundation, which later became the Belgian-American Educational Foundation



Pol Duwez with Theodore von Kármán in 1961. Chien, the dog, belonged to Duwez.

and which awarded the fellowship that I received in 1933 to come to Caltech.

HL: When you went to the School of Mines, were you interested in the mining profession?

PD: No. The school was both mining and metallurgy, and I was interested in metallurgy.

I graduated in 1932 as an engineer, and I left for the United States with a fellowship. I went to the University of Michigan as a graduate student because I wanted to work with Professor Timoshenko, whose field of interest was mechanical engineering, the strength of materials, and so on.

As in any American university, you had to pass a physical examination, which was much more detailed than it was in Belgium, where I had passed the physical examination with the American foundation which was sending me over here. In Michigan it was a mass production, thousands of students going through the line for X rays. And the results of my chest X rays came out badly, and they claimed I had tuberculosis. That created a big stir in Belgium, because the foundation had sent me after a thorough physical examination. The foundation sent me back to Belgium because I could not continue at Michigan. After several months, they decided it was a mistake. They had assumed that the spots on my lung, which were scars from pleurisy that I had had

earlier, were active tuberculosis.

In retrospect, it was a very happy circumstance that they sent me back to Belgium, because in one year I got my doctor's degree in physics at Brussels. When I came back in 1933, I came to Caltech because of Professor von Kármán, who was in aeronautics and very much interested in solid mechanics.

Since I had enough financial support, I had no problem being accepted at Caltech as a research fellow, because they didn't have to pay me. They opened the door, and when I went to see von Kármán, we discussed problems and I started to work immediately. Well, it was a combination of von Kármán and Fritz Zwicky, who was at that time interested in solid state. Later on he became an astrophysicist. But he was interested in the plasticity of metallic crystals, which was a big subject at that time. The field was in a transition between empiricism and real understanding of what was happening. Zwicky and von Kármán were a very good combination for me. I learned quite a bit from my collaboration working under these two excellent men.

HL: When you came here, it was in the middle of the depression in this country.

PD: I did not feel the depression, the reason being that my Belgium fellowship was very generous in giving me \$130 per month. With a roommate, I had an apartment on South Hudson Avenue with a living room, kitchen, and two bedrooms for \$15 each. Eating out twice a day, including a steak dinner, amounted to about \$1.60 a day. The rest — laundry and so on — was minimum. At the end of that first year at Caltech, I managed to save enough money to take a trip around the world. Transportation, by ship obviously, was arranged by the Japanese N.Y.K. Line for a total of \$550. I spent ten days in Japan — then went to Shanghai, Hong Kong, Singapore, Penang, Ceylon, and back to Belgium to see my family. Then I came back to the United States. For all that I spent less than \$650.

So I did not feel the depression, but some people were really in bad shape. I remember on the corner of California Boulevard and Lake Avenue there was a filling station, and the man who used to serve gas on my old Model-T that I bought for \$10, by the way, had a master's degree in geology. He could not find a job in any field of geology.

HL: When you got back to Belgium in 1933, did you know then that the Ger-

mans were preparing again for another war?

PD: In 1933 — yes. Between 1933 and when my wife and I finally left in 1940, it was obvious that something was going on, that sooner or later we would be involved, in spite of the fact that people just could not believe that there would be another German occupation. Well, it did happen. But it was really obvious only a few months before the invasion started. In fact, we were caught there after the Germans were already halfway through Belgium. My daughter, Nadine, was about three years old. We put everything in the Ford we had and took the highway to southern France where my wife had family. We were trying to get as far away as possible from the invading German army, but already they were bombing everything and machine-gunning people on the road.

My wife was an American citizen, although born in France, but naturalized. My little daughter was considered an American citizen because she was registered at the American embassy in Brussels, where she was born. So they could immediately get their sailing pass and be admitted to the United States. But I was not an American citizen, so I had to wait while my family came here to Pasadena.

HL: Had you met your wife in Pasadena?

PD: Yes. Through the Alliance Française. We were invited to see a French movie in a small theater here in Pasadena by a friend who was a professor of geology here at Caltech. My wife had taught in the Los Angeles high school system before, so some of her friends helped her and she got a job with that system in 1940.

I had some difficult times in southern France. I was not supposed to stay there because the Germans had an agreement with the Petain government that all the Belgian people, British, and so on would go back to their own countries. I was with my wife's parents in a small village in southern France, and I knew the *gendarmes* so I could stay there and nobody said anything. The only trouble came when I was taking a train to go somewhere — like to Marseilles to the American consulate — coming out of the station there was a control established by the French, but under German pressure, to find out if some of the people like me didn't have the right papers. I always managed, but I had to wait six months for an American visa, which my wife

arranged in Washington with the help of the Hoover Foundation. By the time the visa came, I had no money left, but I received a mysterious check from Lisbon from a man who had been the prime minister of Belgium. I understood later that he got a request from the American foundation to send me money to go by train from Nîmes to Lisbon. The money enabled me to buy a ticket as far as Madrid, but by then I was short of money again.

You know, the best way to find out about something that is underground is to go to a cafe and ask the waiter if he can help. So I asked a waiter where I could sell gold. He told me, "Go to a building on such and such a street; go to the third floor; knock on the first door to the left, and if somebody answers, tell them what you want to do." So I went there, and there was a very mysterious-looking man with a balance scale. I had a few small things in gold — a watch, a chain, the souvenirs I received for my first prize in cello, for example. He put all of them on the balance and was telling me they were worth so much in Portuguese money. When he reached the number that I had to pay for the ticket, I said, "That's enough." But he had taken practically everything I had. It was nine o'clock in the morning, and the train was leaving at seven or eight at night. I didn't have any money to buy food, so I waited in a public park with my cello and two suitcases. At Lisbon, nobody was waiting for me, but I had word that I could pick up money at such an address. The Foundation, again, had arranged to give me money. So I got a big meal in Lisbon.

HL: I would like to discuss the direction you were going in science — at Caltech and back in Belgium in the period just before the war.

PD: The first year working with von Kármán and Zwicky, I was more or less continuing what I had done in my thesis in Belgium on the plasticity of metallic crystals. It was just the beginning of studying the deformation in metals based on single crystals instead of polycrystalline materials like all metals are. So the information was getting more fundamental. The incentive behind it at that time was that the theoretical physicist can compute the strength of a pure metal based on the atomic bond and some approximations. But when you measure the strength of the pure metal, it is less than one-thousandth of what it should be. So there is a large

discrepancy between what is calculated on the theoretical basis and the actual observation that the material starts deforming much sooner at much lower stress. The approach was that we should check that situation on a single crystal, not a polycrystalline one, because that makes the problem closer to the theoretical value. Well, it was even worse. What was the reason?

The first papers that gave an explanation were published when I was here by Taylor in England. It was shown that the theory assumes a perfect crystal, a perfect arrangement of the atoms into a periodic field. And that is not the case. The real crystal is far from perfect, and — due to the theory of Taylor — it will be very much weaker. But Zwicky was fighting for his idea that the most stable form of the crystals is the imperfect one. It was found out later on that he was not right, but he defended it to the end.

I went back to Belgium in 1935 and was with the Center for Scientific Research there, which is equivalent to the National Science Foundation. Then in 1937 I became director of the National Laboratory for Silicates in the School of Mines in Mons and stayed until 1940. I was put in charge of a new lab working in the field of ceramics and had a chance to start from zero, buy all the equipment. One of the first small pieces of equipment I bought was a Beckman pH meter.

HL: In 1940 did you come directly back to Caltech from Lisbon?

PD: I came back to Caltech and, of course, the first thing I had to do was to go and see Dr. Millikan. He was a remarkable man, familiar with everybody on the staff at Caltech. So he recognized me immediately. He said, "You are very welcome back to Caltech, but I'm sorry we don't have any money to pay you." I said I was not asking for money, and all I wanted was a place to work. He said, "We'll find you a desk somewhere and a room." So I went to see Professor von Kármán and Professor Donald Clark. Von Kármán gave me a problem to work on, and I could use the equipment that Clark had in the basement of Throop Hall. The problem had something to do directly with defense. It was obvious that the United States would be involved in the war sooner or later.

The problem had to do with the resistance of a building or a ship to an exploding charge. So it comes back to the resistance of solids to a rapidly applied load in

the milliseconds range. It was known that under these conditions an elastic wave propagates through the metal, but beyond a certain load the material must deform plastically. I suggested I would perform some experiments using Dr. Clark's equipment to find out what happens exactly.

That was in June. By October or November, I got my preliminary results, and they were already interesting. I wanted to communicate that to von Kármán as soon as I could. I think it was a Friday when I went to the Guggenheim building to show him the results, and I told him what I thought it could mean. He looked at it very hurriedly because he had to leave immediately for New York. I gave him all my rough notes, just a few graphs, and some scribbling of calculations. He took everything. Exactly three days later, he sent me a short letter, pencil-written on New York Central Railway stationery. This letter contained the entire theory of plastic wave propagation. At the end, he says, "I hope this will fit exactly with your experimental finding." In fact, it did check very, very closely. He had the theory written in no time, in very simple mathematics, as usual for von Kármán.

HL: So you did an experiment, and then on the basis of that experiment he pulled the theory together?

PD: I think he had the idea of waves — or propagation of the deformation. But when he saw the shape of the curves, of the results, he immediately saw where they came from, theoretically. That is how his mind worked.

I made more experiments, and the paper was written, the theory by von Kármán and the experiments by myself. A paper was sent for publication to the *Proceedings of the National Academy of Sciences*. Not more than a few days later, he received a letter from Vannevar Bush, director of the NDRC [National Defense Research Council] saying he would request that the paper should be withheld because of its importance to defense. Well, von Kármán told Bush, "That's all right, but I have here a man who has worked six months on this program, did all the research, and has not received any salary. NDRC should support this research with a contract." Bush agreed, but I was not a citizen, so my report was stamped confidential, and I could not receive a copy because I was not cleared. But from there on, I received a salary, and Caltech received a contract. I think the

salary was \$120 a month, which was all right at that time.

HL: Did you continue this kind of work for the rest of the war?

PD: No. I would not say the whole problem was solved, but it reached a point where we could not do much more. What made me change my field again was the information received by the Guggenheim Laboratory people about the German missile — the V-2. That information came here to von Kármán, and the Guggenheim Aeronautical Laboratory (known as GALCIT) started an important project under the sponsorship of the Army Ordnance Department. That was the beginning of the GALCIT rocket research project which later became JPL.

Anyway, von Kármán called me at home one night, and he said, "Would you like to be in charge of a project concerning materials for rockets? We have a big problem, and we have to do something fast. We will probably build a lab to help you work on materials for jet-propelled rockets. We have a meeting tomorrow." I said, "Yes, I will do what I can." So that was the start of my involvement with JPL.

HL: You were going to work on the materials for the rocket itself, not the propellant?

PD: No, not the propellant. That was a chemical problem. He had people for that. But the materials for the combustion chamber must stand very high temperature. You cannot find any materials to stand it, but you can cool the chamber, and that's how it's done.

HL: Did you know how they did it with the V-2s?

PD: We knew they were cooling it. The combustion chamber was essentially ordinary steel, but it was very cleverly cooled. They were injecting some of the propellant to build up a liquid layer between the hot gas and the steel.

Eventually, we did some work at JPL developing alloys for high-temperature, long-time gas turbine blades. Also I originated research on cooling of gas turbine blades, because I was rather pessimistic about increasing the temperature above a certain level. After all, all metals have a melting point. So above a certain temperature, you cannot use a metal, and we worked on ceramics for the blades.

HL: Let's focus a little more on what you did as you began doing most of your research on the campus in the 1950s.

PD: The number of my students in-

creased — slowly, but I got a chance to do some research leading to PhDs for some of them. The subjects became mostly concerned with the structure of new alloys, new metals. Titanium became a research subject, and we worked on the structure of alloys of titanium with other metals. Titanium and aluminum and titanium and vanadium became commercial alloys later on.

Molybdenum came along for use at high temperature. We worked on the so-called sigma phase, which was a very troublesome phase that causes brittleness of some high-temperature turbine alloys. We determined the structure, which turned out to be wrong, but we had prepared very good specimens. Gunnar Bergman [PhD '51], who was a student of Linus Pauling's in chemistry, got our specimens and with enough patience and hard work determined the right structure; and that was his thesis in chemistry.

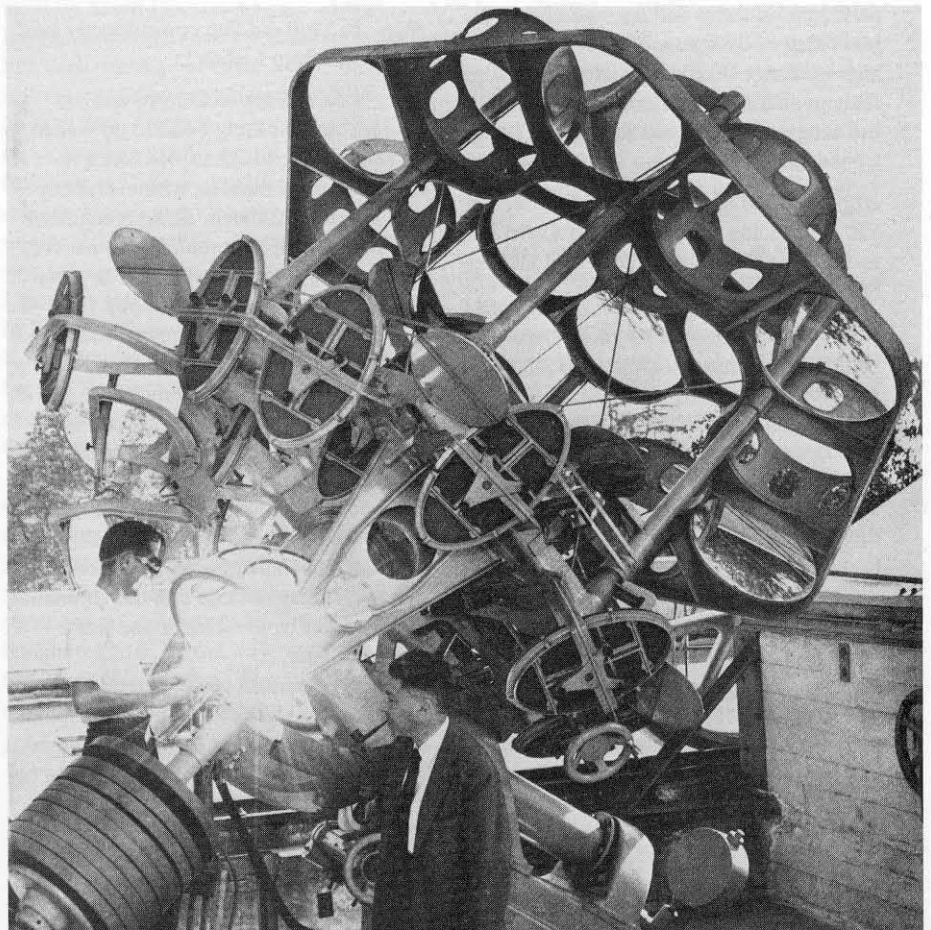
In the meantime, I went back to ceramics, which was my old subject in Belgium. We looked at ceramics made of the oxides of the so-called rare earths in the periodic table — gadolinium, neody-

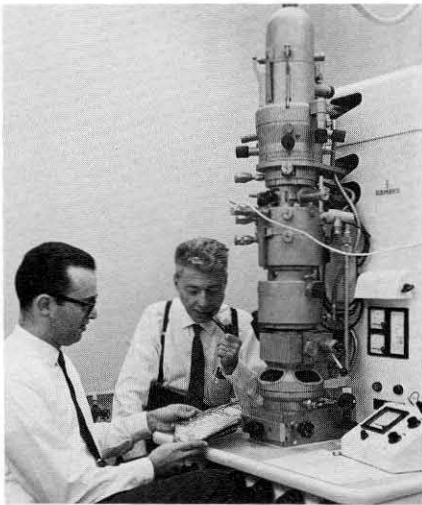
mium, lanthanum, and so on. I always thought that these oxides with high-temperature melting points would eventually be useful. We built special furnaces going to 2200° centigrade and higher. The graphite furnace could go to 2500° C.

Now I don't know how many years later the question came back of studying these same materials that we made at that time. Why? Because they have a high electrical conductivity at high temperatures. If magnetohydrodynamics is developed practically, they will be used as electrodes. This is a typical case of pure research leading to something without knowing it — and also waiting a number of years before it becomes a practical material.

My studies of high-temperature oxides continued for several years after I left JPL, because I had access to the solar furnace located on top of Robinson Astrophysics Laboratory. The solar furnace was built in 1930 by George Ellery Hale, the astronomer, and designed by Russell Porter, who designed most of the Caltech telescopes. In 1932 that solar furnace was in operation. It consisted of 32 lenses, two

The brilliant flash in this 1956 photo of the solar furnace once located on the roof of Robinson Laboratory is the concentration of sun rays on a piece of refractory oxide in the focal area. With Duwez is Eugene Loh (MS '53, PhD '54), at that time a metallurgist with the Stanford Research Institute and now with Hughes Aircraft.





In 1965 Duwez and Ronald Willens (BS '53, MS '54, PhD '61), assistant professor of materials science, were using an electron microscope to check the structure of alloys made with the rapid cooling technique.

feet in diameter, focusing all the sun's rays in a single circular area about one centimeter in diameter. The total power concentrated in that circle was three and a half kilowatts of radiant energy. We could actually melt thorium oxide, so the temperature was about 3500° C. That was not every day, because this required a very clear sky.

HL: I'd like to discuss the evolution of your work — your ideas about science, how your work evolved and what caused those changes.

PD: The main reorientation of my research program came in 1959, when I started a new technique of quenching from the liquid state. By quenching, I mean cooling at extreme rates from the liquid state to bypass what happens during solidification or at least to modify the solidification mechanism. I had that idea as far back as 1956, trying to obtain new structures by passing rapidly through the liquid-solid transition. It took me several years to have a good idea on how to do it, because the classical methods would not apply. This idea became a very simple one. It was to spread the liquid metal as fast as possible — into a layer as thin as possible — on a very good heat-conducting surface, which of course was copper. I never thought it would be possible to completely suppress the crystallization at that time. I just wanted to modify the system, which was copper-silver, and the results were positive. We got a solid solution in the copper-silver system.

We continued the program with other

systems analogous to copper-silver, but in which it was very improbable to find a solid solution. Then something else happened — a new crystalline phase that was not in equilibrium. Then, by analogy again, the next system we took was a gold-silicon alloy, very similar to the previous one. But instead of a crystalline phase came an amorphous phase. That was the first liquid-quench amorphous alloy — the beginning of the still-expanding field of metallic glasses.

HL: Why are they called metallic glasses?

PD: This is a very logical name. The definition of a glass is a solid obtained from a liquid which, when cooled, does not crystallize. This is exactly what we obtained. Now there are international meetings on metallic glasses and books on metallic glasses.

We continued to look for new alloys, the first one being a magnetic alloy. Can we get ferromagnetic behavior in a solid which is not crystalline? The first thing that comes to anybody's mind if you want something ferromagnetic is to use iron, nickel, or cobalt — the three most important magnetic atoms. Iron was the first choice. So we tried to find something that, mixed with iron, would quench into the amorphous state; and, having an empirical approach as to what to try, we chose phosphorus. A student by the name of Lin was doing the experiment, and he made a mistake. He forgot that it should not be melted in graphite. He had a graphite insert in the high-speed gun, shooting a globule of liquid iron-phosphorus alloy. The iron reacted with graphite, and it formed an alloy which contained also carbon. And this third alloy, iron-phosphorus-carbon, was amorphous and turned out to be one of the relatively strong amorphous ferromagnetic alloys. Iron-phosphorus alone was not quite amorphous. It turned out to be a very lucky mistake.

We continued to be interested in magnetism, of course, but there was something else to look for that could exist also in the amorphous state of metallic glasses. That was superconductivity, which is zero electrical resistance at low temperature, discovered by Kamerlingh Onnes in 1907. It was never considered a promising field in technology because it requires liquid helium, and people thought that it could never be applied on a large scale. Our question was to find alloys to quench. First, we must have a supercon-

ducting metal. Second, we must add a metalloid which will make it possible to be amorphous in the ratio of 75/25 or 80/20 (an empirical rule but a good one to follow). So we looked at the superconducting metals such as niobium and, finally, lanthanum, a good candidate for other reasons. Lanthanum-gold was chosen, tested, and turned out to be a superconductor below about 3.5 Kelvin. In looking for promising alloys for practical application, you want the highest possible transition temperature. The maximum reach now is about 18 to 20 K for some alloys — but they are crystalline alloys. For amorphous alloys, the first one we discovered was only 3 or 3.5; but we are up to about 10 K already.

Assuming that we cannot get anything better because it essentially cannot be done with a glassy structure, why do we still carry on research on these alloys? Well, being an educational institution, maybe we should not worry about answering the question from a practical standpoint. It's just science, and we could go on without justifying it except to say that there are new things to explain and many new things to introduce in the theory. But it's a little better if you can find a practical reason, and there is one that I think is very important. The crystalline materials are progressively damaged by high radiation fields; and superconductivity, when the alloy is exposed to high radiation doses (a transition temperature of, say, 18 K for some alloys), goes down to 3 or 4 K. So these amorphous alloys would be useful if the amorphous structure is insensitive to radiation. This has been confirmed recently by experiments made by William Johnson [associate professor of materials science at Caltech] on amorphous superconducting alloys subjected to high doses of radiation in the reactor at Livermore. Currently, there is also quite a bit of excitement on the chemical corrosion resistance of some of these alloys. Some of them are excellent, and they should also be cheaper than ordinary stainless steel.

I must say that what are still behind in metallic glasses are methods of fabrication. In this lab we always were satisfied with very small specimens, maximum one inch in diameter. That's enough for fundamental studies. But if these are going to be applied, we must have sheets of materials that are large enough for transformers or for magnets. And we have some hope that even massive metallic glasses will be obtained in the future. □