Memoires of Victor Glushkov

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DataArt

The following copy is a transcription of the voice records made in a hospital in early 1982, not long before Victor Glushkov passed away. We express our gratitude to his daughter Vera and granddaughter Victoria, who allow us to translate and publish these materials.

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In 1955, I was granted a year-long sabbatical from the Forest Engineering Institute in Sverdlovsk to prepare and defend my thesis for a doctorate at Moscow University. I spent nine months writing my paper and successfully defended it on December 15, 1955 (since I had vacation starting from January 1). During my stay at Moscow University, I lived with doctoral students from Ukraine who introduced me to Academician Boris V. Gnedenko, the former director of the Institute of Mathematics and the Academic Secretary of the Department of Mathematics and Mechanics of the Ukrainian Academy of Sciences.

In 1955, there was a decision to establish prestigious academic computing centers in primary academies of sciences within the Soviet republics, including Ukraine. This task was entrusted to the Institute of Mathematics, and to Boris Gnedenko personally. To accomplish this, the remaining equipment from the former Lebedev Laboratory, which produced MESM (Small Electronic Calculating Machine), was transferred from the Institute of Electronics to the Institute of Mathematics. At that time, the laboratory consisted of at most 40 people, including four Candidates of Sciences, no Doctors, and hardly any young specialists.

In March 1956, I went to Kyiv for the first time. I got acquainted with the applicants (mainly through their personal files since I only came briefly) selected by Gnedenko and other staff members, including those from the computing laboratory, at the University and Polytechnic Institute. During my second visit, this matter was resolved. In most cases, the candidates selected by the Institute of Mathematics suited me. There were a few exceptions, but I will only mention one. Gnedenko did not want to accept Letichevsky, and I insisted that Letichevsky be enrolled.

Most of the members of this new team later became leading employees of the institute.

In the laboratory, we worked on only four topics. One topic, initiated yet by Sergey A. Lebedev himself and carried on by Rabinovich, involved creating a specialized machine for solving systems of linear equations using the Gauss-Seidel method. The second topic was making the “Kyiv” universal machine, led by Dashevsky (a former Lebedev employee), with scientific supervision from Gnedenko. When I arrived, I was involved in scientific supervision as well. Two other topics had secrecy levels.

Since I tended to industrialize production, alongside working on the prototype for “Kyiv” for the Computing Center, I immediately decided to find a wealthy customer who would finance and supply us. Dubna, the Joint Institute for Nuclear Research, became just such a customer. The institute’s administrative director visited us, and we signed a contract and built the second machine for Dubna. This way, we immediately resolved the issues of financing, material, and technical supply, which would have been impossible to solve within the framework of the Academy of Sciences.

I must give credit to Gnedenko; he organized my work very well. He prohibited me from being present at the institute, that is, the laboratory, three days a week, and allowed me to be there only three days (at that time, we still worked on Saturdays, I believe). Three of my working days were given for studying the subject and onboarding to work. During these three days, a temporary acting supervisor, a candidate of science, was assigned to replace me on a daily rotation.

Gnedenko also added V. S. Koryluk and E. L. Yushchenko to the laboratory, so we ended up with six candidates (although Koryluk later did not continue). Unfortunately, none of the technicians were the best employees of Lebedev's team; the top employees were already working on BESM (Big Electronic Computing Machine) in Moscow.

Simultaneously, we began constructing the Computing Center on Bolshaya Kitayevskaya Street, at the corner of Lysogorskaya Street. Initially, we planned to equip this computing center with three machines: Ural-1, which had just gone into production, the “Kyiv’’ machine, and the MESM (Small Electronic Calculating Machine). However, three large halls were built, indicating that we were preparing for a much greater capacity. However, the number of workstations was significantly underestimated, totaling only 400 (based on the assumption that it would be just a computing center).

I should note that at that time, computer designs were based on engineering intuition. Therefore, even highly capable individuals from the radio engineering department, such as Stanislav Zabara (now the director of the Research Institute of Peripheral Equipment at the VUM plant, or rather, the “Electron” association), could not understand how the machine worked when they studied the book by Lebedev, Dashevsky, and Shkabara on the MESM.

Because [in the book] everything is stated as follows: here are the operating cycles of the central control; here are the clock cycles of the local control. There are no explanations for the difference between these local and central controls. This is related to the working style of that time: they got some American materials, cracked and messed them up, and then started making things according to the template.

Ultimately, I figured it out myself and started developing my understanding of how the machine works. Since then, the theory of computing machines has become one of my specialties. I decided to turn machine design from an art into a science. Naturally, the Americans did the same, but we got those materials much later, although the digest on automata studies was available in 1956.

I started working on it and organized a seminar. The first article in this American digest, already translated into Russian, was Turing's article (about the automaton approach), and it was unnecessarily complicated. So one of my first works, if we don't count top secret ones, was finding a much more elegant algebraic, simple, and logically clear concept of the Kleene automaton. I obtained all of Kleene's results (as well as other results). And most importantly, unlike Kleene's results, my theoretical developments were aimed at real-life machine design problems. Because we were analyzing how a machine is made at the seminar (we even finished designing "Kyiv" there), and at the same time, I was working on the theory. That was great because I could see what works and what doesn't.

Furthermore, I was leading a large team for the first time (previously, I was chairing a department in the Urals), so I needed to develop certain organizational principles. I designed these principles, followed them consistently ever since, and they always led to success. I haven't explicitly written about this anywhere yet, although it is kind of an organizational science.

I articulated the following principles for myself.

The unity of theory and practice. This principle is not new, but it is usually understood only in one direction, i.e., people who talk about the unity of theory and practice, in practice, refract this unity in order to make their ideas practically applicable. So that's it. And I clarified it by saying that science, especially young science, should not build theories that do not have practical applications, and added a new provision:

one should not start practical work, no matter how important it may seem, if it is not preceded by theoretical comprehension. Here's what it means: it may turn out that you should do something else instead of the project you have planned, but this work can be more versatile and later cover five hundred applications instead of one.

From the very beginning of work in the laboratory, it became clear that there were a lot of customers ready to purchase modeling of, for example, various kinds of discrete systems. Machines began to penetrate management, especially in special (defense-related) areas, and modeling was required there. We were literally inundated with all kinds of draft resolutions from high authorities that we should model this, model that, etc.

Later, after the formation of the Computing Center, when T. P. Marjanovich's department was established, or rather, at first, the laboratory at my department, he was assigned to do it. I gave him eight topics, i.e., eight orders and customer cards. And he had six people. He came to me and said, “How am I going to fulfill this?” I told him not to do the first, or the third, or the eighth. I told him to make SLENG instead (it was named SLENG later) — a universal language for modeling discrete systems. I gathered all the customers, did some educational work with them, and they agreed that this was exactly what they needed, and in fact, they were responsible for the wrong explanation of their requirements in the customer cards. This is how we achieved extensive coverage of our fundamental research with applications.

The following principle is the unity of long-range and short-range goals. It is close to the first principle, but still, it approaches the issue from another angle, from the perspective of the time needed to perform the work.

It reads as follows. One should not undertake any small individual works, even if they have many practical applications (i.e., they satisfy the first principle) if one cannot see the continuation of these works in the future.

The other side of the principle is this. Long-term research should only be undertaken if it can be broken down into stages, with each step promising a self-sufficient scientific and practical significance.

Based on this, we, for example, outlined a program for the intellectualization of machines, so that work on increasing intelligence in a programmatic way would also lead to an increase in intelligence... That's how the MIR-1 and MIR-2 machines were born, and now we have a machine for automating proofs in prospect.

The same thing can be done with robots. You can solve the problem of vision and hearing, but fail with another issue, say, finger movement – and this work will be useless from the practical point of view and even from the perspective of fundamental research because you cannot study feedback, etc. So, the program on robots is built in exactly the same way.

That is why I paid extraordinary attention to our selections of research topics, and the entire Institute was organized as a computing center. But it was initially apparent that we needed [more people]; by the time the Institute was formed on December 15, we had already invited personnel (and by then I had begun to select them myself) from other cities. That's how we got V. A. Kovalevsky, B. N. Malinovsky, V. I. Skurikhin – I dealt with these staff members and, at the same time, conducted an extensive program for educating and retraining personnel in Kyiv and Ukraine. We needed specialists to create not only our own computing center but also an institute – the Institute of Computational Mathematics, Computer Science, Cybernetics, and Management.

Thus the range of issues that we must solve was outlined. Our number one task was providing calculation services to the institutions of the Academy of Sciences in Kyiv, as well as industry and economics. Another one was creating new machines and their mathematical support. Then came the third task of designing various applications, systems, etc., for economics. At the same time, it was necessary to undergird not only a practical but also a fundamental scientific basis following the above principles.

I took a close look at the seminars we had at that time and organized a number of new workshops where the employees of Laboratory No. 1, which I was in charge of, improved their qualifications.

On December 15, 1957, a branch of the laboratory from the Institute of Mathematics (Computer Science Lab No. 1) was supposed to start operating. This laboratory had to be transformed into an institute. At that time, we already had about 120 employees and some postgraduate students. My first postgraduate student at the University (I simultaneously began to lecture at the University part-time) was A. A. Stogniy, and at the institute – V. P. Derkach.

Until July 1956, I lived alone; then, in August, Valentina Mikhailovna arrived.

Under the Resolution, we had to organize the Computing Center of the Academy of Sciences of the Ukrainian Republic. The Computing Center of the Academy of Sciences of the USSR was formed in 1953, and in 1955 a decree was adopted to establish computing centers in Ukraine, Belarus, Georgia, Uzbekistan, etc. in 1956.

One funny story. In March, when I arrived in Kyiv, Gnedenko was equally eager to invite me to this laboratory and the university department. We went to see

Sidler, then the Dean of the Department of Mechanics and Mathematics. He was sitting there so important; he asked me what department I had headed earlier. Somewhere in a Urals Forestry Institute, the department of theoretical mechanics – nothing impressive. Our university is in the capital city, and we have subsequent requirements here. In short, he was bragging in such a way that I didn't want to go to university immediately. However, I decided from the beginning that I would go to the Academy, not the university.

At the Academy, Gnedenko took me to G. N. Savin. G. N. Savin was an academician who, at that time, held the post of vice president, where V. I. Trefilov is now. He was responsible for the physical, mathematical, and technical sciences section. He was sitting where I am sitting now, in my office.

And, so, here's the conversation. He doubted whether I could manage hundreds at once, when in the Urals I managed only a few people (and these are pretty different things – heading a small department and an institute – nothing similar in terms of organization). We discussed how I would do all this, he approved, and the Academy hired me.

I should say that despite A. V. Palladin being the president of the Academy of Sciences of the Ukrainian SSR, he did not practically lead it. It was managed by the United Party Committee, which is no longer there; B. E. Paton liquidated it. This United Party Committee – Ishchenko from the Institute of Mechanics of the Ukrainian SSR Academy of Sciences then was its secretary – and the Science Department of the Central Committee of the Communist Party of Ukraine helped us immensely in our establishment. Together we solved problems with construction, supply, hiring, residence permits, etc.

Let me say more about organizational principles. I quickly realized that when managing a large team with various subjects, one should apply the principle of decentralized responsibility. I developed it back then and have been adhering to it ever since. It should be noted that not everyone adheres to this principle, but some leaders also come to it somehow with their intuition.

What is this principle about? I allocate areas and appoint managers with deputies, etc., responsible for various research. Then I try to minimize my interference. Even if I see that they are doing everything wrong, I don't direct them specifically, saying that this issue should have been solved this way, but relating to some integral indicators.

This is how it usually manifests itself. For example, I gave the question of apartment allocation to V. S. Mikhalevich. Then someone comes to me complaining that he did something wrong, and they ask me if I agree with their arguments. I replied that I could agree with how you describe the situation, but perhaps Mikhalevich has his own considerations, and we should listen to the other side. They tell me, "Hear him out." I say that I won't. Why not? For a straightforward reason. How much time did Mikhalevich spend on this issue? An hour and a half.

Then I make it simple to explain. If a senior manager, after a five-minute conversation, cancels a decision his subordinate spent hours on, there will be no proper leadership. People usually play on the director's ego: you can do that because you are the director. Who is higher – you or Mikhalevich? But I take a tough line here; I never interfere.

What do I use these conversations for? As in scientific work, I apply them to summarize the fundamental basis for many practical applications. If these were mistakes, we need to find the root cause, and after that, we can make claims. Here I can spend an hour and a half talking to Mikhalevich to make complaints not about individual private issues but about the style of work in general and the underlying principles. This is how I always work, and it has allowed me to build a two-stage management hierarchy, i.e., where I am at the top, then someone else, and then the implementers.

12 But it does not work this well with three or more stages. Because, say, no matter how much I teach A. A. Stogniy and V. S. Mikhalevich to use the same techniques, they cannot do this. They are always bogged down in trying to cover everything themselves. The issue of housing is a question of the real power in the institute, as the saying goes, and that is why nobody wants to miss it. At the same time, more and more things are piling up from above, and they are poorly solved. When I propose to hand specific issues over to someone, they can not believe that this someone will manage them well enough, i.e., it requires more self-possession and an organizational mindset.

Now, when something goes wrong in our institute regarding management, I do not focus primarily on particular mistakes and individuals – it happens that a person simply cannot cope and needs to be replaced. Most often, it is that there is no management mechanism. And management should be based on specific and clear organizational principles.

I will explain it with a very recent example of space distribution in the institute. Mikhalevich, Stogniy, Skurikhin, and the maintenance department had been dealing with this issue for six months and could not allocate anything. They relocated Y.T. Mitulinsky and calculated how many square meters they needed. Mitulinsky comes and says that, in fact, they needed a different amount. And so on.

For example, I do not meet anyone to discuss particular issues and demand that my deputies work with them according to the procedure I immediately invented for them; I am a great master of procedures.

The procedure is as follows. One person, a technical (rather than administrative) worker, keeps files of equipment rooms (with or without a machine – it depends on how many people send him information about changes through the personnel department: hired, fired, etc.). The chief engineer, responsible for the equipment, is charged with developing technical standards for it, determining how much space it should occupy and what kind of workplaces it has. So that employees would not have to plan new workplaces because, very often, there was double or triple counting, etc., which was the basis for all the frauds.

After that, priorities are determined (based on the results of socialistic competition, etc.), and areas are distributed under these priorities. The average amount per person is calculated, and from this average, increments are given up and down according to the priorities. And following this, the area is distributed, that is, almost automatically. Mikhalevich only has to sign the document, or I can sign it.

Now we need to decide how to monitor usage. I suggested that Mikhalevich use the theory of probability, random functions, random sampling, etc., to organize the case. A program with random numbers is made on the machine; every week, it gives out room numbers to be checked by special commissions from the maintenance department. They go there, pull the handle – it is closed. Where are the employees? Some work from home, others are on an extended business trip. They write down how the space is used, and during the subsequent redistribution, they reduce the shares of those who used it worse and increase the claims of the rest. This, in fact, is the whole procedure.

January 5, 1982

The notion of decentralized responsibility includes another vital point. Nowadays, when building hierarchical systems, levels of responsibility are most often assigned according to levels of competence. If someone is given an area of work, that person is considered responsible for everything happening there. In particular, the director is accountable for everything in the institute and may be reprimanded by a higher authority for some misconduct that he had no chance to prevent. The misconduct was committed somewhere at the fifth or sixth level of the hierarchy, and the director himself cannot directly control it.

And the method of decentralized responsibility, as we understand and apply it, is that if, say, a deputy director is entrusted with a particular area and something happens there so that it is necessary to impose a penalty, it should be imposed on the direct perpetrator of this misconduct.

As for the deputy director, he can be penalized either for his own decisions (which are within his level of competence in the hierarchy) or for the misconduct of his subordinates. In the latter case, he is accused not of having made a specific mistake but of having poorly selected personnel and poorly conducted work with the personnel in the subordinate, supervised area. And the work with personnel is a direct responsibility of a leader.

Working with personnel has always been in focus, starting with the entire period of the Institute's formation and throughout its later operation. First of all, we are talking about training and retraining. The work covers all levels – first, those who already work at the Institute. Various kinds of seminars, including scientific and educational ones, were created for them, and lectures were given as the machine design theory developed. Thus, in a few years, it was practically possible to move from intuitive design to meaningful logical structure, first for individual block diagrams, individual sections of block diagrams, and then the whole machine.

We paid special attention to training students. For this purpose, we launched specializations in computational mathematics and computer science at Kyiv State University and Kyiv Polytechnic Institute's Faculty of Radio Engineering. Later these specializations could be turned to the Faculty of Cybernetics at KSU and the Faculty of Automation and Computer Science at KPI.

From the beginning, I demanded that our leading staff members be required to lecture and work with students, either hourly or part-time. We did everything we could to resolve the issue of rates. I must say it was not easy to do this because only professors and doctors of science were allowed to do this [combining teaching and full-time work in a research institution], and we did not have them then. Therefore, with the help of the Science Department of the Party Central Committee of Ukraine, we obtained separate permissions for candidates of science and thus ensured that our young institute had a sufficient influence on the training of students in these specialties.

I also demanded that all employees on business trips to Ukrainian cities with universities close to our specialties visit these universities, lecture there, or hold consultations and get acquainted with the students to make selections before distribution. Even in the third or fourth year, we would find out that in Kharkiv or Lviv, there are capable guys whom we should invite to the institute. This is another form of work.

Then we worked with schoolchildren. From the start, we patronized some high schools where we started teaching programming. Then we began to organize all kinds of contests and Olympiads at the Institute of Cybernetics and launched the Small Academy of Sciences for schoolchildren in the Crimea, where in summer they listened to lectures and the best specialists, both ours and those from Moscow and Novosibirsk. At that time, Lyapunov and Kolmogorov helped us a lot. But Kolmogorov indeed took many of the kids for himself. We organized a boarding school in Feofaniia. At first, it was entirely our institution made under our auspices; its programs were also ours. Then this boarding school was transferred to the university, and the university added physics and all other special subjects, but at first, it was only cybernetics (late 50s – early 60s).

We started lecturing at the House of Scientific and Technical Propaganda for retraining engineers and technicians in Kyiv, then spread throughout Ukraine. The Central Committee of the Ukrainian Komsomol helped us a lot; it took patronage over this case and organized cybernetics schools in major cities, where primarily engineering and technical personnel, future users of machines, were retrained. This was when cycles of lectures on the theories of automata and algorithms were born, which were later published as separate monographs in Kyiv. We had a large army of engineers in Kyiv (since, in Kyiv, there are many radio-electronics engineers) who already knew formal methods of designing electronic computing machines.

This is work with personnel. Therefore, it is unsurprising that in 1969, when the Institute of Cybernetics was awarded the Order of Lenin, the decree's wording said, "...and for training personnel." Few institutes were as involved in this as we were.

We developed curricula for universities, then, of course, postgraduate programs, since there were no such specialties yet, and arranged thesis defenses. We paid much attention to the thesis councils' organization (specialization). At first, of course, it did not work because there were no doctors of science, but then we paid a lot of attention to organizing the boards.

And, finally, we did not forget another link that many people miss. This is the middle one – first of all, technicians-operators of electronic computing machines. We proposed reclassifying the technical school in Lviv Square to train specialists in electronics, and the Central Committee and the Ministry supported us. I don't remember this technical school's earlier name, but I gave two lectures there, and all the teachers and the management got excited. At the same time, the foundations of KIRE – Kharkiv Institute of Radio Electronics – were laid. A solid base for training both developers and users was created in Ukraine.

The critical point in training personnel of the highest qualification (doctors and candidates of science) was the preparation of doctors. Because without solving this problem, we could not get to another one – the lack of people who could supervise postgraduate students and form the core of future thesis boards. Of the four candidates of science Lebedev left (Dashevsky, Shkabara, Rabinovich, and Malinovsky), two, Shkabara and Dashevsky, left quite soon after the formation of the Institute, and only two remained. However, we managed to get several candidates from the Institute of Mathematics – V. S. Mikhalevich (after a big fight with Gnedenko, we managed to lure him to our side), E. L. Yushchenko (Gnedenko allowed her to leave without resistance), Y. V. Blagoveshchensky, and, I think, that's it.

V. S. Korolyuk hesitated for a long time but ultimately did not join. I remember how Mikhalevich and I took a long walk through the forest in Feofaniia, and I told him that he would become a doctor of sciences later than his colleagues at the Institute of Mathematics. But that his further advancement up the academic ladder would be faster, and that he would vote for Korolyuk (in the elections to the Academy of Sciences of the Ukrainian SSR), and not vice versa. In the end, Mikhalevich joined us. All this gave us the opportunity, using exceptions and the positive attitude of the Science Department of the Central Committee of the Communist Party of Ukraine, to organize personnel training. This included being allowed to take post-graduates to candidates of science and we loaded them all with post-graduates. Besides, the candidates were needed as soon as possible, so I started searching in other cities and among the invitees. Thus, for example, I found V. A. Kovalevsky, and Malinovsky found V. I. Skurikhin and introduced him to me, since he was Malinovsky's university friend.

When I selected people to work at the institute, I paid attention not so much to the proximity of their specialty as to their enthusiasm and potential, and also, as Skurikhin puts it, to non-quarrelsome character, to the ability to work in a team, because this is extremely important for our institute – loners, although also needed, cannot form the basis for developments here.

And, of course, the topics were chosen and people arranged in such a way that the topics were as much as possible in line with their interests (this minimized the period of their entry into a new field) and that they could defend their doctoral theses on these topics. This was our doctrine, so we quickly solved the problem of forming highly qualified staff. Starting from 1960, new doctoral theses started appearing quite rapidly. We currently have 60 doctors of science, although we have given quite a few to universities and other organizations.

Our Institute was unique at that time in the speed of personnel training. Of course, Kurchatov and S.P. Korolev were also good at personnel training, but several circumstances facilitated their position. First, they had more opportunities: they paid higher salaries, and doctors of science could join them immediately, because Kurchatov and Korolev were able to quickly make job vacancies for corresponding members and academicians. This is one circumstance.

Another circumstance is that neither Kurchatov nor Korolev were looking for fundamentally new specialists. In other words, they took specialists in mechanics and engines, who had developed their skills in other scientific institutions, to develop rocket engines. In general, it was easier for these specialists to get requalification. But when, say, V.I. Skurikhin, a specialist in electric machines, i.e., strong currents, suddenly became a specialist in system engineering and computing technology, this was a 180-degree turn, which is much more difficult. But we managed to do it.

I can say that at that time – when I was personally involved in recruiting candidates of science and higher-level experts – we rarely missed. We always took people who later took root in the institute, proved to be helpful, and worked effectively for a long time. Subsequently, we invited even several doctors, particularly B. B. Timofeev, and G. E. Pukhov.

Now let me tell you how we worked on computer technology deployment in Ukraine according to these principles. I should mention that the seven-year plan (1958-1965) did not call for a single computer plant to be built in Ukraine, and in general, computing technology was planned to be developed in Belarus, and Armenia — anywhere but Ukraine.

We started work on the automation of a whole range of processes, in particular, the steelmaking process in the Bessemer converter (later in the oxygen converter), the steel-cutting process at shipbuilding plants, control of soda production, control of ammonium nitrate production, etc. But at that time in Moscow, and everywhere in the world, control automation was dominated by traditional school automators relying on continuous techniques. A separate computing device was created for each process, as there were different control algorithms.

When I looked at all these works, armed already with an understanding of what a digital computing machine is and how it differs from an analog one... (further cut off).

January 6, 1982.

To all of the above, I would like to add that the unprecedented decision of the united party committee of the Academy of Sciences of the Ukrainian SSR that the residential building at 11/3 Bolshaya Kitaevskaia be entirely at the disposal of the Institute contributed greatly to attracting and retaining new personnel. This was done for the first time; the usual practice in the Academy of Sciences is that each institute is allocated one, two, or three apartments in one house. We could accommodate our leading staff, which also contributed to strengthening the Institute.

Regarding the multipurpose machine, I already said that automators, and continuous analog devices, dominated technology control automation. So a machine was created for each process, yet it was impossible to develop a device for some of those. Such a machine could work only for a process described by differential equations, not very complex ones.

Therefore, when in 1959, at the All-Union Conference in Kyiv, I proposed the idea of creating a universal control machine, it faced hostility from two sides. Firstly, all the automators headed by V.A. Trapeznikov stood together, claiming that it cannot be, as it can never be. The computing tech specialists also spoke out against it; M. R. Shura-Bura was openly against, while Lebedev's team mostly laughed in their beards. The point was that at that time, everyone imagined a multipurpose device to be a lamp machine, which meant huge rooms, and conditioned air, which did not fit in with industrial production and process control in any way.

But at that time, B. N. Malinovsky was already working on one of the first semiconductor elements for electronic computers in the USSR, which was very helpful for us. Young capable guys joined his team, and we bravely undertook to solve this problem, despite the surprisingly unanimous opposition. Practically no one supported us.

We expressed all the basic ideas that later became dominant – first of all, that the machine must be semiconductor, transportable, highly protected, and low-bit (16-bit) – this is enough to control technology in the vast majority of processes. And most important was the idea of a universal remote telecontrol unit – RTU (a set of analog-to-digital and digital-to-analog converters controlled by the machine and connecting the machine to the production process). All this became basic in our days.

The development of the machine was entrusted to B.N. Malinovsky; he was the chief designer, and I was the scientific supervisor. To save time, I organized the work to finish the project in a record period – it took only two years to get from the idea discussion at the conference in June 1959 to the completion of the machine in July 1961. As far as I know, this result is still a world record for the speed of development and implementation. And this was under conditions when, as I have already said, no one assumed any production of machines (computers) in Ukraine.

So how did we do it? I paralleled all the stages of development, future machine personnel training, and production preparation, wherever possible. That is, on the same day as development started, I went to the Central Committee (at that time, the Defense Industry Department was under the Secretary of the CC Olga Ilinichna Vashchenko, a good acquaintance of N. S. Khrushchev). I told her that no computing machine production was envisioned in Ukraine; we immediately went to Podgorny and decided to organize it – then there were sovnarkhozes [regional economics governance units], and the republic could determine it for itself. Vashchenko found the director of the Kyiv plant Radiopribor and instructed him to prepare the premises and the design bureau to participate in developing and manufacturing the machine. A corresponding decree was issued, as well as another decree of the Central Committee, prepared by us, on constructing the VUM plant with its personnel to be trained within Radiopribor.

Right away, when the factory premises and the design bureau were allocated, I arranged my own lectures for engineers of the design bureau – first of all, on machine design and multipurpose devices. At the same time, work with the plant employees and new technologies was carried out.

Simultaneously, I took care in selecting an automation object. In total, four objects were selected: Mykolaiv Shipbuilding Plant (sheet metal cutting in lofting), Dneprodzerzhinsk Metallurgical Combine (converter control), Severodonetsk Chemical Plant (artificial fibers), and Donetsk Soda Plant. Furthermore, the Dneprodzerzhynsk ammonium nitrate plant was added later. Together with them, we got them prepared for using computers.

At the same time, we launched work to prepare a movable version of RTU, which was one-sided, because, in the beginning, we thought not about replacing a human with a machine in the upper control loop but rather about making an assistant who would advise the master operator on optimization of the control mode. The master operator still had to turn on the control knobs as we understood people's psychology: the converter is an expensive thing, and no director would allow control of it through an incomprehensible machine. Who will be responsible if the steel freezes in the converter? After all, in that case the converter would have to be blown up, meaning tens of millions of rubles in losses. That's why the commands that were built by the machine had to go through a supervisor. This made it easier to create a temporary RTU, since we needed it only from one side — converting sensor readings into a digital code.

The specialists we invited created a device for interfacing with the telegraph network. Data from the mobile RTU was transmitted to our computing center to the "Kyiv" machine, which by that time (in 1958) had already been put into operation. There was interfacing with telegraph channels, and we could come to any enterprise, and usually, large plants have a separate connection to the telegraph line. So we continued this telegraph line to the operator's workplace, connected the sensors via the network using the digital code on teleprinters (Baudot), and at the opposite end, input the information directly into the Kyiv machine. This allowed debugging the software in parallel with the development of the Multipurpose Control Computer (later – "Dnipro-1" (Dnepr). However, the software was intended not for the "Dnipro" but for the "Kyiv" machine. But all the difficulties were bypassed, and the main thing was overcoming fundamental challenges in the control algorithm.

We created a group (not led by B. N. Malinovsky anymore) that developed control algorithms. To be more precise, not a group, but several groups: Skurikhin dealt with Mykolaiv, Malinovsky with the metallurgical plant in Dneprodzerzhinsk, etc. We engaged mathematicians and began to debug control programs, connecting from the side and not interfering with the operators working at the plants in Mykolaiv, Dneprodzerzhinsk, Severodonetsk and Donetsk, while the data were collected from under their hands and transmitted to us in the machine room. According to well-established programs, the "Kyiv" machine began to advise the operator on what modes to maintain next.

Some explanations are required here. Say, melting steel in a Bessemer converter takes 12-14 minutes, but since the process is fast, it is difficult to stop it precisely at a given carbon content. For example, they make low-carbon steel with 0.3% carbon. If the carbon content is brought up to 0.29%, such steel is only suitable for a bed, but it was necessary to get rail steel for wide applications. That is why the operators tried to under-burn: if the content got to 0.35%, they stopped blowing and made an express analysis. Melting lasts 12 minutes, the express analysis lasts 30-40 minutes, and the furnace stands waiting. Sometimes they had to blow it for the third time.

The main task I set here was to reduce the number of blows with simultaneous reduction of rejects, i.e., steel with lower carbon content. We accomplished this task: we increased the converter's productivity by at least 10% using relatively simple algorithms and far from perfect sensors (at that time, there were no precise spectrum analyzers or anything of the kind – all of this came later).

We did the same thing at other production plants. We offered the directors of these plants to arrange competitions of the best dispatchers-operators with the machine: for three days, production was managed by the best operators, and for the other three days, by any operator who needed help from the machine. The results when working with machine assistance were much better everywhere. There were minor successes were in soda production – there, soda output increased by 4%, and in metallurgy, as I have already said, it increased by 10-12%.

The plant directors immediately demanded that we continue these experiments because it was profitable for them. But we said no, we needed a machine for calculations. Here you are, please; the Radiopribor plant is making a serial machine; you can sign up in line. And the people poured in. So we created a market even before the prototype was ready.

From the very beginning, we launched the development as a joint project for the institute and the plant's design bureau without separating where it was ours, and where it was theirs. This saved us at least two more stages because the development was immediately customized for the plant's technology. Usually, the order is as follows: an institute makes a prototype, then a commission comes, writes comments, then the weaknesses are addressed, and the prototype is submitted again, then the manufacturing plant is selected within six months or a year. When the manufacturing plant is chosen, the designers and technologists from this plant come and say, for example, we don't know how to drill these holes, we don’t have a press for such a rack, and everything has to be redesigned. So this all takes a lot of time.

But since the Kyiv machine was made only in two copies, we had fewer problems. And the Dnipro was created as a mass machine, so we had to focus on the plant’s technology right away. And immediately from under the designer’s pen, the things that were to be unchanged in the design of the machine were put into the plant facility for mastering.

Here Matvey Zinovievich (the director of Radiopribor) showed his courage (I have to say that he showed his courage by having a line of a couple of dozen factories behind him). He risked little because even a semi-finished product would have been bought. However, there was still a risk because the state commission could have chosen not to approve it, and the series launch was impossible without this approval. So, there was even a tiny malfeasance committed here. Nevertheless, he started production of ten copies of the machine at the plant facility long before the prototype was presented to the state commission.

When it was presented, everything went very well. The machine perfectly withstood all the tests; for the first time in the country, it worked in a dusty room at a temperature of +50°C (we could not find the temperature limit below zero, because it was in June). And then it turned out that it perfectly withstands various experimental conditions in general. For example, it was transported from KVIRTU (Kyiv Higher Engineering College of Air Defense) to Transcarpathia for military exercises. It was loaded on a truck without special devices or springs, and it shook on our roads. Then it was unloaded, dusted off, turned on, and started working immediately. The machine turned out to be very compact, truly reliable, and adapted to work at industrial enterprises.

Simultaneously, we started another project. At that time, in 1961, we had already started developing the MIR-1 machine. We set the goal, also for the first time in domestic practice, to bring a unified elementary (semiconductor) base for all mini-machines. In 1961, we started to create this series of universal elements, based on which our Design Bureau was born. These were two laboratories, that of Y.T. Mitulinsky and another run by a different group that later, in 1963, formed the basis for the Special Design Bureau of Mathematical Machines and Systems.

Then for ten years (from 1962 to 1972), these elements were essential for machines of the Ministry of Instrumentation and Automation; all devices were made on their base (the elements were produced on printed circuits using more or less up-to-date technology).

As always, there were very few publications in this area, but then it turned out that the Americans had begun work on a multipurpose control semiconductor machine similar to the Dnipro somewhat earlier than we did. Still, they put it into production in June 1961, simultaneously with us. That was one of the moments when we managed to reduce the existing gap to zero, albeit in only one, but a critical direction.

Note that our machine was the first domestic semiconductor machine (unless you take into account special devices that can't be used for all operations). It was the first multipurpose semiconductor machine released to mass production and used for counting, by the way. It also broke another record – the record of production longevity. It was produced for ten years, while for machines, this period usually does not exceed 5-6 years, after which they require serious modernization.

When, before the Apollo-Soyuz joint space flight, it was necessary to tidy up our demonstration room in the Mission Control Center, after a lengthy selection of machines that existed at that time [this work started in 1974 or 1975], the choice was still the Dnipro. Two "Dnipros" controlled a large screen displaying everything – docking, etc. This machine was exported and is still in operation today in many socialist countries.

While the Dnipro machine was being developed, the VUM plant was under construction. As soon as it was built, the production facility, which had already been expanded, and the corresponding part of the design bureau were separated [from Radiopribor] and turned into a new plant. The development of the Dnipro laid the foundation for the VUM plant, and by the time the plant was born, we had introduced the MIR machine, which was also made together with the plant.

And we also realized that even though working with the VUM plant was good, we still needed our own design base. We set a course for creating the special design bureau and its further development. In 1963, a decree was issued establishing the SDB MMS.

January 8, 1982

Today, I want to talk about the formal act of establishing the Institute, the controversies and struggles surrounding it, and the elections.

As I have already mentioned, the Institute of Cybernetics of the Academy of Sciences of the Ukrainian SSR was formed on December 15, 1957. Naturally, the formation of the Institute was preceded by preparatory work, during which, as it often happens, relations with the forming institution somewhat deteriorated. In particular, it concerns my relations with Gnedenko. Here it is important to cover everything correctly because Gnedenko played a significant role in my invitation at the beginning of the formation of the Institute, and I would like these subsequent tensions not to distort everything.

What was the reason for that? First of all, it was personnel. We literally had to fight for every highly qualified specialist. We took some of the people who had been particularly selected by V. S. Mikhalevich and several other young specialists from different departments of the Institute of Mathematics. Here I adhered to the position of fair competition: without hiding anything, I laid out my trump cards and expected that the other side would lay out theirs, and it was up to specialists to decide where they would go. Unfortunately, there still were hard feelings.

Division of property also caused disputes, including those around the passenger cars, which, of course, we needed much more: the Institute of Mathematics was in the city center, and we settled on the outskirts. In addition, almost all the specially classified projects concentrated on us, and we had to transport the related documents.

Shkabara played a nasty role in inflaming passions then. She specialized in constantly playing people off against each other in the lab. Everyone said so with one voice – Malinovsky, Rabinovich, and even Deshevsky. Therefore, one of the first tasks I set was to remove Shkabara from the Institute of Cybernetics. Besides, she was also weak as a researcher. She is passionate but, at the same time, lacks education. Suffice it to say, for example, that she believed that one could solve the problem of artificial intelligence and many others by introducing three-digit logic into the machine: "yes," "no," "maybe."

Such rudimentary ideas have always hindered us, because I aimed the team at solving challenging problems from the beginning, unlike some computing centers and cybernetics institutes, which remained in the position of a "cybernetics talking shop." Furthermore, Shkabara didn't show any integrity because, together with Lebedev, when cybernetics was persecuted, she wrote a philosophical article, "Cybernetics is a pseudoscience and a handmaiden of imperialism," where they proved that they know what a machine can and cannot do, as they design machines themselves after all. That is why everything they say in the West about the capabilities of machines is just nonsense. Then, as soon as cybernetics began to win, she immediately turned into its zealous supporter.

When Shkabara saw that we did not need her services, she began to look for other ways. But, to give her credit, she did one valuable thing: she found N. M. Amosov, established contact with him, and formed a department of biological cybernetics, though not with us, but at the Institute of Mathematics, where she left after we had decided on her departure.

By then, after the Institute of Cybernetics became independent, we had more or less equalized relations with Gnedenko. But his relations within the Institute of Mathematics soured – with his deputies Mitropolsky and O.S. Parasyuk. I should outline that Gnedenko has a problematic nature: he enjoys making fun of people, and they don't like it. He looked down on Mitropolsky as a scientist and did not recognize the whole of Kyiv University either. Therefore, the Mechanical and Mathematical Faculty of KSU opposed him, and gradually the Institute of Mathematics began to turn against him too. Finally, Mitropolsky and Parasyuk started an open struggle to overthrow him from the post of director.

The party bureau made a decision, announcing him incompetent and pursuing the wrong policy. Then, together with Shkabara, he raised a campaign for forming the Institute of Cybernetics. They said that our computing center was just a computing center, but that there was also a need for an Institute of Cybernetics. The Kyiv press immediately joined, and the newspaper "Vecherniy Kyiv" began to issue articles. We had all kinds of cybernetic problems among our tasks; we were established as an institute from the very beginning. So it was a direct blow against us – they wanted to turn us into a calculating station and take all the qualified specialists to the new institute.

Of course, we could not remain indifferent, and spoke calmly about the fact that the Institute of Cybernetics already existed, and we could discuss strengthening it. The Science Department of the Central Committee of the Communist Party of Ukraine and the United Party Committee of the Academy of Sciences of the Ukrainian SSR figured out the matter, and did not let them destroy the institute. The decision stated that on the recommendation of the Presidium of the Academy of Sciences of the Ukrainian SSR, cybernetics should be developed at our establishment. And in February 1962, at our suggestion, the Computing Center was transformed and received the new name of Institute of Cybernetics; at that time, they still wrote "with a Computing Center" in brackets.

Finally, after some stormy meetings, Gnedenko resigned and left for Moscow, where he headed a laboratory at Moscow State University under Kolmogorov. And Shkabara moved to the Institute of Physiology and tried to develop something there but did not succeed. Now, together with Gnedenko, they are writing books where they try to minimize our roles, and in particular my role, in the creation of the Kyiv machine. Generally, this is correct because the "Kyiv" was created mainly by Dashevsky, and neither Shkabara nor Gnedenko nor I had anything to do with it. My only merit is that I expanded the applications (I used this machine remotely for real-time work) and wrote one section in the book.

January 9, 1982

I will add to what I said the day before yesterday. Indeed, they wrote a book about the history of the Institute's creation, where they specifically emphasized that I, although formally listed as the head of the Kyiv machine project, did not actually contribute anything to it. This is generally correct, but it is even more precise that neither Gnedenko nor Shkabara invested anything either. I at least wrote one section in the book, and they did not do even that. Dashevsky did most of this work.

Dashevsky, in fact, could have stayed at the Institute. He was not quarrelsome by character, but he had aggravations with the local party organization, which forced him to leave for the Institute of Gas. Therefore, Z.L. Rabinovich remained the only of Lebedev's associates, candidates of sciences, who was still there. B.N. Malinovsky, although he came to the Institute under Lebedev, saw Lebedev little.

In addition, I had to urgently kick out two more from the Institute – Pentsukh and Yarosh. This was the end of Lebedev's "legacy," which no one could get rid of before me. Yarosh held the position of senior technician on the MESM machine while being a second-year student at the Agricultural Academy, and he wrote complaints about every one of us. I personally conducted the operation that lasted over a year and a half to stay in line with all the requirements of the Labor Code. Ultimately, I kicked everyone out – Shkabara, Pentsukh, and Yarosh.

This immediately helped to improve the climate in the team, and for a long time, we did not have a single anonymous complaint. In the past, it was simply impossible to work – all sorts of commissions kept holding their meetings. Now most of these anonymous complaints come from the SDB, not the institute – most of such letters concern private matters like an apartment allocation or something similar.

That is all I wanted to say about the formation of the Institute. In February 1962, as I said, the transformation took place. Since then, we have been working as the Institute of Cybernetics, and Amosov's department was transferred to our Institute after Gnedenko left. Amosov had de facto worked at ours before because the Institute of Mathematics had no base, and we made a "heart-lung" machine for him, as we possessed small workshops. The first "heart-lung" device in the USSR to be used in heart surgeries was created at our Institute, and Amosov applied it. Then we made artificial valves for him, started pressure chamber treatment, and built a facility at our territory, which housed his laboratory. In general, we expanded in this direction.

Later there were some extensions. In 1963, the SDB was formed, and in 1980-1981, the Software Special Design and Technical Bureau (with A. A. Stogniy) separated from SDB, and the SDB itself became the Central Design Bureau. In essence, it was still called SDB, but several independent design bureaus started operations within its structure.

Then we formed sectors. This is a tradition of the Ukrainian Academy of Sciences: a sector here is bigger than a department, while in the Soviet Union as a whole, it is the opposite. Economic, biological, and medical cybernetics – the full range of cybernetic research was formed.

Now a few words about electoral matters. As you know, I arrived in 1956. At that time, vacancies were announced for the elections to the Academy of Sciences of Ukraine in 1957, and naturally, there was no vacancy for me there. But that time, Valentina Mikhailovna and I were lucky. Gnedenko did not meet the deadline for the department and prepare materials properly; in general, we were not ready for the elections, and the Department of Mathematics and Mechanics elections were postponed to the following year. And when 1958 came, a vacancy was announced.

We pondered it long: it was still early for me to apply in computing, but by that time, I had one open publication, a chapter on machine efficiency (I introduced the concepts of effective performance and the price of the effective performance) in a book on the "Kyiv" machine, two articles published under top secret level and one inventor's certificate. I came from Sverdlovsk with the idea of creating a new data storage device, gave this topic to V. P. Derkach, and, together with him, we received a copyright certificate for this device. Now its significance is somewhat historical, but at that time, it played a certain role.

There needed to be more to claim the title of corresponding member seriously. Therefore, a vacancy in algebra was announced, where I had published strong papers on my doctoral thesis and was preparing an article in "Russian Mathematical Surveys" on Hilbert's fifth problem – it was published in 1959. In sum, I passed clean in algebra. The elections were smooth, maybe even unanimous, because at that time in the Ukrainian Academy, if the Central Committee, the local Party organization, and the bureau recommended someone, everything went easily.

So in 1958, I became a corresponding member of the Academy of Sciences of the Ukrainian Republic. In 1957, I became a candidate member of the CPSU, and in 1958, in November, I became a Party member.

In 1960, there were elections to the USSR Academy of Sciences. I did not intend or prepare to be nominated; the vacancies for corresponding members were only in mathematics. At that time, just at the moment of nomination, I was in the Computing Center of the USSR Academy of Sciences. I went to see A. A. Dorodnitsyn on the third floor. The building of the Steklov Institute of Mathematics had not been built yet, and Steklovka was on the second floor of the Computing Center; they had a shared meeting room. I was told upstairs that Dorodnitsyn was in the hall at the Academic Council (they had a joint council at that time, the Computing Center and Steklovka), and I went to look for him there. He had just come out into the corridor, suddenly saw me, and, slapping himself on the forehead, ran back inside.

After 5 minutes, he came out and said: "I have nominated your candidacy for corresponding members." Since he nominated me, Steklovka voted – and there they had a secret ballot. In its heyday, more than half of the mathematics department members worked at the Steklov Institute, and Steklovka's votes almost automatically ensured a successful election. But those were other times already, and so, of course, it was nice that the Steklov Institute nominated me by secret ballot, though primarily for my algebraic works.

In 1961, I was elected Academician of the Academy of Sciences of the Ukrainian SSR in computer science. By that time, in addition to the monograph on the "Kyiv" machine I have already mentioned, my main works on the theory of automata and books on the theory of algorithms and self-organizing systems had already been published. The work on the Dnipro-1 (a multi-purpose control computer) was also completed; we carried out large-scale work on control at a distance and started the automation of experimental research in the ocean.

We made a continuous line from a buoy equipped with some devices in the Atlantic Ocean to the "Kyiv" machine in our computer room. The data were digitally coded directly in the ocean, sent to a low-power radio transmitter on the buoy top, then to the ship radio station, and from there – directly to the Computing Center and entered into the machine. So we processed data on many buoys in the Atlantic as soon as they were collected. V.I. Skurikhin was responsible for this work on the automation of our research vessel, "Lomonosov." And later, we put the machine directly onboard – it was much more profitable, as the short-wave radio transmitter was unreliable and low-capacity and allowed transferring only a small amount of information.

In February 1962, simultaneously with the reorganization of the Institute, we held a re-election for the Presidium of the Ukrainian SSR Academy of Sciences. Palladin resigned, and Paton B. E. took the seat of the President. We had already invited him to the Institute, and he highly appreciated our work. He knew from his father's experience what it meant to create a new institute, and in just four years, from 1957 to 1961, we immediately achieved such success that the whole Union started talking about it. At that time, they say, in academic hotels, even in Siberia, you could hear people talking about the Institute and our work.

Paton offered me to take the place of the First Vice-President, but I refused because it meant purely organizational work, and I wanted to stay in my specialty. So I was appointed Vice-President of the Section of Physical, Mathematical, and Technical Sciences, and I have been in this position ever since.

In April 1964, I was awarded the Lenin Prize for a series of works on the theory of digital automata and their application to automating the design of computer machines. I must say that mathematicians supported these works, although not unanimously. But instrument makers, the section of instrumentation and computer engineering both now and then poorly supported research on computer engineering and systems, and even more so did not support theoretical works. It so happened that I had very little chance of receiving the Lenin Prize, but M. V. Keldysh supported me. He gave a very thoughtful speech when the work was discussed, and the work passed. And it went well.

In June of the same year, the USSR Academy of

Sciences elections were held. Almost all members of the Mathematics Department of the USSR Academy of Sciences, who understood something about this business, had already visited our Institute by that time: M. A. Lavrentiev, S. L. Sobolev, A. I. Maltsev, M. V. Keldysh (though he came a little later) and others. A. A. Dorodnitsyn visited the Institute from the very beginning of its formation and gave us the greatest and unconditional support at all turns until the last time when we began to claim the leadership of the entire country's computer science.

All department members who visited the Institute of Cybernetics praised our work. Nevertheless, there was one vacancy in the elections to the academicians of the USSR Academy of Sciences under the title "mathematics, including computational mathematics" and four candidates. After the first round, two remained; these were Y. V. Linnik from Leningrad and me. In such a case, they either re-vote or add an extra vacancy.

M. V. Keldysh wrote a request for an additional vacancy. At that time, in 1964, N. V. Podgorny, who used to be the First Secretary of the Communist Party Central Committee of Ukraine at the beginning of my stay in Kyiv, was transferred to Moscow, where he became Chairman of the Presidium of the Supreme Soviet of the USSR. I asked B.E. Paton to call Podgorny with a request to allocate a vacancy. He did, and the vacancy was allocated, so Linnik and I had no conflict and passed for one vacancy together. Then we had excellent relations with him, but unfortunately, he died early. In our department, he was, I think, the only foreign member of the Swedish Academy of Sciences, because he was engaged in multivariate statistics, and there they have the world's best expert in the sphere.

When they announced the Nobel Prize to be given also for economics, he began to prepare my nomination; we have his letter on this issue at home. But at that time, it was too early to nominate me. Full members of the Swedish Academy of Sciences, foreign members, and Nobel Prize winners enjoy the right of nomination for the Nobel Prize. But Linnik died suddenly.

Now I'll tell you about awards and other elections. In 1964, I was elected a member of the Kyiv Regional Committee of the Communist Party of Ukraine. At the XXIII Congress of the Communist Party of Ukraine, I was elected a member of the Central Committee of the CPU, although I was not a delegate. Then I was a delegate to the XXIV, XXV, and XXVI Congresses of the CPSU and the XXIV, XXV, and XXVI Congresses of the CP of Ukraine, and each time I was elected to the Central Committee of the CP of the Ukrainian Republic.

In 1966, I was elected a deputy of the Supreme Soviet of the Ukrainian SSR by the Odesa central constituency, and from 1970 to the present day, I have been a deputy of the Supreme Soviet of the USSR in the Kharkiv central constituency.

Now about the medals I won. In 1969, the Academy of Sciences of the Ukrainian SSR celebrated its fiftieth anniversary. Some institutes and scientists were awarded for this anniversary. In particular, both the Institute of Cybernetics and the Institute of Electric Welding named after E. O. Paton received the Orders of Lenin "For Successes in the Development of Science and Personnel Training," as the order on awarding our institute said.

I became a Hero of Socialist Labor and received my first Order of Lenin. I received my second Order of Lenin at the end of the five-year plan in 1976, and before that, in 1973, in connection with my fiftieth anniversary, I received the Order of the October Revolution. In 1977 (78), in connection with the 225th anniversary of the USSR Academy of Sciences, I was awarded my third Order of Lenin.

In 1969, my team was awarded the USSR State Prize for the MIR-1 machine. It did not go smoothly; the Mathematics Section of the Lenin and State Prizes Committee supported it, but the Instrumentation and Computer Engineering Section did not. It is tough to get this prize – you have to get three-quarters of the votes by secret ballot. Most of the voters don't understand anything about the subject because there are 110 or 115 people of different specialties in the plenum, including representatives of the working class. They basically look at how the sections voted, i.e., they vote automatically. We had strong support from different sides, and we got the prize.

It is challenging to receive the State Prize of the USSR for a second time. It is written in the Statute that this is possible only in exceptional cases and at least five years after the first one. In 1977 we managed to get the second State Prize of the USSR for the theory of computer design (a new cycle of works – not on automata, but on the algebra of algorithms). I received this prize with Y. V. Kapitonova and V. P. Derkach., who did work on automated production.

In 1970, the team under my leadership received the State Prize of the Ukrainian SSR for the "Lviv" ICS.

In 1981, together with Sergeyev from Kharkiv, we received the State Prize of the Ukrainian SSR for several works that were top secret.

Together with a large team, I received the USSR Council of Ministers Award for the Bars system. The specifics of the award are that the Council of Ministers itself does not pay money [the agency leading the project does], so it is allowed to include up to 50 people on the list.

In addition, I received two foreign orders: the 1st Degree Order of the People's Republic of Bulgaria in connection with my fiftieth birthday and for active assistance, contribution, etc., and the GDR Order Banner of Labor of the 1st Degree. I received this one for developing a new machine that controls automatic telephone stations. It is a reed switch, called "hercon" for short: it means a hermetically sealed contact that is a tiny vacuum relay with a quick response time. When we made this machine, it was not recognized at first because the Institute of the Ministry of Communication Industry in Leningrad made a machine in 12 cabinets, while ours was only in one, and it worked better. Now the firm "Robotron" produces this machine, and our country buys it from the GDR.

I also received this German award for implementing several automated systems, particularly at an aluminum plant near Berlin, a machine-building plant in Erfurt, and some others. Another project was the forecast for computer technology development, which served as the base for a five-year plan for the GDR national economy development. I received the order in 1976 based on the results of this five-year plan. The new plan for 1976-1980 referred to our forecast.

I have other, mainly commemorative, medals too.

Now about elections to foreign academies of sciences. First, in _____, without any preliminary organizational work, I was elected a full member of the International Leopoldina Academy in Halle. The German

Academy of Natural Scientists Leopoldina, the oldest German society of naturalists, was founded in 1652, and in 1687 Emperor Leopold I approved it as the Holy Roman Empire Academy bearing the name of Emperor Leopold for the observation of nature and gave it extensive rights and privileges. In 1972, the Academy united over 900 prominent scientists from various countries. Its members included M. Planck, A. Einstein, and other outstanding scientists.

Academician N. G. Basov, a foreign member of the German Academy of Sciences, nominated me, and I was elected a foreign member of this Academy.

During a trip to Poland, I made a very successful series of reports on the algebra of algorithms and programs, and on the prospects of computational mathematics. The Polish Academy of Sciences President listened to the last paper and liked it very much. I was also supported by the head of the Science Department of the PZPR Central Committee, a corresponding member of the Academy. At the suggestion of the President, I was elected a foreign member of the Polish Academy of Sciences.

In addition, I was elected a foreign member of the Bulgarian Academy of Sciences.

Academicians and corresponding members of the Academy of Sciences of the Ukrainian SSR are not allowed to receive ordinary prizes. One can only receive named ones, and by secret ballot. I was one of the first to receive the Krylov Prize (most likely for works related to programming technology).

In 1979, I received the Lebedev Prize. In 1980, I was awarded the Krylov Prize at the USSR Academy of Sciences for a series of works on automated control systems, particularly for Displan.

I have seven gold medals of VDNKh of the USSR, and over 20 inventions.

Next, I would like to tell you about the main scientific work directions at the Institute of Cybernetics of the Academy of Sciences of the Ukrainian SSR. First, I put development of the theory of computing machines with three main lines to the head of my attention: the basics of formal machine design, architecture development, and programming technology. I proposed a method of specialized programming programs, now known as "application packages" – when an organizing program for a group of related tasks allows for large-block programming.

These three directions have passed certain stages of development. At first, methods of automata theory were developed in the field of formalized design, when we have to work with each state separately. Then they were replaced by the algebra of algorithms and programs, i.e., we began to work with algorithms as formal logical objects – we managed to find such an approach. The third stage is the algebra of data structures and its connection with the algebra of algorithms and programs. These are the stages of one line.

The second line went through the following stages. At first, I gave all of the programming tasks, except for one task on specialized programming programs, to E. L. Yushchenko and V. L. Korolyuk, who actively participated in our seminar (while I was hardly engaged in it myself).

The work on specialized programming programs turned out to be untimely, and nobody understood its point. At that time everyone was striving to look for universal programming languages.

Korolyuk and Yushchenko created the Address programming language, which was quite a great success – it was the first time that the notion of address mapping was precisely formulated, which had been lacking in the applied theory of algorithms before.

Then there was a large work cycle I was already involved in. This was the development of algorithmic languages for arithmetic programming, including the Analytic language for the MIR-2 machine. I had a lot to do there.

The next step was programming technology.

And finally comes the synthesis of these lines, which is the "Project" system, which is partially implemented, and is going to be implemented further. Here the works on formalizing design and programming are combined, i.e., the machine is represented as a software and hardware complex. It means that the machine can be made smaller by complicating the programs while all the software is automatically rewritten for the new machine. That is the development for this line.

Computer architecture development goes sideways because a human should still bring constructive ideas; the initial concept should come from them. And the machine system allows us to refine, overcome difficulties, and optimize the design according to this or that combined criterion, which is not possible to do manually even having good architectural ideas, and so on.

Since the late 50s, I have based my direction of machine architecture on a consistent rejection of the principles of the well-known von Neumann. In 1944 in the USA, he articulated the following fundamentals of computer construction. First, the sequential structure of the language – commands are executed one after another. Secondly, the command-address principle – an instruction contains the addresses of operands, and commands are stored in memory just like the operands. Third is the instruction system's maximum simplicity, i.e., the maximum simplicity of the machine language.

The exact way these principles sound is not surprising. I analyzed them from a philosophical point of view. In an age when machines are based on tubes and each digit of an arithmetic device is at least one triode, it is clear that a machine should be simple with simple commands.

At that time, I already foresaw the development of microelectronics, manufacturing structural elements in a single technological process at very low cost. And I formulated a goal for physicists: composite construction of a solid body from which a machine environment is obtained.

In this case, von Neumann's principles are no longer helpful. Instead of the first principle, which we decided to give up, I proposed a machine language because compiling systems was becoming more complicated, and it was necessary to simplify programming not only from the point of view of languages and compilers, but also for the machine, bringing the machine language closer to the input one. This caused sharp criticism from everybody, including S. A. Lebedev. Nevertheless, we implemented this idea in the "MIR" series of computers and continued to implement it further.

The second line took a lot of time and effort.

When programming became a type of industrial production, I involved I. V. Velbitsky in developing its technology.

As for artificial intelligence, here I decided to put the automation of mathematical reasoning, automation of proofs, and as the first stage, automation of algebraic calculations at the head. Following the principle of long-range and short-range goals, we do not just make automation, but design a machine that implements all this. Hence the MIR-2 machine has self-sufficient significance independently of the artificial intelligence program. This is an intermediate stage where some artificial intelligence results are implemented.

True, this is still a primitive artificial intelligence. Formal algebraic transformations were developed long ago, before cybernetics, so common sense does not recognize the transcription of formalisms known before cybernetics as intelligence. Although, of course, when the machine starts cracking both indefinite and definite integrals, it looks compelling because not every lecturer in the mathematics department can take such an integral. And the machine independently finds substitutions, and not easy, but rather difficult ones, and so on.

To accomplish the main task of communicating with a machine in natural language, we must, of course, first of all, automate logical reasoning. This is the easiest part because some formalisms have already been constructed. However, the analysis of these formalisms has shown that they are unsuitable for automation, i.e., classical mathematical logic does not work for this purpose.

Therefore, we have put forward the task of building practical mathematical logic, which is being successfully solved. This is the core, the main line. Then, when this language of mathematical proof is ready programmatically, we will implement it in the architecture of machines.

The second direction of artificial intelligence is related to sensory perception – first of all, artificial vision and hearing. The main thing here, of course, is vision, because a human receives most information through vision. We did not have a single person who could work on this, so I especially found Kovalevsky in Kharkiv, arranged for him to be transferred to us, and organized the work on image recognition.

Following the principle of unity of long-range and short-range goals, we decided that we needed an intermediate output – an automatic machine for reading typewritten letters and numbers. We produced it in a small series of 5 or 8 units. It turned out to be expensive, so getting it to mass production was unprofitable – it could hardly compete with punch cards, and is needed only where there are written reports. So far, it has a special purpose application – it is used where it is impossible or inefficient to put written text and punched cards together.

Then Kovalevsky independently developed speech recognition applications, and T.K. Vintsyuk from Lviv is working on it. This is how we covered the sensory-related line.

I initially stated another mission of automating the moving function, the motor function of robots. In particular, I set the task to make an automatic hand on a cart that could ride along the control panel of any object, switch toggle switches and disconnectors, turn knobs, and so on, while simultaneously perceiving the instrument readings using simplified vision capable of only analyzing scale divisions.

Unfortunately, this task did not find the right person. I could not find anyone who liked to work with mechanics using their hands. I set up this job back in 1959 when no one even mentioned robots. But we failed to find the right candidate to deal with it, and I realized it myself. We picked a person by exclusion – we had the exploitation department head Parkhomenko, who could not do other things, but needed some scientific work. But, of course, he did not succeed. Now the mechanical robot arm is still a bottleneck for us.

Now we have a strong person, V. Rybak, my deputy on the Robot Council, who has adopted my work style. He has connections with the largest defense enterprises in Ukraine and produces good mechanics for them. And we make the control system, mathematical schemes, etc., for him. But this work is very much overdue. If then, when

I ordered this, we had high-quality workshops and could make it, we could have had a hand in 1963, and would have been the first in the world to do so. Unfortunately, you cannot always succeed.

The synthesis of all these lines is in robots – manipulative robots with a hand and vision. The second synthesis is the automation of reasoning with the automation of language constructions.

At the same time, we began work on recognizing the meaning of phrases in Russian, i.e., we stepped into the field of semantic networks, as they call it now. I had A. A. Stogniy and partly A. A. Letichevsky directly in charge of this work, but then I switched Letichevsky to the automation of proofs. And we did a good job, by the way. I did the work with algorithms, and Stogniy made good programs.

When we did it, in 1962, we made a solid impression worldwide; you could say it was a sensation. From the input sentences stream, this algorithm built a semantic network, i.e., defined which words correspond to each other. Say, "The chair stands on the ceiling"; although grammatically correct, the phrase is false semantically. And so on. We created a rudimentary world picture and found a solution for economical coding.

Then A. A. Stogniy left there to go into discrete pattern recognition, into Zhuravlev's subject, and I left this area too, so it withered away. It should have been connected with machine translation, but again we lacked people, and I also did not have enough time to engage in this whole endeavor's algorithmic work. But when I made a report on this subject at IFIP in Munich in 1962, it caused a sensation among the Americans – they had nothing like it. It was then that I was elected to the IFIP program committee. This is such a large area, divided into a number of smaller ones.

The next line is process control. Since the number of processes is unlimited, I did not see our task as being competition with automation institutes. I thought that technological institutes should be engaged in the automation of technology.

Our task is to give a machine or, rather, a series of control machines, to provide textbooks for technical universities that would teach how to program on such machines, and use them and monographs for designers describing how to design discrete control systems. And we need to stay on the ground, leading some complex systems ourselves. Because if you do fundamental work in the abstract without relying on practice, it always goes off to the side, and practitioners do not perceive it afterward. They ask snarky questions that the authors of fundamental research cannot answer. Then the practitioners cast these studies aside without considering them, even if they have kernels of wisdom.

Therefore, we took such a line. Under this line, we made the Dnipro-1 machine, then Dnipro-2. Still, in general, after the Ministry of Instrumentation, Automation, and Control Systems was formed, the responsibility for creating and producing control machines fell on them. Unfortunately, they adopted the line of using universal minicomputers as control machines, and these were the RDR series, i.e., CM-3, CM-4, and not Hewlett-Packard, better adapted for this purpose.

Of course, this knocked us for a loop. Since we had no industry, we could no longer push our work through. There we had to present a fundamentally new idea, something far ahead of American developments. And such an idea has yet to emerge in control machines, so we switched mainly to the theory of technology process control and selective automation for particular complex technologies.

Now, about ideas. There are indeed no fundamentally new ideas in the field of control machine architecture yet. But in 1970, I put forward the notion of a software and hardware complex oriented to classes of applications, based, again, on the general philosophical approach and the history of technology. In the beginning, everyone tries to solve everything with a universal means. Then, when the application areas expand, narrower fields are identified, where mass production of appropriate technical means is still possible. More specialized complexes are made for them.

In particular, we had such concepts as a machine and a system. A system is already adapted to a specific process with special devices, programs, etc. But there was nothing in between. So, I proposed to analyze application areas and find those where the technical complex has common features. For example, to control information technologies (book printing, etc.), sensors and RTUs (remote terminal units) are not needed, but we need, say, data preparation devices on floppy disks. And so on.

All processes are thus developed by hardware and then software classes. Then we make all the hardware and software for such a class once and adapt them from now on.

When I expressed this idea, it met a predictably hostile reaction, just like all my other ideas. And even at the State Planning Committee, they said, "No, it cannot be because Americans do not have such a concept. And Americans do not have it because it is hidden inside the companies that develop such things, but I know they have it."

So, what did I do then? To introduce such a concept in our country, when I went to Finland, I met with a representative of a Swedish computer firm, and they are highly interested in our market. Their own machines are bad, so they buy American ones and try to resell them to us. And the Americans are catching them red-handed. I told them to adopt the concept of a software and hardware complex and announce that their firm specializes in it. You buy a machine from Hewlett-Packard, memory from another company, disks from a third, assemble it, make programs, and sell us the new and improved machine. No one can accuse you of resale – you are making a new product.

Oh, how they seized on the idea! They immediately translated the term into Swedish. I sent the prints to the State Planning Committee, and they exclaimed: "How come it is already in Sweden! Not only in America but in Sweden they have it!". They raised all flags in a hurry and called everyone to catch up. And now there is a decision already in the CMEA (A. A. Stogniy heads the commission), but they have split it all into too narrow classes – they got more than a hundred, which is too much. They need to choose a smaller number. All CMEA countries are already working on this, and we translated the term back from Swedish into Russian.

Recently, we agreed to slowly shift from continuous production technologies to information technologies, such as the automation of newspaper production, bank operations, etc. This area is almost totally neglected in the USSR, while the Institute of Automation of the Academy of Sciences of the Ukrainian SSR and many others are engaged in the automation of continuous technologies.

Another direction is the automation of scientific research. Initially, it was experimental research, and we only processed the results, which meant automatic measurements and processing. I have already said that we did it back in the early 60s – we processed data from the Atlantic Ocean at a distance, and the availability of a control machine with an RTU allowed us to do it earlier than the Americans. They have the CAMAC system, which Nesterikhin preaches, and which is intended for communication with objects; it is better than our RTU Dnipro, but the Americans made it only in 1967, and the USO Dnipro dates back to 1961. But the position of Y. E. Nesterikhin and A. P. Alexandrov is to turn the whole country backward, and in 1977 they forced [Soviet engineers] to copy CAMAC, which the Americans had made in 1967. By then, we already had much better solutions, but they have not yet been approved.

Now I am appointed the head of the all-union target complex program on design and scientific research automation, so we have begun implementing our ideology.

In the future, we envision that this will be combined with deductive reasoning so that the machine not only processes the results but also tests hypotheses and builds theories based on them. In short, it will produce a complete printed product, first dialogically and then independently. Here is a further program in the field of research automation.

The chief designer of the machine “Dnipro-1” was B. N. Malinovsky, who worked together with A. G. Kukharchuk. Now Malinovsky leads the Council for Scientific Research Automation at the Academy of Sciences of the Ukrainian SSR. He has a lot of capable young guys working in this area. I help him because, as the vice-president, I supervise these councils [the Council for Scientific Research Automation, the Council for Computer Science led by A. A. Stogniy, the Council for Robots, the one on ICS from the Presidium led by V. S. Mikhalevich, and many others].

That is why I give them the main directions; now, I set the main task as follows: to organize the production of problem-oriented laboratories, which should be produced at a plant. For example, we have mass application of X-ray structural analysis units. Now one plant produces X-ray machines; another produces spectrum analyzers; the third makes a computer; the fourth produces CAMAC, etc., and the Automation Council assembles them all together.

Of course, this is not an industrial approach, and we will not automate the country at such a pace until the end of the 21st century. Therefore, I suggested the following: not to scatter, but to choose two or three laboratories (we have already defined which ones), and by the end of 1983 to issue complex projects of automated workplaces and equipment interfacing, then to solve the issue of their mass production. In particular, these will be laboratories for X-ray structural analysis, mass spectrography, and several other laboratories used in chemistry, physics, and biology.

I have already agreed with the Tochtelektropribor plant that they will take over the production of such laboratories. Then the Academy of Sciences will be able to order them for itself; the producer will take care of the assembly, as it should be, and it will not be handcrafted as it is now. Of course, for some unique experiments, scientists will have to do the installation themselves. But this should be an exception, not the rule. The rule should be that the industry makes a supervised assembly, and we should make developments for the industry. B. N. Malinovsky did not realize this at once, but when he did, he joined in full force, and I must give him credit, he knows how to work.

Microcomputers take their place in software and hardware complexes and laboratories. In principle, it is possible to put sensors everywhere, pump the entire mass of information into a large machine, process it, and output the result. But then we will get huge, complex information flows, the throughput of sensors must be high, and the software is complicated.

Therefore, the system should be distributed. Part of the processing should be done on-site using a microcomputer built into the instrument, part — on a minicomputer, and it is possible to access a large computer if necessary. For example, to process the results of complex nuclear experiments, we connect a BESM-6 or EC 1060 machine at our CC through a 96-kilohertz wide radio channel — we create such a network using a radio channel. And next to the machine is a minicomputer that directly processes the results of the experiments.

Then it turns out that the experiment is not limited only to data collection. The most complicated part is the experimental setup. For example, for the thermonuclear laser reactor that Basov is developing, the experiment results are processed on a computer, say, in a day, and its finetuning takes half a year — the tuning must be exact. That is why it is vital to solve the problem of computer device tuning, and for this purpose, we should already apply robots. And this should also be included in the software and hardware complex.

Because when, say, X-ray crystal analysis is performed in geochemistry, the crystal has to be rotated, its position in relation to the X-ray beam has to be changed, it has to be moved to the right place, etc. Now the experimenter does all of this, which takes a long time. But in a software and hardware complex, such things should be done automatically. Because otherwise, if the processing of the results takes, for example, half the time, automation cannot speed up the experiment more than twice as fast. We need an integrated approach here. Of course, Y. E. Nesterikhin and his company understand nothing about this; they usually get something 5-8 years after the Americans because this is their work style, and the Americans are just getting to this point.

Now about the implementation of our results. We have substantially automated the Institute of Strength Problems of the Academy of Sciences of the Ukrainian SSR, i.e., material fatigue tests. This will apparently be the first problem-oriented laboratory for all mechanical institutes. Then we did a number of works with N. P. Semenenko at the Institute of Geology and Geophysics of the Ukrainian SSR Academy of Sciences, and with P. G. Kostyuk (the Bogomolets Institute of Physiology), and we automated two experiments. They made a precise micromanipulator in their workshops, and we are adjusting the control of this manipulator.

And finally, computer-aided design systems – CAD. We have singled out the task of computing machine design automation. This task is exclusively on us; we must create the theory of design and many other things. That is why we consider this task separately. In the rest of designing – in construction, mechanical engineering, etc. – we do not have to deal with theory. It's done by the relevant institutes. And we, again, must create software and hardware complexes and develop integrated projects to automate all stages.

We made two such systems: one for builders in Kyiv at the Institute of Experimental Zonal Design and one system under a secrecy level for mechanical engineering in Leningrad. The system for construction design automation worked out well: it automatically produces drawings, project and estimate documentation, etc. V. I. Skurikhin and A. A. Morozov from the Special Design Bureau are engaged in this work.

I forgot to mention that the automation of physical research is closely related to the automation of testing. V. I. Skurikhin and G. I. Kornienko are responsible for testing complex objects: Kornienko for shipbuilders and the Navy, and Skurikhin and Morozov for aviation. When Alexandrov saw our results, his eyes popped out.

Nesterikhin showed him, as the latest achievement, the processing of the experiment data by 16 channels from 16 sensors. And the system developed by Kornienko works on the Caspian Sea and has 1,200 channels. Though only 600 are used now, but it can work with 1,200. Alexandrov marveled and then left, seeming to have forgotten everything he had seen. However, he gave me the all-union target context program, not Nesterikhin.

There is also great cooperation on CAD; we have an all-union target program on the automation of scientific research, complex object testing, and automation of design and engineering works. I officially supervise this work (V. I. Skurikhin is my deputy), and our institute is formally in charge of the automation of complex object testing.

This is another line, and it also interlocks with robots because, for example, you cannot wholly solve the task of automating the design of airplanes only analytically. You still have to make an airplane model with built-in tubes for supplying air and measuring differential pressure with built-in sensors. Currently, this model is made manually, which takes several months.

Even though some parts are already produced on program-controlled machines, we still have to assemble them and fix the sensors manually. The task of developing a micro-robot that could do all this has already been set and is in progress. There is unlimited space here because we see the automated system of the whole science and technology development as the ultimate goal. That is, computers independently do experiments, set up the experimental installation and design it, get results, process them, get primary, secondary, etc. processing, build theories, check if the old theories were correct, and, if necessary, they create new ones. January 10, 1982.

In the field of mathematical methods, we have initially taken such a line to cover some of the most characteristic methods, especially for large machines, and necessary for practical application. In particular, continuous optimization methods, then a number of discrete problems – discrete optimization and discrete pattern recognition, then algebraic and analytic transformations, and some problems of multidimensional mathematical physics.

The area of optimization was assigned to V. S. Mikhalevich, who led the seminar. The discrete methods were first dealt with by A. A. Stogniy, then I. V. Sergienko joined in. The methods of mathematical physics were dealt with by I. N. Molchanov, Ivanov, and others. In this case, we didn't successfully create a team at the all-union level, but we tracked all the best developments. A. A. Letichevskii and the whole team of the "Mir" developers handle analytical transformations.

Another direction, which emerged a little later in connection with the development of the OGAS, is computer networks and data banks. We have Nikulin and Nikitin in charge of the networks, while F. I. Andon from A. A. Stogniy's team at SDTB works on data banks.

As far as networks are concerned, we were the first in the world to express this idea. We were the first to carry out transmissions at a distance and, probably, we were the first to make, if not a network, then at least remote terminals working not in special general use systems for technological processes automation, measurement results processing, etc. [The Americans had already had terminals for special systems before, and so had we].

We made the world's first sketch project of a computer network, which has not been fully implemented anywhere in the world at the moment. I developed this project in 1962-1964 at the personal request of the Chairman of the USSR Council of Ministers A. N. Kosygin, and sent it to the government. But there were no decisions on it.

Data banks are related to this issue because, in the end, the case of data banks breaks down into two problems: data banks for individual machines (here we were not going to compete with the Americans, they had been developing this for a long time, and we were only monitoring it) and distributed data banks for the OGAS – here we were supposed to play the leading role. But unfortunately, we have not yet gathered a team that would do this work at the proper level. Moreover, this is an enormous task, which requires a team not on the scale of the Institute but on the scale of the whole USSR, i.e., we need an integrated target program. We have such a program on EGSVT (the union-wide computing centers network); A. A. Dorodnitsyn heads it, but we're the ones who mostly have to deal with it in practice.

The next direction, which we declared at the start, but that emerged later, as we could not find objects and people at once, is the management of economic entities: enterprises, industries, and, finally, the creation of a national and republican automated system. The work in this area began in 1962 with a design draft creation. Then we started working on specific enterprise management systems, ICS, in 1963-1964. At this time, we began to think about the Lviv system, and started its development in 1965.

V. I. Skurikhin and Morozov were focused on this case. They are in charge of broad research areas in the Institute of Cybernetics and in the SDB MMS, so, of course, they did not do it all on their own, but also V. V. Shkurba, T. P. Podchasova and others were involved.

The line we chose was to make not a unique, but a standard system, for example, for machine-building and instrument-making enterprises to stick to industrial implementation methods. We planned to make an SDB, cooperate with the industry, take care of the installation, and train people in the industry.

This, of course, required much more research and development work than in the case of an individual system. It meant about 25-30 times more work at the initial stage of development because the algorithms and software had to include not only those found, say, at the Lviv plant, but also those that can be used at similar plants. That is, it was necessary to create, so to speak, functional redundancy of the system so that later, when binding, adjusting, installing, and launching the system, one could simply choose from the available stock what would be started up at this particular plant. Here we had to maximize the use of programs working with a tabular representation of enterprise features, to use parameters instead of numerical values as much as possible. Such parametric programs are usually slower and require specific methods to run in the system.

In 1965, I proposed a notion of a specialized operating system intended for systems with a regular task flow plus a small percentage of irregular tasks. The point is that the operating systems installed on IBM-360 machines in 1965 were universally suitable for batch mode and good for computer centers (relatively good, of course) that dealt with random task flows. But in ICS, we usually deal with regular tasks. So, for example, we know that at a particular time, a specific task should come for processing, so we cannot engage in multiprogramming, interrupts, etc. Instead, we can use a schedule and prepare information in advance so that when the task is processed, the necessary information is already there (say, magnetic tapes are set, the first portion of the data is transferred to the RAM, etc.). There must be a notion of task scheduling, and multiprogramming should be used only as an addition to fill in the gaps that arise with irregular tasks and to adjust the new tasks that emerge as the system evolves. This work is essential in connection with OGAS, and I will discuss this further.

Here, the concept of a software and hardware complex oriented to application classes has emerged again, as in the case of process control, only with broader typing.

A new stage in automated enterprise management systems development began relatively recently, in the second half of the 70s. These are the so-called complex ICS, which organically accumulate the issues of computer-aided design, computer-aided technology management, test automation, and organizational management automation. Such a CICS, the first of its kind in the country, is being created now for the new Ulyanovsk aircraft plant. V. I. Skurikhin and A. A. Morozov, together with almost the entire Morozov design bureau, are engaged in it again.

In the late 60s and early 70s, we made two main systems: the Lviv system and the Kuntsevo one for the Kuntsevo radio plant. These systems were developed to cover almost all tasks in the domain of machine-building industries.

We managed to sign the relevant orders that 600 systems under construction at that time in nine ministries (machine-building and instrument-making) must be created based on the Kuntsevo system. But even in the ministry where I. A. Danilchenko works, the Kuntsevo ideology was mainly implemented formally. Before that, they had their own significant developments, for example, in LOMO or the Kirov Plant, so, although they attached the designation "Kuntsevo" officially, only one ministry – the Ministry of Machine Building (head institute in Tula with the director V. N. Zasypkin) practically implemented the typing policy on the basis of "Kuntsevo." It happened because the ministry took up this task later than others.

To some extent, typification has also been carried out at Pervyshin's, at the Ministry of Means of Communication Industry. And the ministries, which did their own groundwork, did not want to give them up. Nevertheless, even within the framework of one Ministry of Machine Building, there are at least 50 systems at large and important plants. In one leap, they caught up with all other ministries and, in many aspects, even overtook them.

Another independent line in the Institute emerged in the area of large systems modeling with the help of universal languages specially developed by us: first SLENG, then NEDIS. Practically, it is, of course, a part of the direction of computing machines and systems design automation. But with the transition to continuous systems, we stepped beyond the design of computing systems, so this line has acquired an independent significance. The department of T. M. Maryanovich is handling this. The prospect here is to combine system optimization methods with modeling languages and large systems descriptions so that it would be possible to define constraints in the corresponding languages, change specific parameters, and the recalculation would be done automatically.

1. Member of the Central Committee of the Communist Party of Ukraine.
2. Deputy of the Supreme Soviet of the USSR and the Supreme Soviet of the Ukrainian SSR.
3. Vice-President of the Academy of Sciences of the Ukrainian SSR.
4. Director of the Institute of Cybernetics of the Ukrainian SSR Academy of Sciences.
5. Member of the Bureau of the Department of Mathematics, Mechanics and Cybernetics of the Ukrainian SSR Academy of Sciences.
6. Chairman of the Scientific Council on the problem "Cybernetics" of the Academy of Sciences of the Ukrainian SSR.
7. Member of the Bureau of the Scientific Council on the Complex Problem "Cybernetics" of the Academy of Sciences of the Ukrainian SSR.
8. Chairman of the Scientific Council on Computer Science and Control Systems of the USSR State Committee for Science and Technology, Presidium of the USSR Academy of Sciences.
9. Member of the Bureau of the Mathematics Department of the USSR Academy of Sciences.
10. Member of the Mathematics Department of the USSR Academy of Sciences.
11. Chairman of the Scientific Council on Applied Problems of the Academy of Sciences of the Ukrainian SSR.
12. Chairman of the Commission on the problems of architectures of large Computing systems architectures and peripheral equipment of the Coordination Committee of the USSR Academy of Sciences on Computer Science.

13. Chairman of the Scientific Council on the problem of “Bionics” (since 1977).
14. Member of the Committee on Applied Methods of Mathematics and Computer Science of the All-Union Council of Scientific and Technical Societies.
15. Member of the Committee on System Analysis (CSA) at the Presidium of the Academy of Sciences of the Ukrainian SSR.
16. Chairman of the State Expert Council of the State Planning Committee of the Ukrainian SSR on Computer Science and Automation.
17. Member of the Standing Committee on International Relations with Governmental Organizations.
18. Member of the Soviet part of the Problem Commission of Multilateral Cooperation of Academies of Sciences of Socialist Countries “Scientific Issues of Computer Science”.
19. Member of the Bureau of the Scientific and Methodical Council for Propaganda of Management Problems in the National Economy of the Board of the Society “Znanie”.
20. Member of the Plenum of the Editorial and Publishing Council of the Academy of Sciences of the Ukrainian SSR.
21. Member of the Republican Council for coordination of scientific research in the field of natural and social sciences.
22. Member of the Committee for State Prizes of the Ukrainian SSR under the Council of Ministers of the Ukrainian SSR.
23. Chairman of the group of scientists on the draft plan for 1980 on computer engineering, control systems and instrumentation (SCST USSR).
24. Chairman of the temporary commission for the preparation of a comprehensive target program to

improve the planning and management of scientific and technological progress in the country.

25. Member of the Scientific Council of the USSR Academy of Sciences on the complex problem "Philosophical and Social Problems of Science and Technology", member of the section "Man, Science, Technology".

26. Member of the Commission on Science and Technology of the Presidium of the Supreme Soviet of the USSR.
27. Member of the scientific and editorial board of the publishing house "Soviet Encyclopedia".
28. Member of the Interdepartmental Commission on Design Automation (USSR Ministry of Radio and Industry).
29. Member of the Presidium of the National Committee of the Soviet Union on Automatic Control.
30. Member of the Interdepartmental Commission on Automated Control Systems of the USSR Ministry of Radio-Prom of the USSR.
31. Member of the Moscow Mathematical Society.
32. Member of the Committee for Lenin and State Prizes of the USSR in the field of science and technology under the USSR Council of Ministers.
33. Member of the editorial board of the journal "Izvestia AS USSR. Technical Cybernetics".
34. Member of the Coordinating Council for the Improvement and Automation of Urban Management in Kiev.
35. Chairman of the Scientific Council of KSA "Computers and Systems".
36. Deputy Chairman of the Coordinating Committee of the USSR Academy of Sciences on Computer Science.
37. Chairman of the Expert Commission for the A. N. Krylov Prize.

38. Member of the section "Scientific and Technological Progress and Ensuring Peace" of the Scientific Council for Peace and Disarmament Research.
39. Member of the Academic Council of the Academy of the USSR Ministry of Internal Affairs.
40. Chairman of the Scientific Commission on Mathematical Cybernetics at the Mathematics Department of the USSR Academy of Sciences. 41.
41. Member of the Bureau of the Scientific Council of the Ukrainian SSR Academy of Sciences on the problem "Biological Physics".
42. Member of the Bureau of the Scientific Council on the complex problem "Philosophical Issues of Modern Natural Science" of the USSR Academy of Sciences.
43. Editor-in-Chief of the journals "Cybernetics" and "Control Systems and Computers".
44. Member of the editorial board of a number of popular science journals: "Nauka i zhizn", "Tekhnika-molodezhi", and others.
45. Head of the Department of the Moscow Institute of Physics and Technology.
46. Head of the Department of Theoretical Cybernetics at Kiev State University.
47. Member of the editorial board of international scientific journals: "Information Processing Letters" (Holland), "American Encyclopedia of Computer Science and Engineering".
48. Foreign member of the German Academy of Naturalists "Leopoldina".
49. Foreign member of the Bulgarian Academy of Sciences.
50. Foreign member of the Academy of Sciences of the GDR.
51. Foreign member of the Academy of Sciences of Poland.

52. Honorary doctor of the Technical University of Dresden.
53. Member of the International Cybernetics Association (PNR).
54. Member of the program committee of the International Congress on Information Processing.
55. Chairman of the program committee of the International Congress on Information Processing.
56. Scientific Director of the Institute of National Economy Management (Moscow).