

## Reminiscences of different years

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I became acquainted with Andrei Dmitrievich at the end of 1951 when after graduating from the Khar'kov University I began to work on the project in the Department of G. N. Flerov.

I was fortunate in associating with Andrei Dmitrievich in the period of his fruitful scientific activity, when, along with solving complicated problems of creating and developing thermonuclear weapons he put forward a number of fundamental ideas in physics. At first it was a contact of a young specialist with the man around whose name legends have arisen as of an exceptionally gifted scientist, the author of the hydrogen bomb. The democratism of Andrei Dmitrievich reduced to a minimum the influence of this difference, so I practically did not feel it, but then it existed objectively and had to affect my perception of events.

The first thing that I noted starting with the first encounters, is the very laconic and characteristic style of his statements. At times his arguments appeared to be fragmentary, devoid of ordinary logic, and sometimes so trivial that they aroused surprise. It took me some time to become used to his manner of expressing his thoughts and to learn how to understand him. That's the way it appeared to me. But in subsequent years of association with Andrei Dmitrievich sometimes I caught myself on realizing that the meaning of some of his statements only becomes clear after some cogitation after a lapse of some time. He had too large a step for ordinary perception on releasing the results of his cogitations. I remember that sometimes at seminars Ya. B. Zel'dovich, seeing that the presentation by Andrei Dmitrievich was not being sufficiently well understood, took upon himself the role of a commentator. In my opinion this did not evoke much joy in Andrei Dmitrievich, but externally he did not express his displeasure.

I also remember the manner of his conversations. He usually liked to sit supporting his chin with his left hand and looking at the person with whom he was speaking. At times his glance appeared to depart somewhere and the impression was created that he had stopped listening to what was being said to him. Apparently at that time his "powerful brain computer" (an expression of Ya. B. Zel'dovich) began working intensively on the solution of some problem. However, it turned out that he had been attentively listening to his partner in conversation. It appears to me that one of the most surprising features of Andrei Dmitrievich was his ability of parallel thinking. Pages of the draft of an article on magnetic cumulation written by Andrei Dmitrievich have been preserved on the reverse side of which there are estimates of the mass of a quark. His habit of doodling while thinking is well known. V. A. Davidenko made a collection of his doodles; where it is at present we do not know.

His calm nature, politeness, and delicacy in dealing with people attracted attention.

I would like to show the special nature of that time and

of association with Andrei Dmitrievich on the example of a small group of experimenters in which I worked in the period 1951–1953. This group, consisting primarily of young specialists (the oldest one—its leader—was 33 years old), was occupied with nuclear physics investigations associated with the development of the first thermonuclear explosive. It should be noted that the "father of the hydrogen bomb" was then 30 years old, and this doubtless facilitated a rapid establishment of contact of members of the group with him.

The leader of our group was a remarkable man and a talented scientist, Yu. A. Zysin, with whom in subsequent years I became friends. He was an inventive man inclined towards scientific fantasy, and I think this attracted Andrei Dmitrievich. Between him and Yuri Aronovich not only business but also personal relationship became established. Their families also interacted. I met many times with Andrei Dmitrievich and Klavdiya Alekseevna in the hospitable house of the Zysins where Ya. B. Zel'dovich, V. Yu. Gavrilov, Yu. A. Romanov, B. D. Stsiborskiĭ and others often met. The surroundings were informal—modest refreshments, discussions, games of chess.

In 1961 Andrei Dmitrievich courageously stood for retaining the moratorium on nuclear testing and this led to his conflict with N. S. Khrushchev. Andrei Dmitrievich remembered that he became isolated: "Only one person came up to me and expressed solidarity with my point of view. This was Yuri Aronovich Zysin who is now deceased."

As they say "the parish is only as good as the priest," and indeed, in our group very good talented young people had gathered, between whom and Andrei Dmitrievich working contact and comradely relationships were rapidly established.

Andrei Dmitrievich warmly reminisced about our group<sup>2)</sup>: "Members of Zysin's group worked in shifts, but, learning that I was coming, they would all gather together and we unhurriedly in a very friendly and calm atmosphere discussed the results of the experiments. I usually went away from them about 9 PM."<sup>3)</sup> It was in one of such visits that I met him. A tall, slender man with a very pleasant face entered the room unhurriedly, smiling and energetically rubbing his hands. As I learned later, this was a sign of his good humor. The photographs of that time reproduce well his soft external appearance of an intellectual.

Our meetings with him were of a regular nature in the course of a year and a half, approximately two-three times a month, as required. During that tense time it was very important to create a calm, informal atmosphere for discussions. Such traits of Andrei Dmitrievich as the knack of listening to the person conversing with him, to treat with respect the opinion of others and not to force upon them his own opinion, facilitated the creation of such an atmosphere, while his calm nature balanced the heightened emotional level of Yuri Aronovich. Usually our meetings began with a

discussion of the results of measurements and of the program of forthcoming experiments and ended, as Andrei Dmitrievich used to say, with "prattle on general subjects." After his trips to Moscow he told us about the latest scientific news. Andrei Dmitrievich used every possibility for obtaining information. I remember that the taking of an oral examination for the degree of candidate, and he was the chairman of the examining committee, turned into a seminar. The person being examined gave a report previously prepared on one of the problems in which Andrei Dmitrievich was interested.

Today it is difficult to imagine how a relatively small group of young people who had only recently completed their student days could manage to make so many measurements carried out in the course of approximately two years. One should at the same time have in mind that parallel with the measurements a great amount of work was devoted to the creation of the experimental base for the investigations. According to today's standards this work would be more than sufficient to occupy the members of the division for much more than a year. The program of the work of our group included the measurement of the constants of elementary nuclear processes, the conduct of experiments on the study of the kinetics of neutron processes in special apparatus simulating the structure and geometry of the thermonuclear charge, the study of processes involved in light nuclei, and a number of other problems, and also the development of a radiochemical method of determining the power of the thermonuclear explosion. To carry out this program required tremendous effort. Daily selfless labor provided in those years the meaning of existence for the participants in the work.

An essential role was played by the psychological factor—we were convinced of the importance and the vital necessity of the enterprise in which we were engaged. This conviction was shared and supported by Andrei Dmitrievich. Usually he took away with him the results of the measurements and they were immediately used in the calculations on which he reported to us at our next meeting. The progress of the work was strictly monitored, so that one had to render an account concerning each constant. And, of course, it is difficult to overestimate the role played by Yuri Aronovich and Andrei Dmitrievich in the fact that over a short period of time they succeeded in converting our group into a creative team capable of solving complex problems. This was a magnificent school in which by carrying out concrete investigations we learned the correct formulation and solution of physics problems, and acquired the experience of experimental work. There was no denying that we were flattered by the contact with Andrei Dmitrievich and his great attention to our work, and this supported our enthusiasm. I shall mention several examples of the participation of Andrei Dmitrievich in nuclear physics investigations. At the initial stage of the work he explained to us the significance and place in the design system of the constants being measured by us. This made it possible to formulate correctly the requirements on the accuracy of determining the constants and thereby to reduce significantly the amount of measurements. In the overall experiments on a model installation the question arose of taking into account the perturbing influence of the measuring channels, and of the channel along which the source was introduced into the center of the apparatus. No

methods were in existence of taking such effects into account by calculation. Andrei Dmitrievich held out a helping hand by using his ability to produce simple models and to give estimates reflecting the physics essence of the situation. He attracted experimenters to this work and they did quite well in solving the problems. Finally his amazing intuition aided in avoiding many possible errors and unnecessary expenditure of effort. I recall a case when, a short time before testing, doubts arose as to the correctness of one of the constants which was essential for the calculations. Andrei Dmitrievich predicted its possible value and I was assigned to make a rush measurement. Several days later I was invited into the office of the scientific director Yu. B. Khariton. Among those present were I. V. Kurchatov, Andrei Dmitrievich, Ya. B. Zel'dovich, V. A. Davidenko, Yu. A. Zysin, and some others; I do not have an exact recollection. Something was written on the blackboard which Yakov Borisovich covered by his hand as I entered. I was asked to give the result that I obtained and they wrote it on the blackboard. After this Yakov Borisovich took his hand away and three numbers became visible. One—was the value of the constant predicted by Andrei Dmitrievich, and the two others were its experimental values measured in one of the institutes in Moscow and by myself. As far as I can remember the difference amounted to not more than 20%.

Our group was situated in a small single-story house with a dome. Previously research on explosive substances was conducted in it. In the former explosion chamber a many-tonne uranium assembly was installed. Here also was situated a neutron generator—an accelerator of deuterons, and detectors of the radiation. Personnel and recording apparatus were situated in an adjoining room behind a shielding wall. Andrei Dmitrievich liked to come to our laboratory. He greatly respected the work of the experimenters: "This was a special world of high-voltage apparatus, of twinkling lights of counting circuits, of the metal (uranium) glistening mysteriously with a violet tinge." But the reality was more prosaic. In order to create this "special" world literally on an empty place, tremendous effort and great inventiveness were required. Contrary to common opinion, there was practically nothing there. Radiotechnical apparatus was constructed from radio parts salvaged from written-off army (Canadian) portable radio sets of the time of the Second World War. The high-voltage apparatus for the accelerator was constructed using parts from ordinary x-ray apparatus. There was a lack of vacuum equipment, of laboratory instruments, of materials. Andrei Dmitrievich tried to help, but the possibilities were extremely limited. To some extent personal connections with some institutes came to the rescue. Also we did not have designers. Nevertheless the task of creating an experimental basis for research was accomplished successfully—detectors and other apparatus that was unique at the time were developed. Moreover, the members of the group helped research workers of a number of institutes in the capital that were drawn into the work of the project. In our laboratory a neutron generator was built which had record-setting characteristics, and this provided the possibility of solving a number of important problems. Later it was transferred to the Institute of I. V. Kurchatov and was used in the laboratory of P. E. Spivak in work on making a more precise determination of the lifetime of a neutron.

I would like to say a few words on living conditions in

those severe years. In the morning, going to work, we met columns of prisoners who were being led to work. To obtain a permit to go on leave from the project was a major problem. I had the opportunity of going on leave for the first time after two years of work. Preparation for the tests of the hydrogen bomb were carried out in conditions of strict secrecy. Access to information was highly restricted, which interfered with work very badly. This led to curious incidents. One of our staff members, having read in the newspaper a report of a successful test of the hydrogen bomb, exclaimed: "Somewhere people are doing good work."

At the same time it would not be correct to describe our life only in somber colors. We were young, and life seemed to be wonderful. Now one is amazed how by working twelve hours a day, and sometimes around the clock, we had time to read much, to socialize, to go visiting, to study, to take part in sports and many other things. But this is the way it was. In the summer time on Sundays, time permitting, the majority went to the stadium where competitions between divisions were organized. I remember that I, in spite of my not very great height, took part in volley ball matches in the same team with such acknowledged master as G. N. Flerov. Or, having barely learned to play tennis, I played against I. E. Tamm who very much disliked losing. Chess and table tennis tournaments were held between the theoreticians and the experimenters. In the winter we went cross country skiing in the forest. I recall with gratitude the lectures which were

given to the young specialists by I. E. Tamm, Ya. B. Zel'dovich, D. A. Frank-Kamenetskii, V. Yu. Gavrilov, and others. And, of course, when one succeeded to get away for a few days to Moscow or Leningrad there were the theaters.

At the beginning of August 1953 we completed the laboratory stage of nuclear physics investigations—calibration on a neutron generator of the indicators for determining the power of an explosion. Their irradiation lasted continuously for several days. On August 12, 1953 the successful test of the first Soviet thermonuclear charge concluded the extensive work of the team of the project, which is now the All-Union Institute of Experimental Physics.

Recalling what it was that distinguished Andrei Dmitrievich from other well-known scientists and wonderful people with whom we had the good fortune to be closely associated in those years, it seems to me that first of all—it was the spontaneity and naturalness of the manifestation of his talent, of the internal decency and benevolent attitude to people. This also found expression both in the trifles of daily life and in those cases when effort was required to manifest them. It was said that when the person in charge of the secret department in which particularly important documents were stored turned to Andrei Dmitrievich with a question which of the documents it is necessary to save first of all in case of an exceptional situation, the answer came that first of all it is necessary to save the scientists and their families, and they will be able to write as many documents as is needed. I

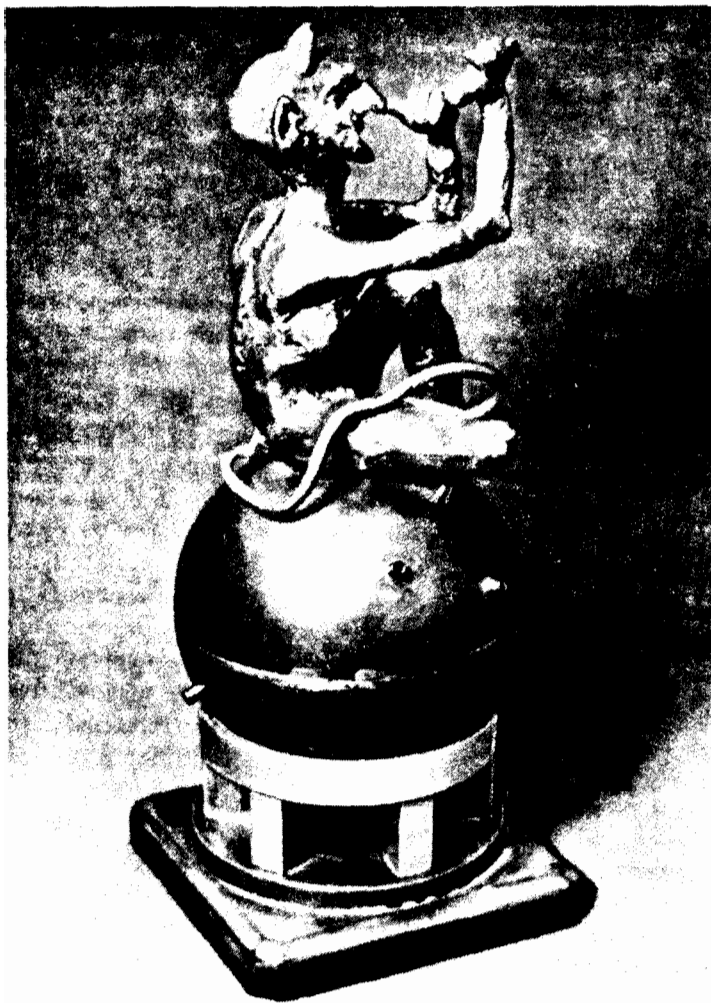


FIG. 1. Mock-up of the model assembly of the first hydrogen bomb (Yu. S. Klinov).



FIG. 2. L. V. Al'tshuler, E. G. Bonner, and A. D. Sakharov, 1987, Moscow, Chkalov Street.

remember that one of my friends was defending his candidate's dissertation. At this moment Andrei Dmitrievich, who had just flown in from Moscow, appeared in the room where the examination was being held. Having heard the defense he intervened and said that the day before he was present at the defense of a doctoral dissertation in one of the institutes in the capital which was inferior to the work being presented here, and that it would be fair to award to the person being examined the degree of doctor of sciences. His proposal was accepted.

Well-known is the case when in 1951 (!) Andrei Dmitrievich and E. I. Zababakhin made a presentation to the higher administration (A. P. Zavenyagin) in defense of L. V. Al'tshuler and dissipated the danger threatening him. Likewise Andrei Dmitrievich did not remain indifferent to the fate of the driver of a car (in which Andrei Dmitrievich and Yuri Aronovich were riding) who was being disciplined by appropriate authorities after a small accident with the car. One can cite many such examples.

Association with Andrei Dmitrievich was for us, the young men, a school of kindness and morality, a school of out-of-the-ordinary approaches to scientific problems and to their solutions. I have retained the strongest impression of his unusual personality.

With the passage of time the views of Andrei Dmitrievich and his evaluation of events underwent a change. He, the great humanist, was frequently asked how he evaluates his participation in the creation of thermonuclear weapons. The same question is posed also to other participants in this work.

It seems to me that the most complete answer he gave in an interview at the time of the 38th conference of the participants of the Pugwash movement in 1988. He said: "But, fate caught up with me... And when I was brought into this work (and we, I repeat, regarded it as important and necessary) I began working not as a result of coercion but of conviction, and providing much initiative. But I can also not hide a different aspect, the work was of great interest to me. This is not

what Fermi called 'interesting physics,' here the interest came from the grandiose nature of the problem, the possibility to show what you yourself are capable of, in the first instance to show this to oneself." The logic of the development of science inevitably led to the creation of nuclear and thermonuclear weapons, and fate selected Andrei Dmitrievich. The history of the work on creating these weapons in the USA and the USSR confirms that scientists deprived of information on each other's work think in the same way, and this is determined by the inner logic of the scientific process. Mankind can expect, whether it wants this or not, many other great scientific discoveries which can represent a potential danger for it. The most important point, which Andrei Dmitrievich repeatedly emphasized—is the recognition by scientists of their great responsibility. He set such an example and did all that he could to ensure that this terrible weapon should never be used.

In 1990 an American journal published the article "The H-Bomb: Who Really Gave Away the Secret?". In it is stated that the information transmitted by Fuchs could not have aided the Russians in creating a hydrogen bomb, but at the same time yet another attempt is made, with quotations from the well-known physicist Hans Bethe to show that "from the ratio of various isotopes Andrei Sakharov could quite easily have drawn the conclusion that...the thermonuclear reaction took place in compressed material.<sup>4)</sup>" The reference is to the fact that at the end of 1952 a complicated heavy (65 tons) device "Mike" was exploded on the ground in order to check the Ulam-Teller idea of the "real" hydrogen bomb, or, as Andrei Dmitrievich called it—"the third idea." As a result of the explosion a large amount of radioactive products was formed, the element analysis of which, in principle, can enable one to make definite conclusions on the structural features of a thermonuclear charge. The authors of the article write: "And so, by detonating "Mike" the United States may have led the Soviets to the correct approach." According to Bethe, "I think this is probably true. I can't prove it,

and I'm sure that Sakharov, although he is now free to a certain extent, hardly intends to tell us." I shall attempt to comment on this briefly. Naturally, attempts to analyze the products of the "Mike" explosion were undertaken, and Andrei Dmitrievich mentions one of these in his book "Memoirs". However, they turned out to be unsuccessful for a single simple reason—at that time we simply could not carry out such an analysis. First of all we did not have a method of collecting samples of radioactive products of an explosion in the region of the Pacific Ocean (the "Mike" explosion was carried out at the Eniwetok atoll), which is extremely important for obtaining informative samples. The propagation of products of the explosion in the upper layers of the atmosphere to some distance from the point of explosion is accompanied by an essential change in the content of elements, which excludes the possibility of reliable conclusions. Also we did not have the methodology of analysis and the apparatus for detailed analysis of the samples with respect to the content of elements. For these reasons the analysis which was restricted primarily to the search for  $\text{Be}^7$  and  $\text{U}^{237}$  was done on atmospheric fallout (snow) that fell in the middle region of Russia. As should have been expected the activity of the samples obtained turned out to be at the level of the natural background which excluded the possibility of any sort of analysis. One can assert that Andrei Dmitrievich did not have any information about the element content of the products of the "Mike" explosion. The development of the "third idea" of Andrei Dmitrievich and his collaborators was based from beginning to end on their ideas and on the calculations of our mathematicians. It should be noted that in November 1955 the first test was made for releasing from an aircraft a military variant(!) of the Soviet "real" hydrogen bomb.

One of the beautiful physics ideas put forward by Andrei Dmitrievich in 1955 was the magnetic cumulation of energy. This amazingly simple idea is based on the law of electromagnetic induction which is well known to everybody. But, in order to arrive at it, one had to look in a new way on well-known matters, and this is what Andrei Dmitrievich did. This quality of his was thus noted by I. E. Tamm: "Sakharov looks at everything as if he has a blank sheet of paper in front of him, and therefore makes amazing discoveries." I remember when this idea became known to one very inventive man, he for a long time could not reconcile himself to the idea: "How is it that I did not think through to such a simple matter."

The idea of magnetic cumulation came to Andrei Dmitrievich first of all as a possible solution to the problem of controlled pulsed thermonuclear fusion. At first he was examining the possibility of using a powerful gas discharge induced by a rapidly varying superintense magnetic field. He soon discovered difficulties in realizing such a scheme, and subsequently other methods of attaining conditions of thermonuclear fusion under conditions of high density of magnetic energy were discussed. In particular an examination was made of compression by shells, accelerated by pressure of a superintense magnetic field, of a previously heated plasma, magnetic confinement of a powerful high-current discharge and a number of other methods. Andrei Dmitrievich anticipated subsequent events. Publication in 1960 of the results of investigators in the Los Alamos laboratory who

reported obtaining a magnetic field of 14 million oersted led to intensive development of work on magnetic cumulation in laboratories of many countries. The main stimulus for this work was the hope for rapid success in solving the problem of pulsed thermonuclear fusion.

It should be noted that when Andrei Dmitrievich learned of the construction of a ruby laser he put forward a thought natural for him of utilizing laser radiation for exciting explosive thermonuclear reaction in small spheres with a DT-mixture. At a seminar in 1960 or 1961 he for the first time laid the foundations for a scheme with implosion; subsequently it was improved as a result of surrounding the sphere by a shell (of heavy elements) with openings for the introduction of laser radiation into the gap between them. The estimates by Andrei Dmitrievich were made more precise in the calculations of N. A. Popov. Today targets of that type are widely used in research on laser thermonuclear fusion.

The second problem which stimulated the development of the idea of magnetic cumulation is the construction of a nuclear charge with small energy release. The estimates made by Andrei Dmitrievich showed the promise of utilizing magnetic pressure for compressing small masses of active material with the aim of bringing them to the above-critical state. Here the basic difficulty, which until now has not been overcome, is associated with producing spherical compression of matter by a magnetic field which is in principle cylindrically symmetric.

One should note the approach to the problem which is characteristic of the creativity of Andrei Dmitrievich. He not only put forward the idea of magnetic cumulation, but also proposed specific construction schemes of devices in which the transformation of the energy of explosion into magnetic energy takes place—magnetocumulative generators. For the problem of thermonuclear fusion—the generators of a superintense magnetic field—MK-1 and for magnetic compression of materials—energy generators MK-2. In his report devoted to magnetic cumulation along with the presentation of the principle there are sketches of devices with characteristic dimensions which have been carried out by Andrei Dmitrievich.

Although these problems have not been solved, magnetic cumulation of the energy of the most powerful energy source, such as a chemical and a nuclear explosion, opened up new possibilities of research in different fields of physics. Today there exists no alternative to the explosive method of generating superintense magnetic fields obtained within relatively great volumes. On the basis of the MK-2 generators compact powerful pulsed sources of energy have been produced with characteristics which correspond to the limiting possibilities of modern pulsed energetics.

Andrei Dmitrievich was proud of the idea of magnetic cumulation and took active part in work on its realization until 1968. In spite of his being very busy in those years, Andrei Dmitrievich devoted relatively much attention and time to this work, which sometimes made theoreticians jealous. In meetings with him both current problems were discussed, as well as different projects including fantastical ones. He trusted us and allowed us complete freedom of action. Sometimes Andrei Dmitrievich came to the explosions area, where experiments with magnetocumulative generators were conducted. He admired the experimental tech-



nique—synchronous operation of devices in the explosion experiment. I remember that he expressed particular surprise on the ability of some of the experimenters to predict the polarity of the signals from the receivers recorded on an oscillograph.

In the spring of 1952 on the initiative of Andrei Dmitrievich first experiments were conducted in which a compression of a magnetic flux by an explosion was achieved, and the initial magnetic field of thirty thousand oersted was amplified to 1 million oersted. This experiment was the beginning of work on the generation of superintense magnetic fields. In 1964 in one of the experiments a record-breaking magnetic field of 25 million oersted was recorded. At a meeting honoring Yu. B. Khariton in connection with his sixtieth birthday Andrei Dmitrievich presented to him a print of the oscillogram of the signal from the detector of the field in this experiment.

In 1965 Andrei Dmitrievich received an invitation to the first international conference on megagauss magnetic fields generated by explosions which took place in Italy. We had something to present at this conference and he made an effort to obtain permission for this trip. Later he described this story in some detail. In one of the culminating moments I became a witness of the "conversation" of Andrei Dmitrievich with the top administration, and for the first time I saw this usually calm man in a state of extreme irritation. Having used up all the sensible arguments and seeing the uselessness of further effort, Andrei Dmitrievich switched to high tonality supporting his words by energetic pounding of the fist on the table. But even such energetic action on his part did not help us—we did not receive permission to participate in the conference. Still Andrei Dmitrievich succeeded to send brief abstracts of eight reports which were published in the Proceedings of the conference. However, the reports themselves were not sent, and we lost priority in a number of essential questions.

In 1957 the first publication on magnetic cumulation by Ya. P. Terletskii appeared referring to priority of 1952, i.e., when we already were carrying out investigations. Although no question arose regarding priority of Andrei Dmitrievich he was saddened. However, when we in an article which we were preparing for publication wrote: "Ya. P. Terletskii, apparently (!), independently put forth the idea..."—told us off so strongly that we remembered it for a long time.

After the publication by Ya. P. Terletskii we undertook repeated attempts to publish our article. On one of the copies of the article that once again had been sent back to us Andrei Dmitrievich wrote: "Lay it aside for a while and submit again." Our first publications<sup>5</sup> appeared in 1965. The article was signed by the principal participants in the work during the period 1952–1965. Subsequently the authors of the article began to be named in the literature as the "Sakharov group." In addition to them the following people participated in the work: G. A. Tsykov, A. A. Chvileva, K. I. Panevkin, and others.

Andrei Dmitrievich noted a wide circle of possible applications of magnetic cumulation: study of properties of materials in superintense fields, investigations in plasma physics, simulation of astrophysical phenomena, attainment of high pressures, and others. However, the most fundamental scientific application he regarded as the creation of superpowerful MK accelerators of charged particles to ultra-

high energies— $10^{12}$  eV. The projects of such accelerators examined by him were, as he himself noted, "almost fantastical," the realization of which required carrying out underground nuclear explosions of megaton power. Andrei Dmitrievich also noted the possibility of carrying out experiments on colliding beams from two MK-accelerators using pulsed lenses with magnetic field energies of hundreds of kilotons. It is amazing that he, understanding the gigantic difficulties of realizing these grandiose projects, repeatedly returned to them. After many years of thinking about them he wrote: "I consider that one-time systems with record-breaking characteristics can also give very significant information. I do not exclude the possibility even now that at some stage we shall have to come back to pulsed MK-accelerators." These grandiose projects have not been realized. However, the question remains: Did Andrei Dmitrievich really see such important problems that merited the expenditure of great effort and means, or was this a manifestation of his tremendous creative potential?

After Andrei Dmitrievich went away from the project in 1968 meetings with him became rare although he had not lost interest in the work on magnetic cumulation. At that time (1969) he had a great sorrow—Klavdiya Alekseevna became seriously ill and he undertook desperate attempts to save her. Later on he reproached himself that he entrusted her treatment not to the right doctors. After the tragic event—the death of Klavdiya Alekseevna—we visited him and spent quite a long time with him. We were uncomfortable to see the living conditions in which the world-famed scientist had to live. But the main thing that astounded us was the silence of the telephone, as if everyone had forgotten of his existence. After this we occasionally met him in the cafe "Kontinent" on the Sokol where he used to take dinners with Lyuba and Dima. Andrei Dmitrievich suffered keenly from the calamity that befell him and gave the impression of an unhappy man abandoned by everybody. This was a hard time for him. Not only his lifestyle changed, but also his social-political views were undergoing an evolution.

I remember well meeting with him on 21 May 1971 when I was commissioned by the "Sakharov group" to go to congratulate him on his fiftieth birthday. Our skilled craftsmen prepared a mockup of an underground experiment on the realization of a powerful MK-accelerator—the subject of his cogitations over many years. The mockup operated according to a prescribed program—imitated the sound and flash of an explosion, the mountain was rent asunder and within it one could see the accelerator. It seemed to me that Andrei Dmitrievich was pleased by my coming, and he also liked the gift. He played with the mockup for a while, and then remarked that we overdid the energy of the particles. When Dima came back from school the three of us played with the toy. Andrei Dmitrievich was in a reasonably good mood and we could quietly talk about work on magnetic cumulation before the arrival of guests. It seemed to me that he listened to me with interest.

In 1979 when I was nominated as a candidate for becoming a Corresponding-Member of the Academy of Sciences of the USSR Andrei Dmitrievich acquainted himself, on the basis of the materials presented, with the then current state of magnetic cumulation. He appreciated the method that had been found for stabilizing the process of magnetic



FIG. 3. A. D. Sakharov, 11 December 1989, Physics Institute of the Academy of Sciences.

cumulation by compressing the magnetic flux by a system of coaxial shells; the cascade generators of reproducible magnetic fields in the range of tens of megaoersted; methodological developments and a number of other results obtained in recent years. As I have been told later his convincing presentations played not a small role in my election.

Before the tragic event—the exile of Andrei Dmitrievich to Gor'kiĭ—we succeeded in the course of several short meetings to discuss with him a number of questions. After his return from Gor'kiĭ we met in 1987 at the spring general meeting of the Academy. He was pleased to see me and began with animation, as in earlier years, to quiz me as to how the young boys are doing, how is Robert (Lyudaeв), what's new. The conversation was not a long one, after a few minutes he hurried away to give an interview. Only when we began to say goodbye I paid attention to his external appearance and was amazed as to how much he had changed. This aged stooping man little resembled the Sakharov whom I remembered.

In the course of subsequent meetings at the Academy we succeeded in discussing some problems in mounting experiments on the generation of magnetic fields of  $10^8$ – $10^9$  Oe in the case of magnetic cumulation of energy of a nuclear explosion of not very great power. He promised to think

about experiments in such fields, but I had a feeling that political activity had completely captured his attention.

I also remember our last meeting—the spring general meeting of the Academy in 1989, Andrei Dmitrievich in the presidium. During a break I came up to him and gave him a journal which I brought from the USA. We exchanged a few words and he for some reason decided to introduce me to E. P. Velikhov with whom I had been acquainted since long ago. Then photographers surrounded us and some people unknown to me and pushed me aside.

Forty years had passed from the time when Andrei Dmitrievich first put forward the idea of magnetic cumulation. Researchers in the USSR who had solved the problem of stable generation of record-setting fields and who had constructed the most powerful pulsed sources of energy made a determining contribution to its successful realization. International conferences, that have already become regular, on the generation of megaoersted magnetic fields and related experiments make it possible now to speak of the development of a new promising direction in the physics of high densities of energy—megaoersted physics, one of whose creators was Andrei Dmitrievich. His idea of mounting extreme experiments—achievement in terrestrial conditions of magnetic fields of  $10^9$ – $10^{10}$  Oe, characteristic for astro-

physical objects now awaits its solution. One of such possibilities which is of considerable scientific interest—is magnetic cumulation of the energy of a nuclear explosion. Such experiments can be carried out using underground nuclear explosions with relatively low energy release which would not produce ecological damage. Andrei Dmitrievich considered that international cooperation should be a necessary condition for such experiments.

Andrei Dmitrievich was a remarkable man, and I am infinitely grateful to fate for her gift to me of the good fortune of having personal contact with him.

<sup>1)</sup> The article has been written for a collection of reminiscences about A. D. Sakharov.

<sup>2)</sup> The group included: G. P. Antropov, Yu. S. Klintsov, A. Lbov, P. P. Lebedev, N. G. Moskvina, F. Nasyrov, A. I. Pavlovskii, V. N. Polynov, O. K. Surskii, V. P. Tsarev, and others.

<sup>3)</sup> Here and further below quotations are taken from the book "Memoirs" (in Russian), Chekhov Publishers, New York, 1990. (An English translation has been published by Hutchinson, London, 1990).

<sup>4)</sup> A Russian translation of the article *The Hydrogen Bomb: Who Really Gave Away the Secret?*, published in the Bulletin of the Atomic Scientists in Jan./Feb. 1990, is available in Inzhiner, 1990, No. 8 p. 30, No. 9, p. 28, and in this issue of Usp. Fiz. Nauk.

<sup>5)</sup> A. D. Sakharov, P. Z. Lyudaev, E. N. Smirnov, Yu. I. Plyushchev, A. I. Pavlovskii, V. K. Chernyshev, E. A. Feoktistova, E. I. Zharinov, Yu. A. Zysin, Dokl. AN SSSR 196, No. 1 (1965) p. 65 (reprinted in this issue of Usp. Fiz. Nauk) [Sov. Phys. Dokl. 10, 1045–1047 (1966), also S 3, pp. 23–34].

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