

FROM THE HISTORY OF PHYSICS

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On the history of the research into controlled thermonuclear fusion

Contents

Abstract. **The problem of controlled nuclear fusion (CNF) is a colossal scientific and technological challenge on a global scale; enormous teams of scientists in many countries are still trying to solve this problem. 50 years ago, on May 5, 1951, the USSR Council of Ministers Resolution enacted a governmental program, apparently the first in the world, "On conducting research and experimental work to clarify the feasibility of building a magnetic thermonuclear reactor". The three papers below briefly outline the history and sequence of events together with the evolution of ideas that led to the first governmental decisions to carry out the work that would clarify whether the creation of a controlled thermonuclear reactor was feasible, and also summarize the results of the first decades of research.**

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The initial period in the history of nuclear fusion research at the Kurchatov Institute[1]

V D Shafranov

1. Introduction

The first period of research into controlled nuclear fusion (CNF) can be defined as the years 1951 – 1975. By the end of this period, tokamaks, i.e. machines with toroidal electric current and strong magnetic field, had become dominant among systems with magnetic plasma confinement. In view of the progress in thermonuclear weapons this research was absolutely top secret, which left a dramatic imprint on its history. We can mention, for example, that the adoption of the official programs on fusion research in the USA and the USSR was stimulated by the President of Argentina having announced an allegedly successful implementation of a controlled fusion reaction in his country.

This article discusses the changes in the direction of fusion research at the Kurchatov Institute of Atomic Energy at the early stages, such as a switch from the already planned laboratory-scale toroidal models of a magnetic thermonuclear reactor (MTR) suggested by A D Sakharov and I E Tamm to linear pinches, and then the switch back from pinches to toroidal systems (tokamaks).

The article mentions the first international contacts on fusion-related problems that took place prior to the 2nd Geneva Conference on Peaceful Uses of Atomic Energy (1958) where for the first time there were presentations of declassified research that was being conducted at the time in a number of countries.

We mention a period of profound pessimism in the possibility of solving the CNF problems (roughly the second half of the 1950s). We discuss the first successes in plasma stabilization in open traps during the initial period of work, which instilled optimism into researchers. This success led to the concept of an 'average' magnetic potential well acting as a stabilizer of magnetohydrodynamic instabilities; the principle also worked in closed systems of plasma confinement (tokamaks and stellarators).

The article ends with a brief outline of the strategic approach to participation in the international pilot project for a thermonuclear reactor based on the tokamak principle and certain innovative lines of research at the Russian Research Centre 'Kurchatov Institute'.

[1] This article was written in Russian on the basis of a report presented in English at a seminar on December 21, 2000 during a visit to the National Institute for Fusion Science in Toki, Gifu, Japan.

V D Shafranov Institute of Nuclear Fusion, Russian Research Centre 'Kurchatov Institute'
pl. Kurchatova 1, 123182 Moscow, Russian Federation
Tel./Fax (7-095) 196-76 76
E-mail: shafran@nfi.kiae.su

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2. General remarks

Nuclear fusion research began in the mid-20th century primarily in the countries occupied with building hydrogen bomb weaponry. The reason was simple: these were the countries with accumulated knowledge and experience of managing and implementing the high-cost projects required for this type of research. As for the unheard-of speed of organizing the research work, this is explained by the following two factors:

(1) The original purpose of designing nuclear fusion reactors with deuterium plasma was primarily the generation

of bomb-grade materials (charges) for thermonuclear weapons. Both in the USSR and the USA, the paramount stimulus for making a decision on launching the CNF project was a determination not to be left behind by the opposition in becoming armed with the most powerful weapons.

(2) Successes in designing thermonuclear bombs led to confidence in a similarly fast solution to the problem of designing a nuclear fusion reactor. This hope proved too optimistic; however, a reorientation of the program to the generation of electric energy using the inexhaustible and ecologically clean source of fusion reactions became a driving stimulus for attacking the CNF problem the world over.

The 25 years in the history of the nuclear fusion research at the Kurchatov Institute that we are to describe cover the years from 1951 to 1975. It should be emphasized that as early as 1955 H Bahba who chaired the First International Conference on Peaceful Uses of Atomic Energy dared to predict at its opening that "a method of controlled release of the energy of nuclear fusion would be discovered in the next 20 years", i.e. by 1975. In a certain sense, this prediction proved correct. By this time, a plasma at a temperature of the order of 1 keV was created on the T-3 tokamak and the modified T-4 device (1968 – 1969). At the beginning of the 1970s, a decisive turn to tokamaks occurred in many laboratories working on the magnetic plasma confinement. At the Kurchatov Institute, the year 1975 ended with the launch of a sufficiently large, for the time, T-10 tokamak (still operating today). A plasma with an ion temperature of 7 to 8 keV was generated somewhat later (1978) on the PLT tokamak of the same generation (launched the same year in Princeton) and using the injection of a fast deuterium atomic beam.

For us, this initial period is mostly connected with the name of L A Artsimovich, the first head of the state program in CNF research; he died in 1973.

At its early stages, the thermonuclear research was strictly classified even after its orientation was switched from support of military programs to peaceful uses of atomic energy. Inside the guarded territory of the Laboratory of Measuring Instruments of the USSR Academy of Sciences (LIPAN was the code name of the future Kurchatov Institute), only a small group of senior researchers knew what was going on in the new building of the Bureau of Electric Instruments (BÉP) which was situated not far from the building of the Department of Electric Equipment (OÉA) where L A Artsimovich orchestrated the techniques of electromagnetic isotope separation for the manufacturing of fissile material for atomic bombs. Even the top secret reports used for some time such misleading terms as 'goo' (designating the plasma), 'altitude' (temperature), 'jet' (magnetic field), etc. As a result, the phrase 'high-temperature plasma in a magnetic field' was coded by the word combination 'high-altitude goo in a jet'. All participants of the project took pride in advancing to the magnificent goal: the generation of energy 'out of water' (the potential resources of energy inherent in deuterons in 1 litre of water is three hundred times greater than in 1 litre of petrol). We awaited with impatience any information on CNF work from other countries, first of all from Britain and the USA [2].

[2] The classified nature of the work made a dramatic imprint on the fates of some participants of the program. For instance, a very prominent theoretician B I Davydov was taken out of the MTR research in late 1951 and then fired in 1952 for his acquaintance with someone who formerly emigrated from the USSR.

Curiously enough, scientists in each of the first three countries starting their CNF research based on a closed toroidal system discovered their own approaches to magnetic plasma confinement.

Experiments with toroidal gas discharge in the UK created a field of 'toroidal pinches with reversed toroidal magnetic field', abbreviated to RFP (reversed field pinches). Currently, large machines of this type exist, one in Padova, Italy, and another in Boston, USA.

The proposal made by A D Sakharov and I E Tamm for a 'magnetic thermonuclear reactor' led to 'tokamak' systems, which grew to dominate the world program of CNF research.

L Spitzer invented the closed system of magnetic confinement with nested magnetic plasma surfaces in which each magnetic line of force extends along the system (the topological torus) while rotating by a certain angle ('rotational transform') and covers the whole closed toroidal surface; this approach generated the fundamental research field of steady-state 'stellarators' or 'helical' systems of magnetic plasma confinement. These systems suffered from delays in their progress owing to their greater complexity and unsuccessful experiments at the initial stage in their history. At the present moment they have gained a 'second life' alongside the traditional approach; the largest contemporary representative nowadays is the largest helical system, the LHD (Large Helical Device), in Japan. 'Advanced helical systems' are being developed as well; a 'live' representative is the large stellarator WVII-X that is being constructed in Greifswald (Germany).

The stellarator research was started in the Princeton Plasma Physics Laboratory; at present this laboratory is implementing a compact tokamak and an innovative 'quasi-symmetric' stellarator NCSX with a self-generated 'bootstrap current' (the current connected with a specific drift trajectory on the torus) which helps to improve the plasma parameters.

In addition to closed systems, scientists in the USA and the USSR arrived independently at the concept of open magnetic systems with magnetic mirrors (this is the American term) or magnetic plugs (the Russian term). At the present moment this field has mostly survived in the research towns of Tsukuba in Japan and of Novosibirsk in Russia.

3. CNF research: global status before Geneva-58

Until the 2nd Geneva Conference on Peaceful Uses of Atomic Energy (1958), all CNF research was strictly classified. What follows now is a brief chronology of the relevant significant events in the United Kingdom, the USA and the USSR.

Great Britain

1946. G P Thomson (see paper [1]) and M Blackman [2] patented a toroidal nuclear fusion reactor using deuterium. The stated power of the reactor $P_{\rm DD} = 9$ MW. The initial plasma heating was produced by a 500-kA alternating current.

1949. First experiments with toroidal discharges (P Thonemann (see Ref. [1]); implementation of the pinch effect at a current of $J = 27$ kA by S W Cousins and A A Ware [3]).

1955. The idea of stabilizing the discharge with a magnetic field (R J Bickerton) (see paper [1]).

April 1956. I V Kurchatov's lecture at Harwell.

1958. A sensation (British newspapers of 25 January; a publication in *Nature* accompanied with American papers [4]): plasma with an ion temperature $T_{\rm i} \cong 300$ eV (!) was obtained on the large toroidal machine ZETA (the radii of the plasma torus $a_{\rm p} = 0.5$ m, $R = 1.5$ m). This announcement proved to be an error. However, ten years later at the Third IAEA Conference (Novosibirsk, 1968) an announcement was made of the discovery on this machine of a self-organizing quiescent mode with the generation of a magnetic flux in the plasma, which exceeded the initial magnetic flux within the conducting housing: the magnetic flux outside the plasma was negative. Hence the term 'reversed field pinch' (RFP).

USA

1945–1946. Edward Teller's seminars on CNF. Negative results of experiments with beams (J Tuck, S Ulam) (see book [5]).

March 1951. The announcement by Argentina's President Juan Peron of the successful demonstration of controlled thermonuclear reaction by R Richter led L Spitzer to the invention of the stellarator as a solenoid shaped into a 3D figure of 8.

May 11, 1951. L Spitzer's proposals are discussed at the Atomic Energy Commission (AEC).

July 7, 1951. A contract for a research project is signed with Princeton University (the Matterhorn project).

All works on CNF (on pinches at Los Alamos, on the mirror trap at Livermore and others) are merged somewhat later into the single Sherwood project.

USSR

1950. O A Lavrent'ev wrote letters to Moscow with, among other things, a description of the idea of realizing controlled fusion of deuterium nuclei in an electrostatic field (sent from Sakhalin Island at the end of July).

1950. A D Sakharov responds to O A Lavrent'ev's proposal (August 18) with a remark that a 'highly reflective grid is required', 'having a thin current-carrying segment', in order to reflect almost all incident nuclei back into the reactor.

August–September 1950. The idea of creating high-temperature plasma directly in the magnetic field. A D Sakharov and I E Tamm work on the MTR theory.

October–December 1950. Higher echelons of the administration get acquainted with the idea of magnetic plasma confinement.

January–February 1951. A sequence of discussions and the preparation of the draft of the governmental resolution for the work on the magnetic thermonuclear reactor (MTR).

April 5, 1951. Stalin approved USSR Council of Ministers Order for the creation of a laboratory-scale model of the MTR [see below Goncharov G A *Phys. Usp.* **44** 851 (2001)].

Mid-April 1951. Discussions on the logistics and management of the MTR problem intensify in response to the information received from President Juan Peron's speech (March 25).

May 5, 1951. Stalin approved USSR Council of Ministers Resolution on the organization of MTR research (see below the Section "From the Archive of the President, Russian Federation").

1951–1955. Experimental and theoretical research into toroidal and linear discharges; short-lived innovations (such as RF plasma confinement, etc.).

1955. Pilot tokamak (still using a ceramic discharge chamber): torus with a magnetic field (Russ. abbr. TMP).

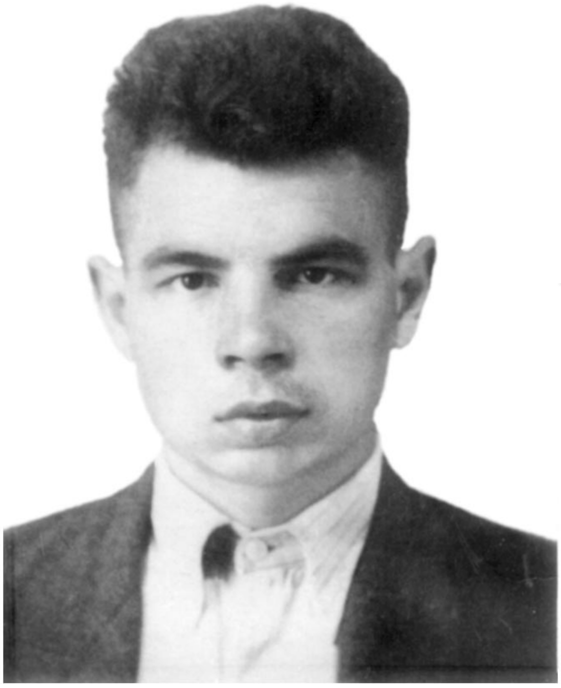
3.1. 50 years ago

May 2001 was thus the 50th anniversary of the official launching of CNF research in the USSR and the USA. The history of this research is both amusing and dramatic [3].

In the USSR, this story began with a letter written by a Red Army sergeant Oleg Lavrent'ev, who was posted at Sakhalin Island, to the Central Committee of the Communist Party of the Soviet Union (see below the Section "From the Archive of the President, Russian Federation").

The letter suggested creating the hydrogen bomb using the atomic bomb to prime it and even more interestingly, there was a suggestion for the electrostatic confinement of deuterium nuclei for industrial-scale generation of energy; two spherical grids placed under a negative and positive potentials were to be used for the purpose. The letter was sent for review to A D Sakharov who wrote that "the author formulates a very important and not necessarily hopeless problem". He mentioned a number of difficulties in realizing the electrostatic confinement and pointed out that the grid must have "wide meshes and a *thin current-carrying part which will have to reflect almost all incident nuclei back into the reactor* (italicized by V D Shafranov). In all likelihood, this requirement is incompatible with the mechanical strength of the device". However, the reviewer continued that "it cannot be excluded that certain changes in the project may correct this difficulty". At the end of his review, Sakharov emphasized that regardless of the results of further discussion "at this point, we must not overlook the creative initiative of the author". It is remarkable that the letter was sent from Sakhalin on July 29, 1950 and A D Sakharov's review was signed as soon as August 18, 1950. By that time O A Lavrent'ev, having passed examinations for the last three years of high school and demobilized from the army, had already enrolled in Moscow State University. Lavrent'ev's letter prompted A D Sakharov to the idea of the magnetic thermal insulation of high-temperature plasma. By October, A D Sakharov and I E Tamm completed the first evaluations of a magnetic thermonuclear reactor. In January 1951, the MTR project suggested by A D Sakharov and I E Tamm gained approval and O A Lavrent'ev, who stimulated A D Sakharov to the idea of 'magnetized' plasma, had obtained accelerated graduation from the university with the help of the authorities. Having graduated from Moscow State

[3] One of the first attempts at CNF research is mentioned in G A Gamow's book in connection with reminiscences of a meeting with one of the leaders of the country, N I Bukharin: "Nikolaĭ Bukharin is a veteran revolutionary and a close friend of the late Lenin; furthermore, he is the only one among the leading communists (with the exception of Lenin himself, of course) who was born into an old Russian family. I encountered him when his rank in the hierarchy was lowered and he occupied a relatively modest position as a Committee chairman [the Supreme Council of National Economy (VSNKh)]. His responsibilities covered monitoring the progress of Soviet science and technology; there can be no doubt that this position was of no political importance (Bukharin fell victim to Stalin's purges and was executed five years after I left Russia). He was once present at my lecture at the Academy of Sciences (which at that time was based in Leningrad) on thermonuclear reactions and their role as the energy source of the Sun and other stars. When the lecture ended, he suggested that I take the post of the head of project on developing controlled nuclear fusion reactions [this proposal was made in 1932 (!)]. I could have at my disposal, for several minutes one night every week, the entire electric power of the Moscow industrial region in order to send it through a very thick copper wire saturated with small 'bubbles' of lithium–hydrogen mixture. I declined the proposal and I am glad that I took this decision, because at that time it would definitely not have worked" [6].



Oleg Aleksandrovich Lavrent'ev

University, O A Lavrent'ev received an invitation to work, on L A Artsimovich's recommendation, at the Khar'kov Physico-Technical Institute, where he still works. The experiments he was running on electrostatic reflection of the electrons that escape from an axisymmetric 'anti-mirror' trap across an annular magnetic gap caused great interest at the Kurchatov Institute. A special trap which was toroidal (so that it could remove the loss of particles along the axis) got the acronym **ATOLL** [**A**nti-mirror **TO**roidal **L**avrent'ev trap (**L**ovushka in Russ. abbr.)] and was built and operated in the 1981 – 1985 by the outstanding experimental physicist M S Ioffe in order to study in detail the physical processes in the plasma of this trap with four annular gaps. The results of research with ATOLL were published in 1989 [Ioffe M S et al. "Plasma confinement in magnetoelectrostatic trap", in *Itogi Nauki i Tekhniki. Physics of Plasma* (Ed. V D Shafranov) Vol. 9 (Moscow: VINITI Publ., 1989) p. 5].

3.2 The idea of magnetic thermal insulation of the plasma [4]

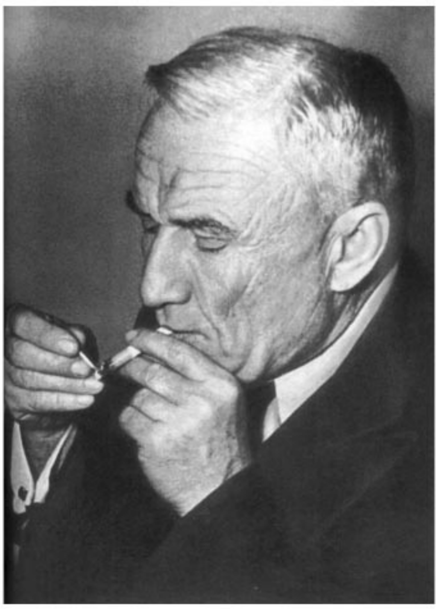
As A D Sakharov saw it, the main new feature of O A Lavrentiev's idea was the low density of the confined particles. However, he was not happy with the long ranges of particles, which would inevitably lead to undesirable interactions of high-energy particles with the construction materials. Would it be possible to arrange for the trajectories of freely moving particles not to leave a prescribed volume? It would! A charged particle in a strong magnetic field follows a helix along a magnetic line of force. Therefore, a high-temperature plasma must be created in a toroidal solenoid. If the curvature of the solenoid is neglected, particles will impact the chamber walls only as a result of interparticle collisions — that is, as a result of diffusion across the magnetic field. However, the trajectory of a particle can shift after each collision only by a distance on the order of the Larmor radius (about 1 cm for deuterium ions and less then 1 mm for electrons at $B = 50$ kG and plasma temperature $T_{\mathrm{p}} \sim 50$ keV). Therefore, the energy transfer to the construction elements of the reactor is greatly reduced.

A D Sakharov discussed this problem with I E Tamm who had just returned from his holidays. Even though very busy at that moment with the work on the hydrogen bomb, they started to discuss the physical problems arising in the new field and to evaluate the parameters of the magnetic thermonuclear reactor (the name was suggested by I E Tamm); the curvature of the plasma torus was neglected [5]. By the end of October 1950, the idea of the MTR was communicated to I V Kurchatov and his young deputy I N Golovin. On his short visits to LIPAN, A D Sakharov discussed a method of

[4] In this section I make use of the oral and written narratives of I N Golovin.

[5] A D Sakharov writes in a footnote to his paper "The theory of the magnetic thermonuclear reactor" [7] that a calculation for the large model was first done by I E Tamm in October 1950.

Andreĭ Dmitrievich Sakharov



Igor' Evgen'evich Tamm

Lyman Spitzer

eliminating the vertical drift of charged particles in the toroidal magnetic field relative to the torus plane.

First he suggested suspending a coil on the chamber axis, carrying a toroidal current whose magnetic field would convert magnetic lines of force into helical lines, thus creating a system of nested toroidal magnetic surfaces. Later he chose to generate such a current in the plasma itself by induction. To sustain the current-carrying plasma ring in equilibrium, he suggested a toroidal copper housing cut in two places: along the torus to allow the introduction of the toroidal magnetic field, and across the torus for the introduction of toroidal emf which would generate and sustain electric current in the plasma.

In 1957, this system was given the name 'tokamak'. In January 1951, I V Kurchatov organized a workshop on the MTR with the leading designers of atomic weapons and, having gained their support, began preparations for a governmental resolution on launching the MTR work. In February 1951, the project was forwarded to L P Beriya. By April, no final decision had been made on the project. "In mid-April, the Minister of the Electric Industry D V Efremov suddenly stormed into Kurchatov's study with a magazine in his hand, which reported successful experiments by someone called Richter in Argentina, who had detected neutrons in gas discharge" [8].

3.3 President Peron on Ronald Richter's 'success'

On March 25, 1951, Argentina's President Juan Peron made an announcement that experiments by a German physicist Ronald Richter succeeded in a "controlled release of atomic energy at a superhigh temperature of millions of degrees without using uranium fuel"; Richter was working in a specially created secret laboratory on the Hewmall island in Argentina. Having learned from D V Efremov about President Peron's announcement, I V Kurchatov immediately communicated with L P Beriya who urgently convened a panel to discuss organizational aspects on the subject and a draft of a governmental resolution submitted earlier. It was suggested that the CNF research program be headed by L A Artsimovich (who, without resigning from his job of launching a plant for electromagnetic separation of isotopes, was to spend one third of his time on the new problem of controlled nuclear fusion). M A Leontovich was to head the theoretical effort (on I E Tamm's recommendation) [6].

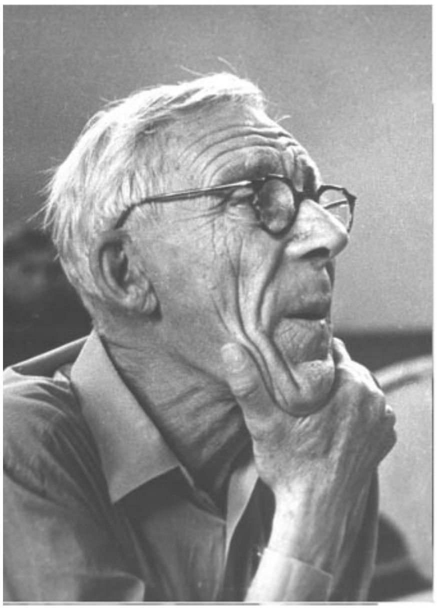
A detailed Resolution from the USSR Council of Ministers, which ordered heads of certain industrial plants to meet the demands of the CNF research team, was already signed by I V Stalin on May 5, 1951.

In October 1951, A D Sakharov and I E Tamm prepared projects for their research programs for CNF (published later in the collection of articles [7], pp. 3, 20, 31). The parameters of an 'optimal' MTR in Sakharov's calculations for a cylindrical model were: the major and minor radii of the plasma torus were $R = 12$ m and $a_{\rm p} = 2$ m, $B = 50$ kG, $n = 10^{14}$ cm^{-3}, $T = 100$ keV, and $P_{\rm DD} = 880\,000$ kW.

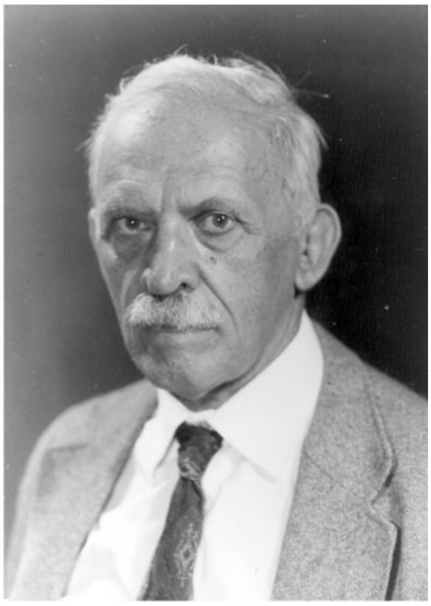
According to calculations, this plant could produce up to 100 g of tritium or 80 times that of ^{233}U per day. Sakharov noted at this point that the energy-production value of the ^{233}U which could be burned in a conventional reactor would greatly exceed the heat liberation in the nuclear fusion reactor itself. These remarks clarify that the decisive factor for enacting the decision on the CNF program at the time was the possibility of manufacturing charges for hydrogen or atomic bombs.

It must again be emphasized that the curvature of the torus was not taken into account in the reactor calculations. In fact, the toroidal current suggested by Sakharov to compensate for the toroidal drift of charged particles leads to crucial changes in the physics of toroidal plasma confinement. One of them is that the theory of transport phenomena must especially take into account the drift trajectories of charged particles in the presence of the azimuthal component of the magnetic field (the future 'neoclassics' of A A Galeev and R Z Sagdeev!) — this was already mentioned in I E Tamm's paper (see [7] p. 31).

[6] As legend has it, one of Beriya's assistants started to murmur into his ear that Leontovich was a security risk. Beriya replied with a pronounced Georgian accent: "Eef you kepp an aye on heem, he do no hemm" (if you keep an eye on him, he'll do no harm) .


Mikhail Aleksandrovich Leontovich


Lev Andreevich Artsimovich


Igor' Nikolaevich Golovin

3.4 The road from the idea to its implementation

An analysis of closed toroidal systems highlighted the problem of toroidal drift of charged particles. We mentioned above that A D Sakharov suggested two methods to close the drift trajectories of charged particles inside the chamber (the term used at the time was 'stabilization of toroidal drift'):

(1) To add a poloidal magnetic field created by an internal current ring suspended by cables or by a horizontal magnetic field;

(2) To induce a high-frequency current in the plasma itself. The second of these techniques was more realistic and led to experiments with a single-pulse discharge sent from capacitor batteries.

At the Kurchatov Institute, the need to inject toroidal current led to a proposal to forgo the toroidal magnetic field completely. The main effort was first concentrated on pinches in which, according to the Bennett relation $J^2 = 4c^2NT$ [9], the plasma temperature must grow in proportion to the square of the current, $T \sim J^2$! It seemed that this approach promised a fast resolution of the problem [7]. Only a small group headed by I N Golovin and N A Yavlinskiĭ continued to do research along the ideas of Sakharov and Tamm.

3.5 Rapid discharges

Experimenters in the meantime were 'storming' direct discharges but without visible success. It seemed that better vacuum conditions, a change in the scenario of preparing the discharge and so on should lead to success.

Finally, on July 4, 1952, counters started to detect neutrons in N V Filippov's group: the deuterium plasma pinch generated neutrons! Hope appeared that modifications of the experimental program might gradually increase the plasma temperature. However, L A Artsimovich's demand that everything be checked extremely thoroughly killed the euphoria. This was soon followed by profound disappointment: the pinch instability did not allow the temperature to rise with increasing current.

The pinch discharge program was later modified. Short-pulsed discharges formed far from the walls by virtue of a special chamber shape initiated a program of plasma focus study led by N V Filippov.

3.6 Toroidal discharge stabilized by a strong magnetic field

The theory of pinch stabilization by a longitudinal magnetic field again reoriented the studies towards A D Sakharov's suggestion: to use *both* the toroidal magnetic field *and* the toroidal current. However, their functions had, in a certain sense, changed: in the new system, the toroidal current provided equilibrium and plasma confinement, while the magnetic field served to create the discharge stability. However, there was still no hint of a plasma temperature increase. Both the first toroidal and cylindrical plants had ceramic chambers. Local overheating of a wall with low thermal conduction caused strong sputtering, plasma pollution and intense UV radiation. As a result, the plasma temperature remained low, at a level of 10 – 30 eV.

In 1955, the first tokamak-like machine was built — the TMP [8]. It still had a ceramic discharge chamber with a helical metallic insert. Silicon lines in the plasma radiation spectra were evidence of chamber wall evaporation caused by high thermal loads [10].

3.7 Period of pessimism: until the beginning of the 1970s

A temperature not exceeding 30 eV was typical for a long time. There was no progress either in pinches or in toroidal systems.

Searching for other scenarios of plasma confinement, Gersh I Budker came up with the idea of a direct axisymmetric magnetic system with enhanced magnetic fields at its ends (the 'probkotron'). Some time later I N Golovin, at the time the deputy director of the Kurchatov Institute, decided to focus on this relatively simple option and design OGRA, a large trap with magnetic plugs (mirrors). Physical studies were also started of low-density plasma confinement in small mirror traps. A number of very novel proposals were made, such as the confinement of hot plasma by an RF electromagnetic field, a collapsing plasma ring, etc., however, without appreciable success. Theoretical studies of stability on plasma models with a well-defined edge pointed unambiguously to the unavoidability of segments with convex magnetic lines of force, through which plasma could leak out of the confinement volume. A period of more than five years of profound pessimism towards the feasibility of solving the CNF problem was setting in.

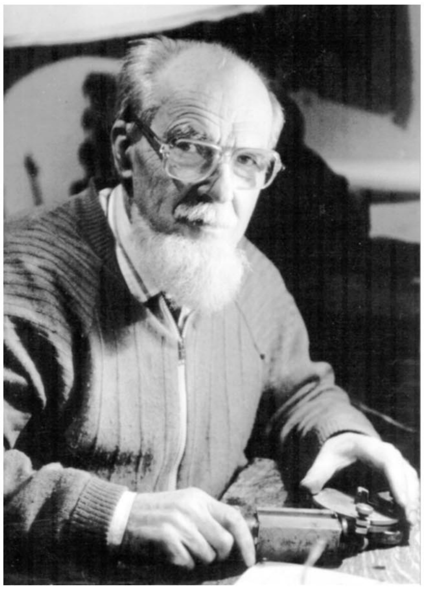
3.8 All-Union Conference of 1955

This was the situation in which I V Kurchatov decided that the CNF program needed declassifying. He started with organizing the All-Union conference in 1955 (classified even though with considerably wide attendance) to discuss the CNF work in his institute. Presentations were made by L A Artsimovich and M A Leontovich. The participants of the conference, who heard about the MTR problem for the first time, were amazed by the scale of the research goals (and by the work already conducted!) and committed themselves to the necessary support and continuation. An interest in CNF research was also implanted into other classified research establishments.

It is likely that another factor of similar importance for the CNF project was the presence at this conference of the young B B Kadomtsev. Inspired by this completely new field of research that promised interesting physics, he decided to leave Obninsk where he together with his superior D I Blokhintsev were working on weaponry problems, and soon joined our group. Ten years later he was one of the ablest theoreticians in the CNF field. In 1965, at the 2nd IAEA Conference in Culham, Great Britain, he was given the honor of summarizing the results of the reported theoretical papers at the

[7] **Brief digression.** I began working in the nuclear fusion field at just this time. In February 1952, two months after graduation from Moscow State University, I was sent to LIPAN, a classified research institute. M A Leontovich put me in his small group of theoreticians. Only my very first report (evaluation of high-frequency plasma heating in a strong magnetic field) was related to the Tamm – Sakharov MTR. I was able to read Tamm's and Sakharov's reports (extremely rich in content), as well as other early reports on MTR, only after they were declassified in 1958. The main subjects of research at the beginning were pinches. In my first year I completed some work with M A Leontovich on the stabilization of helical perturbations by longitudinal magnetic field on a model of linear straight plasma filament with a surface current. S I Braginskiĭ who was working on his famous transport equations in magnetized plasma had applied them first of all to pinch discharges without a longitudinal magnetic field. Together we extended these studies to pinches with a longitudinal magnetic field.

[8] The term 'tokamak' was not yet in use; it found its way into the language after 1958.



Nikolaĭ Vasil'evich Filippov

Nutan Aronovich Yavlinskiĭ

Stanislav Iosifovich Braginskiĭ

concluding session. The experimental work was summarized at this conference by L Spitzer [9].

3.9 First steps to international cooperation on CNF

Soon after the All-Union meeting of 1955, I V Kurchatov made another step towards declassifying the experiments on pinches. In April the following year, I V Kurchatov, as a member of a Soviet governmental delegation headed by N S Khrushchev, arrived in Great Britain and gave a lecture "On the feasibility of the thermonuclear reaction in gas discharge" at the atomic research center at Harwell. This was the first real step towards international cooperation in CNF. At subsequent international conferences on plasma physics, the physicists involved in the not yet declassified CNF research were able to recognize their colleagues from the contents of the delivered talks.

In April 1956, the first delegation from abroad visited the Kurchatov Institute. These were members of the Swedish Academy of Sciences. As a response gesture, H Alfven invited L A Artsimovich and I N Golovin to Stockholm in autumn 1956 to take part in an astrophysics conference. L A Artsimovich and I N Golovin gave talks that were implicitly connected to the pinches and the tokamak research program. Here they met L Spitzer and R Pease who led the British CNF program.

In June 1957, a large number of papers related to CNF were presented to the conference on "Ionization phenomena in gases" in Venice (however, the problem was not explicitly mentioned). The papers were presented by R Bickerton, L Bierman, M Rosenbluth, V D Shafranov, J Tuck and other participants of the CNF research. At this point only the work by S A Colgate on neutrons in pinches was openly connected with CNF. In reality, however, this was the first international conference with a large number of reports related to the CNF field.

[9] We also note that the experimental summary at the First IAEA Conference in Salzburg in 1962 was made by L A Artsimovich, and the theoretical part was summarized by the outstanding American theoretician M Rosenbluth.

3.10 The ZETA facility and the boom it caused

The astrophysics conference of 1956 in Stockholm and the 1957 conference in Venice presented no breakthroughs. Unexpectedly, British papers announced in January 1958 the sensational news that a temperature of 300 eV had been achieved at Harwell on the ZETA facility.

The task of figuring out what ZETA was really like fell to S I Braginskiĭ and myself. We knew from the newspapers that the facility looked as a spherical one. This meant for us that it was a compact (low aspect ratio) toroidal system. We also knew that compactness was required to stabilize the plasma with a strong magnetic field (in tokamak-type systems). Another possibility was stabilization by a weak magnetic field trapped into the pinch where the plasma was compressed, with the discharge chamber having conducting walls (the future reversed field pinch configuration). However, this possibility did not require compactness. Furthermore, we did not quite believe in the possibility of confining the trapped ('frozen-in') toroidal magnetic field for a sufficiently long time. In view of these arguments, we concluded that ZETA was a tokamak-type system.

The January issue of *Nature* soon arrived with the results of experiments on the ZETA facility (also presenting some results obtained by American authors). Our conjecture was obviously wrong. However, our analysis of toroidal systems (even though idealized in certain respects) helped the progress of a project prepared by N A Yavlinskiĭ for a relatively large tokamak, T-3 (I N Golovin was busy at the time with the large open trap OGRA). The ZETA results (which proved to be erroneous) were the last intriguing story before the 2nd Conference on Peaceful Uses of Atomic Energy in Geneva. This conference gave a start to broad-scale international cooperation.

4. CNF before and after the Geneva Conference of 1958

The Kurchatov Institute's papers on CNF were declassified before the start of the 2nd Geneva Conference (1958) and published in the collections *Plasma Physics and Problems of*

Controlled Thermonuclear Reactions (4 green volumes edited by M A Leontovich).

The conference in September 1958 presented numerous approaches to plasma confinement (L A Artsimovich called it 'a fair of ideas'). The 'star' of the program at the Geneva conference was L Spitzer's stellarator. This was indeed the concept of a stationary magnetic system for plasma confinement — the ideal for CNF (!).

The stellarator affected our research too:

(1) Realizing how important L Spitzer's proposal was, I V Kurchatov prodded N A Yavlinskiĭ to change to the stellarator direction instead of continuing the construction of the new tokamak (this was exactly the T-3 tokamak). N A Yavlinskiĭ asked S I Braginskiĭ and myself to compare this tokamak (this term was not yet in use and here we resort to it for brevity only) with the stellarator. We gave roughly the following arguments in favor of the tokamak. The minor chamber radius is greater in the tokamak than in a stellarator of equal chamber length; hence the walls have a smaller effect on the discharge. Further, if heating is achieved by current only (no other methods were available at the time) the advantage lies with the systems with a higher current. This approach helped the tokamak line to survive at the time.

(2) The sword of Damocles of the CNF was the enhanced Bohm diffusion which seemed universal and which was detected both on the 'figure of 8' stellarator and later on the combined 2- and 3-thread stellarator C (with a race track shape). This diffusion caused depression among the researchers. However, theoreticians who tried to decipher the mechanism of this diffusion and 'sifted' a plethora of potential instabilities were able to develop the theory of turbulence and thereby facilitated the progress in plasma physics.

(3) The tokamak – stellarator competition intensified the work on CNF.

5. Pessimism defeated

Positive tendencies started to appear in the CNF research in the 1960s. Hope was born that the plasma behavior could be controlled.

1961. Experiments by M S Ioffe on plasma stabilization in open traps were reported at the 1st IAEA Conference in Salzburg (Austria). B B Kadomtsev's explanation of Ioffe's experiments showed that there was no Bohm diffusion (!). The consequence for toroidal systems: the medium magnetic well, $(\langle B^2\rangle + 2\mu_0 p)' > 0$, in a closed system with variable curvature of the lines of force of the magnetic field is the real stabilizing factor [10].

1962. First success of tokamaks is announced: a correction to the plasma position by a vertical magnetic field resulted in improved plasma parameters.

1965. The 2nd IAEA Conference holds in Culham (UK). To quote from L A Artsimovich's report: "The confinement time in our experiments is almost 10 times the Bohm limit". L Spitzer, in his review of experimental work, concluded that this coefficient is not yet a proof of the absence of Bohm diffusion.

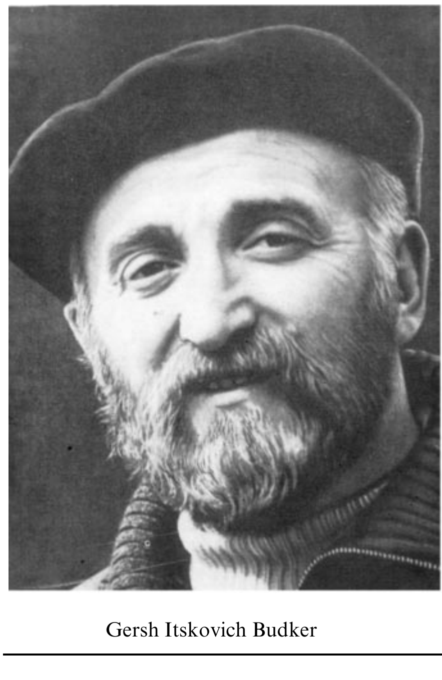
1968. The 3rd IAEA Conference was convened in Novosibirsk. It was reported that the mean electron temperature $\langle T_e\rangle$ on the T-3 tokamak approached 1 keV, and that Bohm diffusion was definitely absent (!). Not everybody believed this, though. Then L A Artsimovich invited the Culham physicists with their laser diagnostics (five tons of equipment) to measure the local electron temperature by the Thomson scattering technique.

1969. The 2nd workshop on toroidal systems holds in Dubna. D Robinson reported on the local measurements of the electron temperature T_e on the T-3 tokamak. This was the triumph of the tokamaks!

1970. The 'tokamak' direction of research became international. Other lines of attack on CNF were being closed in a number of American and European laboratories.

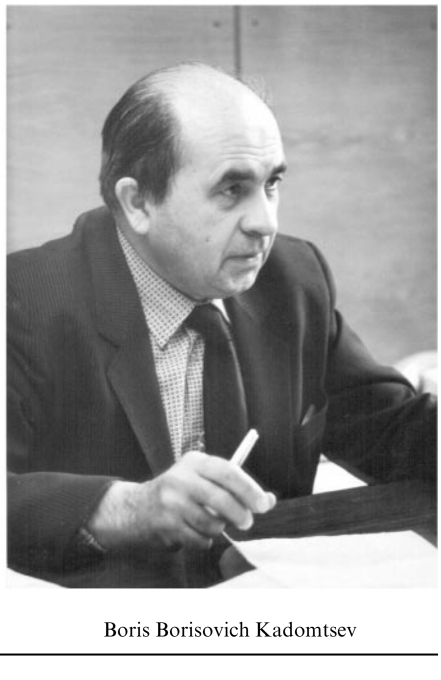
1975. The T-10 tokamak was launched at the Kurchatov Institute and the PLT machine at Princeton (USA); three years later a plasma at a temperature of about 8 keV was reported on the PLT.

[10] By the petitioning of the American physicists, M S Ioffe received the Ford Prize. This had a dramatic outcome. On instructions 'from above', M S Ioffe had to refuse to accept the prize, which inevitably soured his relations with his colleagues abroad.



Gersh Itskovich Budker

Mikhail Solomonovich Ioffe



Boris Borisovich Kadomtsev

Boris Borisovich Kadomtsev and Andreĭ Dmitrievich Sakharov in the hall of the T-15 Tokamak in the I V Kurchatov Institute of Atomic Energy, 1987 (photograph by Yu E Makarov).

These tokamaks are now being superseded by still larger, prereactor-scale machines. At the present moment, work is being completed on the international project of an experimental reactor – tokamak, ITER-FEAT.

The main results of the first period in CNF history in the Kurchatov Institute:

(1) The possibility of confining a high-temperature plasma has been proved.

(2) The tokamak as a sufficiently simple system with ohmic heating became a leader that opened the way to CNF in other types of system, first of all in stationary stellarator-type systems.

The current status of CNF research in the Institute of Nuclear Fusion at the Russian Research Centre 'Kurchatov Institute' (Director V P Smirnov, Corresponding Member of the Russian Academy of Sciences):

(1) The T-10 tokamak is operating (the main field of research is the so-called transport barriers) [11].

(2) A project for the T-15M tokamak is at the design stage, to model ITER-FEAT as a mobile assistant in choosing the work regimes, clarifying unexpected phenomena in the fusion plasma, etc.

(3) The pinch program is being revamped.

(4) Certain potentially interesting directions for neutron-poor nuclear fuel $D - {}^3He$ are being investigated.

(5) Multifaceted theoretical research on the key issues of plasma physics and CNF are being continued.

6. Conclusions

The first 20 years in the history of research into controlled nuclear fusion were years of uncertainty. The subsequent five years removed this uncertainty, and the icebreaker that cleared the way to other magnetic systems of plasma confinement was the tokamak. There is no doubt any more in the feasibility of a controlled thermonuclear reaction in a plasma confined by a magnetic field. Problems of technology and materials science have come to the fore (and are being solved).

CNF studies are a viable branch of the modern science. They have contributed greatly to fundamental scientific progress.

Nonlinear phenomena in continuous media, including 2- and 3-dimensional solitons, the processes of stochastization and formation of structures (self-organization) are the examples of growing fields that have been greatly stimulated by high-temperature plasma physics.

CNF studies stimulated the progress in vacuum technology applied to large volumes, the technology of superconducting magnetic systems, the creation of high-power microwave generators, etc.

The viability of the problem of controlled nuclear fusion is an objective guarantee of its further progress.

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