Physics – Uspekhi **40** (8) 859 – 867 (1997)

LETTERS TO THE EDITORS PACS number: **01.65.+g**

On the history of creation of the Soviet hydrogen bomb

G A Goncharov

In 1996, *Uspekhi Fizicheskikh Nauk* and *Physics Today* carried my articles on the history of the hydrogen bomb programs in the Soviet Union and the United States [1]. They continued a series of publications on the early history of thermonuclear research and development in the USSR. When I wrote my articles [1], I drew not only upon domestic documentary sources and foreign publications but also on intelligence reports the Soviet Union received early in its atomic project. However, my analysis in [1] of the role that intelligence reports played in the inception and progress of the Soviet thermonuclear project elicited a negative response from V B Adamskiĭ and Yu N Smirnov who sent a letter to the Editor of *Uspekhi Fizicheskikh Nauk* which is published in the present issue [6].

This letter is my answer to the criticism Adamskiĭ and Smirnov level in [6] against my articles [1]. I shall overlook quoting my unpublished progress report of 1994, which was substantially revised in [1].

Above all, Adamskiĭ and Smirnov question my statement in [1] that 'the first inquiry in the Soviet Union into the possible use of the nuclear energy of the light elements was stimulated by intelligence reports on US superbomb activities, which began to arrive in 1945' (which my opponents stretch to mean that 'in their work on thermonuclear weapons in 1945 – 1946, Soviet scientists were 'stimulated by intelligence reports'). To prove that I am wrong, they refer to I I Gurevich's words S S Gershtein quoted in *Uspekhi Fizicheskikh Nauk* in 1991 [7]. The reference was to the commentary on the report, *Utilization of the Nuclear Energy of the Light Elements*, submitted by Gurevich, Ya B Zel'dovich, I Ya Pomeranchuk, and Yu B Khariton, which Gurevich made because of A D Sakharov's suspicion that the work was 'a solid piece of plagiary.' According to [7], Gurevich said 'they knew of no one who might be concerned with the matter. Simply, deuteron and reactions involving light nuclei were within his and Pomeranchuk's scope of interest as the source of stellar energy and as a way to glean information about nuclear forces. In a joint discussion, Zel'dovich and Khariton remarked that nuclear fusion could be effected under terrestrial conditions by heating deuterium in the shock wave initiated by an atomic explosion (emphasizing that this process would offer a way to explode an unlimited quantity of a light element). Hence came their joint proposal which they submitted to I V Kurchatov.' Gurevich showed Gershtein an attested Xerox copy of the proposal which survived in the Institute of Atomic Energy's archives. The "proposal was type-written on seven pages with equations hand-inserted by Gurevich and marked '1946' by Kurchatov at the bottom of the last page (IAE Archives. 2-1-368, 1946)." 'This is tangible evidence that we did not know anything about US work,' Gurevich said pointing at the title page of the paper. 'You must realize how the proposal would have been classified and how many seals it would have carried otherwise.' "About the same we can read in Khariton, Adamskiĭ, and Smirnov [5]: 'The point is that in 1946 Gurevich, Zel'dovich, Pomeranchuk, and Khariton submitted a proposal to Kurchatov in the form of an unclassified report. Clearly, if the report had been drawn up using intelligence data, it would have automatically been classified as top secret.' 'The report of the four authors was type-written as an unclassified document, has never been classified, and is kept until now in the open funds of the Kurchatov Institute's archives.'

What do official documents tell us? They tell that the December 17, 1945 session of the Special Committee's Technical Council heard Zel'dovich's paper, 'On the Excitation of Reactions in Light Nuclei,' in the presence of Gurevich, Pomeranchuk, and Khariton (the latter attending the session not only as one of the authors, but also as a member of the Technical Council). The paper was based on the report, *Utilization of the Nuclear Energy of the Light Elements*, drawn up for the session by Gurevich, Zel'dovich, Pomeranchuk, and Khariton. A copy of the report was attached to every copy of the official Minutes No. 12 of the session. The copies were identical to the article of the four authors published in *Uspekhi Fizicheskikh Nauk* in 1991 [8], except for the date: the official copies of the report did not carry the date '1946' given in [8]. And they could not carry it because the true date of the report was 1945. It remains to add that the official copies of Gurevich, Zel'dovich, Pomeranchuk, and Khariton's report were classified as "Top Secret. Special Dossier" — the highest classification category used at the time. Moreover, the copy of the report kept in the Russian Federation Atomic Energy Ministry's archives carries an additional stamp, "Store on par with ciphers." Therefore, Adamskiĭ and Smirnov's argument is in my favor and not against me.

G A Goncharov All-Russian Research Institute of Experimental Physics
607190 Sarov, Nizhniĭ Novgorod Oblast, Russia
Tel.: (8-31-30) 45-778
Fax: (8-31-30) 42-729
E-mail: gagonch@vniief.ru

Received July 15, 1997
Uspekhi Fizicheskikh Nauk **167** (8) 903 – 912 (1997)
Translated by B V Kuznetsov; edited by S N Gorin

Most probably, the copy of the four authors' report at the Kurchatov Institute was the one Kurchatov kept for himself and had not originally been registered officially. This seems to be borne out by the date '1946' apparently added by mistake at a later time. Because of this, the copy of the report kept at the Kurchatov Institute was not automatically classified when Kurchatov turned a copy over to the First Chief Directorate for presentation to the Special Committee.

Of course, when I say in [1] that the USSR's first inquiry into the possible use of the nuclear energy of light elements was stimulated at the end of 1945 by intelligence reports, I do so not because the official copies of the report [8] had the highest security classification, but on the strength of an analysis of the situation and the contents of specific documentary evidence. Of course, Soviet scientists took an interest in and addressed the use of the nuclear energy of light elements not because intelligence reports had prompted them to do so. Relevantly, it is pointed out in [9] that 'As early as 1932 [that is, well before the advent of the atomic bomb — Author] there were suggestions by Russian scientists and others that thermonuclear reactions might release enormous amounts of energy.' In [1], I mention a memo Ya I Frenkel sent to Kurchatov, noting that an atomic explosion would possibly be able to excite nuclear reactions in a medium of light elements. In 1946, he even published his views on the use of the nuclear energy of light elements in the journal *Priroda* [10].

However, before he received Frenkel's letter, Kurchatov had got hold of intelligence reports on US work along that very line. Similar reports kept on arriving after this. The information they carried was serious indeed. It was not 'a minor detail' which I treated 'as a stimulating factor.' I cannot agree with my opponents when they say that the year 1945, when the Soviet Union first inquired into the problem of explosive nuclear fusion, was one of acute confrontation between the United States and the Soviet Union. That was the year of the common victory of the Allies in World War II. Churchill had not yet made his speech in Foulton, and the cold war started at a later time.

Nor can I agree with my opponents' assertion that the situation in 1945 was such that nuclear physicists in both countries were urged on toward thermonuclear research. It was in 1945 that the Soviet Union was faced with the fact that the United States was the sole possessor of the atomic weapon and used it to bomb two Japanese cities on August 6 and 9. In August 1945, the Soviet political leaders made the decision to step up the effort to build an atomic industry and an atomic bomb. At their sessions, the Special Committee and its Technical Council, both set up by the USSR Defense Committee's Resolution dated August 20, 1945, regularly considered the most fundamental issues of the Soviet atomic project and intelligence reports.

In such a situation, in the difficult period at the beginning of work on building an atomic industry and an atomic bomb, a serious cause was necessary to induce Kurchatov to raise the question of the explosive use of the nuclear energy of the light elements and for the Technical Council to discuss it at its session. Undoubtedly, the arrival of repeated intelligence reports on US superbomb work brought Kurchatov to instruct Khariton to draw up a report jointly with Gurevich, Zel'dovich, and Pomeranchuk [8]. The public statement of M. Oliphant in Britain in October 1945 on the possibility of building a superbomb (which I mention in [1]) only supplemented the intelligence reports. This information could not but worry the directors of the Soviet atomic project: the Soviet Union had a hard and long way to go to the atomic bomb, while US scientists were already exploring the possibility of building a superbomb.

In March 1945, the Soviet intelligence service received a report that Teller was working on a superbomb at Los Alamos. The subsequent messages reported that work was under way at Los Alamos on a superbomb whose yield could be brought up to an equivalent of 1 million tons of TNT and that the superbomb was to operate on a principle whereby, using a small quantity of uranium-235 or plutonium-239 as the primary source, a chain reaction could be initiated in the less scarce deuterium. It was stressed that only theoretical proposals had been made for this weapon and that there were hopes that the superbomb would be a success but poor prospects. Nevertheless, work on the hydrogen bomb was to be carried on until, at least, its unfeasibility was proved.

In September 1945, the Soviet intelligence service got hold of a synopsis of Fermi's lectures on the superbomb (according to [9], he delivered it to the staff of the Los Alamos laboratory in 1945). The synopsis not only contained important concrete data on the unique properties of tritium, unknown in the Soviet Union, but also (as follows from [11]) an outline of the theoretical views which the Los Alamos scientists held in 1945 and which served as the basis of the 'classical Super' project. The crucial point of the lectures was the hypothesized possibility of achieving nonequilibrium combustion for deuterium. The synopsis also contained equations for the radiation energy losses by including the inverse Compton effect, a key physical effect as a result of which, as was learned much later, both the classical Super and its Soviet equivalent, the Truba, failed to work as expected. Various approaches were examined in the lectures for the initiation of nuclear reactions in a deuterium-filled cylinder (it was noted, however, that all the schemes proposed to date for the initiation of a superbomb were rather vague). In one scheme, a jet of fast deuterons was to be injected into the initial part of the cylinder. The jet could be produced by the explosion of a shaped atomic bomb or by the Compton collisions of photons issuing from an atomic bomb. According to the synopsis, the preferable scheme was one where nuclear reactions in a deuterium-filled cylinder would be initiated by a stream of neutrons issuing from an atomic bomb (by way of an intermediate chamber filled with a mixture of deuterium and tritium).

There is documented reason to think that the synopsis of Fermi's lectures was passed on to the Soviets by Klaus Fuchs on September 19, 1945 via Harry Gold whom Fuchs met for the last time in Santa Fe. By that time, Fuchs already had the reputation of a reliable source of information on the US atomic project. Kurchatov could not pass Fuchs' information on the Superbomb unnoticed.

It is beyond doubt, however, that in discharging Kurchatov's assignment, Gurevich, Zel'dovich, Pomeranchuk, and Khariton considered the problem of the explosive release of the nuclear energy of the light elements and drew up their report [8] without direct resort to intelligence data. (Moreover, by no means all of the authors of [8], as Gurevich indicates, have even been informed on the existence of such materials.) Comparison of their report with Fermi's lectures leads one to conclude that the report [8] suggested original approaches, proposals, and views different from Fermi's, although both had points in common. In both, it was assumed that the deuterium charge would be a cylinder, that use would be made of the nonequilibrium combustion of

deuterium, and that the shaped-charge (cumulative) effect could serve for initiation purposes. As to the differences, Fermi's lectures had in view the use of liquid deuterium of normal density, whereas the report [8] stressed that it would be desirable to have deuterium of the highest possible density which could be attained by using it under high pressure.

There were also important differences of a conceptual character. According to the data presented in [11], which are in agreement with Fermi's lectures, the energy released by the nuclear combustion of deuterium in the classical Super would be primarily transferred, as US scientists believed, in the collisions of the neutrons generated in the D + D and D + T reactions with deuterium nuclei. In the report [8], on the other hand, it was hypothesized that nuclear combustion in a deuterium-filled cylinder would proceed by way of detonation, that is, the propagation of a shock wave through the deuterium. It was further hypothesized that the nuclear combustion of deuterium could be initiated by the shock wave of an atomic explosion. The report [8] stressed that the problem of initiation would be a hard one to solve. It was noted that 'the initiation conditions could be improved by using uranium charges of increased size and special shape (the shaped-charge effect) and by placing some heavy elements in the deuterium near the initiator which would receive the radiation impulse.' As the report went on to say, 'it would be useful to employ massive burst-confining shells in order that initiation nuclear detonation could occur.' It was stressed that '[our] judgment on the possible use of an explosive nuclear reaction stems from the application of the present-day theory of detonation developed at the Institute of Chemical Physics.'

In my opinion, it is an unquestionable fact that Soviet scientists were original and creative in their approach from the outset of research on the explosive release of the nuclear energy of light elements, but it does not at all imply that work along this line in the Soviet Union was initiated by Soviet scientists. Statements on this issue need to be especially accurate, since otherwise a distorted picture of the actual stand and policy of the Soviet Union might result.

The Soviet Union launched its thermonuclear project as an answer to the on-going effort that the United States started earlier to develop its superbomb. In this connection, I would like to quote a passage from Sakharov's *Memoirs* [12]: 'If I am right in my guess about the spy origin of the version of the thermonuclear weapon on which Zel'dovich, Kompaneets and others worked in 1940s – 50s, then this fortifies Oppenheimer's stand in the fundamental plane' (as Sakharov noted earlier, Oppenheimer 'tried to hold back the US H-bomb program; he believed, that the USSR would not then press on with its own thermonuclear superweapon. Teller opposed his view.'). 'Actually, it turns out that the Americans started the entire 'chain' and if it were not for them, the Soviet Union would have never started its effort on thermonuclear weapons or would have done so a good deal later... . But in the situation that arose in the course of the Teller – Oppenheimer dispute it would be too late to argue who was the first. The events had gone out of control. Neither the USSR nor the USA could stop... .'

As Sakharov notes, Oppenheimer first came up against the US H-bomb program in 1948, that is, at about the same time as I E Tamm's group started their calculations on the H-bomb. In that year, the USSR H-bomb activities took on an irrepressible character (the process became finally irrepressible after US President Truman issued a proclamation on January 31, 1950 directing the US Atomic Energy Commission to 'continue its work on all forms of atomic weapons, including the so-called hydrogen or superbomb,' and the USSR Council of Ministers adopted, on February 26, 1950, a resolution, in response to Truman's directive, to create a Soviet H-bomb).

The change in Soviet H-bomb research in 1948 was related to the arrival of new intelligence reports. As I note in [1], on March 13, 1948, Fuchs passed materials on to the Soviet Union through the Soviet intelligence officer in London, A S Feklisov, which included a description and elements of a theoretical validation of the classical Super design with a two-stage initiation system operating on the radiation implosion principle. This was an event that played 'an extraordinary role in the development of the nuclear program in the USSR and had a considerable impact on the organization of future activities' Adamskiĭ and Smirnov [6] disagree with my assessment of that event. What grounds did I have to do so?

As I note in [1], the Soviet political leaders viewed the new material on the superbomb and improved designs of atomic bombs (also passed on by Fuchs at the same time) turned over by Fuchs as evidence of the possible US lead in their development, and required that emergency measures be taken to step up research on similar H-bombs in the Soviet Union with official state support. On April 23, 1948, Beria instructed Vannikov, Kurchatov, and Khariton to carefully analyze the materials and present with minimum delay an assessment of their practical value and concrete proposals on the following matters: which research, design and engineering tasks should be assigned to whom and what period, in view of the new data contained in materials 'a' and 'b', would be required for the design of an atomic superbomb and new types of atomic bombs; to whom and for what period the task of verifying (by methods within our capabilities) the data received should be assigned; and which corrections should be made in the R&D plan for 1948 (in the sense of speeding it up) in connection with the new data. Note that Beria had in view the R&D plan adopted earlier by a resolution of the USSR Council of Ministers. Research on the H-bomb had never previously been given such a high status.

On May 5, 1948, Khariton submitted an assessment of Fuchs' materials 'a' and 'b,' stating that they contained some rather interesting and previously unknown data which might accelerate the achievement of certain practical tasks. Material 'a' related to a superbomb in which the active substance would be deuterium and the detonator would be uranium-235. The material described the main components of the superbomb and included a sketch giving an idea of size of some important parts. It described the entire initiation train: first, uranium-235, then a mixture of deuterium and 50% tritium, next a mixture of deuterium and 4% tritium, and finally deuterium. There were *a number of not fully clear, but physically important remarks* [here and elsewhere the italics are supplied by the author] pertaining to the initiation mechanism, such as *a radiation-transparent filler and its opaque shell*, and the transfer of the reaction from the 50% deuterium detonator [this refers to the secondary unit using a liquid deuterium – tritium mixture] to the intermediate 4%-tritium detonator by neutrons. From an examination of both old and the latest materials, the impression was that after a long search of theoretical and experimental character, the basic design had at last tentatively been found... . It would be expedient to get down to a conceptual design of the superbomb... .

On the same day, May 5, 1948, Vannikov and Kurchatov submitted their assessment of Fuchs' new materials. They noted that the *fundamental remarks* in material 'a' on the role of tritium as well as particles and photons in the transfer of an explosion from uranium-235 to deuterium were new. The materials would be valuable in that respect and should help Zel'dovich in the work he was doing on the superbomb under the plan approved by the First Chief Directorate. As Vannikov and Kurchatov noted, more effort should be put into research in that field and work on the structural design should be started. They proposed a plan of theoretical research that should be completed by January 1, 1949 in cooperation with the USSR Academy of Sciences' Institute of Mathematics. A crucial point in their assessment was the proposal that the USSR Academy of Sciences' Institute of Physics should take part in the study of reactions involving deuterium and tritium and in tackling 'the most urgent theoretical aspects of the superbomb.' They also proposed that an engineering group should be set up at the KB-11 to work on the design of a deuterium superbomb and that a conceptual design should be completed by January 1, 1949.

The proposals submitted by Vannikov, Kurchatov, and Khariton were examined and approved by the Special Committee at its June 5, 1948 session and taken as the basis for the resolutions passed by the USSR Council of Ministers on June 10, 1948.

To believe Rhodes [13], US President Truman had never heard of the H-bomb until October 1949. Almost eighteen months earlier, thanks to Fuchs, Stalin, as head of the Soviet Government, approved the USSR Council of Ministers' resolution directing (in the sections pertaining to the H-bomb) verification, both theoretically and experimentally of the feasibility of the H-bomb and, in particular, setting up (in 48 hours) a theoretical group under Tamm at the USSR Academy of Sciences' Institute of Physics to carry out research on the theory of combustion of deuterium to specifications from Laboratory No. 2 (Khariton and Zel'dovich).

The fact that a new, specialized theoretical group, which included top-notch scientists, was drawn into work on the superbomb signified a radical change in the management of activities and was an objective factor that had a strong bearing on the progress of Soviet H-bomb research. To demonstrate, as early as 1948, Sakharov and Ginzburg of Tamm's theoretical group conceived their original ideas of the Sloika (Layer Cake) configuration and the use of a new thermonuclear fuel, lithium-6 deuteride. These ideas were the basis for a new line of work on the thermonuclear weapon in the Soviet Union. In January 1949, Sakharov suggested 'the use of an additional plutonium charge to initially compress the Layer Cake' — the prototype of a two-stage thermonuclear charge. These ideas enabled Soviet scientists to steer their own course, distinct from that chosen in the United States, toward the goal. Owing to this course, the Soviet Union was able to create its own thermonuclear weapon in a shorter time than did the United States (as reckoned from the start of research in each country) and to score the impressive success I describe in detail in [1]. So I had in mind the foregoing when I gave my assessment of the March 13, 1948 event. Can any one refute its extremely important role?

Nevertheless, Adamskiĭ and Smirnov [6] find it fit to ask: "How can one possibly insist that Fuchs' information played 'an extraordinary role in the development of the nuclear program in the USSR and had a considerable impact on the organization of future activities'? For nothing of the kind happened in either the United States or Britain where the specialists would obviously have had the originals of the materials that had found their way into the Soviet Union." On the latter point, my opponents are right: Fuchs' materials did not provoke governmental decisions either in the United States or Britain. But in the Soviet Union their exceptional role showed up above all in that they initiated special governmental decisions whose execution proved so fruitful in the technical plane. By passing new materials on to the Soviet intelligence service, Fuchs made the directors of the Soviet atomic project give them the attention they (or, more accurately, the data pertaining to the superbomb) never enjoyed in either the United States or Britain at the time.

Let us now take a look at the documentary evidence that throws light on the role that the concrete physical information and ideas contained in Fuchs' 1948 materials played in the effort of Soviet scientists to develop a two-stage thermonuclear charge. From Khariton, Vannikov and Kurchatov's assessment of Fuchs' 1948 materials quoted above it is clear that the three were aware of the novelty, physical implications, and fundamental character of the radiation-implosion initiation scheme for the classical Super presented. But they have not understood, at the time, the physical essence of the initiation mechanism and the significance of the key elements of the initiation system responsible for the radiation implosion. This could not but tell on the manner in which the concrete physical aspects of Fuchs' materials could affect the work of Soviet scientists.

In the list of points on which they disagree with me, Adamskiĭ and Smirnov [6] include the assertion that 'Klaus Fuchs' information was allegedly crucial for Soviet physicists' work on a two-stage thermonuclear charge where the main assembly would be compressed by the radiation of an atomic explosion (the radiation implosion principle).' I do not find such an assertion in my articles [1], but I have to note that Zel'dovich and Sakharov connected the commencement of work on the principle of a Soviet analog of the Teller – Ulam configuration (which uses radiation implosion) with research on a two-stage initiator for the Tube, functionally similar to the two-stage initiator mentioned in Fuchs' materials.

For confirmation, let us turn to the most authoritative source — the report of June 25, 1955 on the choice of design and the theoretical substantiation of the RDS-37, the first Soviet two-stage thermonuclear charge [1]. The introduction to the report, written by Zel'dovich and Sakharov, clearly states that the new underlying principle of the RDS-37 charge was developed in the theoretical sectors starting from 1950. That is, Zel'dovich and Sakharov dated the start of work on the principle of the Soviet two-stage H-bomb using radiation implosion back to 1950. According to the documentary evidence available, the only two-stage scheme considered in 1950 was the initiator scheme for the Tube configuration.

On February 10, 1950, or 5 days after the Special Committee had passed its resolution, *On Measures to Develop the RDS-6*, which was a response to US President Truman's proclamation of January 31, 1950, Zel'dovich wrote a report, *The Hydrogen Deuterium Bomb*. In discussing a deuterium H-bomb design whose initiating compartment would use a secondary unit of a deuterium–tritium mixture, Zel'dovich presented a scheme similar in structural features and physical essence to Fuchs' scheme from his 1948 report, but preference was given to another scheme, where a primary gun-type atomic bomb was to be encased in a shell of

a heavy material. He referred to Fuchs' scheme as a more elaborate alternative. In describing the operating principle of the initiating compartment in the Fuchs scheme, Zel'dovich noted that the deuterium – tritium mixture in the secondary unit would be heated by the explosive energy of an atomic bomb, but failed to stress the most crucial thing (from today's point of view): that the deuterium – tritium mixture would be *compressed* as the radiation of the primary atomic bomb heated both it and the inert material into which the mixture would be immersed; that is, he failed to focus on the radiation implosion (in Fuchs' original report it was pointed out that the transport of radiation would equalize the temperature in the deuterium – tritium mixture and the inert material, thus giving rise to a pressure differential. Upon compression, the deuterium – tritium mixture would be ignited, that is, a nuclear reaction would start).

Based on Zel'dovich's and other reports of 1950 on the H-bomb problem, including Khariton's summary report, I felt it legitimate to state in [1] that Soviet scientists accepted the idea of using a two-stage initiator for the Tube, suggested by Fuchs' 1948 material, along with a deuterium – tritium mixture as the secondary unit. However, as work on the two-stage initiator went on, it was conjectured that the secondary deuterium – tritium mixture unit could be compressed and, as a result, ignited by the energy of a shock wave. Therefore, an arrangement with a gun-type atomic bomb and a heavy radiation-impervious tamper was chosen as the main scheme for the two-stage initiator of the Tube. Fuchs' seemingly more complicated scheme with a light tamper transparent for the radiation from the atomic bomb, confined in an opaque enclosure, and operating on the radiation implosion principle remained in the background and was never subjected to mathematical analysis.

Therefore, the research done in 1950 on a two-stage initiator for the Tube, which, according to Zel'dovich and Sakharov, marked the start of work in the Soviet Union on a new design principle for the H-bomb failed to provide the prerequisites for a successful advance toward its creation, since the development of a two-stage thermonuclear bomb was needed. This realization occurred in the Soviet Union in 1952, that is, before the United States carried out its Mike thermonuclear test. It was Zel'dovich who stressed the need for theoretical and experimental research along this line in a document, *Work on the RDS-6*, dated September 20, 1952. It is not unlikely that as early as 1952 Davidenko proposed a two-stage binary thermonuclear charge scheme similar to the one Zel'dovich and Sakharov investigated in January 1954 [1]. In his scheme, the thermonuclear unit would be compressed by the material component of the energy released by a primary atomic explosion.

In early 1953, work on a two-stage thermonuclear charge was included in the plan of Zel'dovich's theoretical sector. The stumbling block was the symmetric compression of the thermonuclear unit. In 1953, A P Zavenyagin and D A Frank-Kamenetskii presented their view on how the difficulties related to the problem could possibly be overcome — they proposed their own schemes for two-stage thermonuclear charges named the Kandelyabr (Candelabrum) and the Britva (Razor) [1, 6, 14]. In those schemes, too, the thermonuclear core would be compressed by the material component of the energy released by an atomic explosion. The schemes had no connection with intelligence reports (in the sense that they did not fit any specific information of the kind).

I am surprised by the reasoning of my opponents who infer from the simple reference in only one phrase in [1] to Zavenyagin's proposal that "Goncharov's article gives a false idea about Zavenyagin's proposal as well" and that "This phrase could not but mislead many. His argument seems to be this: since in the United States a two-stage charge was identified with a binary one and since Zavenyagin was not a physicist, his 'original scheme' was certainly the handiwork of the intelligence service." I do not know if US nuclear physicists identify a two-stage thermonuclear charge with a binary thermonuclear charge, but I feel it necessary to stress that if I had even the slightest doubt about the independence of Zavenyagin's and Frank-Kamenetskii's proposals, I would have never described them as 'original.'

Nor can I agree with my opponents' assertion that 'Historically, Zavenyagin's proposal was the first impetus that set the course of the search,' that is, a search for ways of achieving 'a degree of compression of the thermonuclear fuel beyond the capabilities of conventional explosives.' This assertion of my opponents runs counter to all the documentary evidence mentioned above, including Zel'dovich's and Sakharov's statements. According to them, the work on the principle of a two-stage thermonuclear charge began in the Soviet Union in 1950; the discussions of 1952 were an important intermediate stage, and in 1953 the theorists at Arzamas-16 began to work on a planned basis (true, it was the plan of Sector No. 2 rather than of KB-11 as a whole), and the course of the search was mapped by Sakharov in his first report on the Layer Cake, issued as early as 1949. As to Zavenyagin's proposal, I may say that by virtue of his position he was informed of all discussions held at Arzamas-16 on the two-stage thermonuclear charge and clearly realized how important such a development would be if it could be implemented in practice. He was also informed of the difficulties the scientists faced in their attempts to achieve a symmetrical compression of the thermonuclear unit in the two-stage configuration. That was why he came up with his idea of the Candelabrum in one of the discussions as a possible way to achieve symmetric compression. Of course, Zavenyagin's cumbersome scheme was never taken seriously.

Some hopes were pinned on the Razor configuration, but the simpler binary design evoked the greatest interest. In January 1954, Zel'dovich and Sakharov presented a memo in which they assessed the two-stage binary thermonuclear charge presumably proposed by Davidenko. As I note in [1], nothing in this memo suggested a comprehension that it would be possible to release radiation from an atomic bomb and to use it to compress the thermonuclear unit. The revelation came in the early months of 1954, possibly soon after the United States had tested the high-yield Bravo shot on March 1, 1954 with tragic consequences. The world saw the difference between a fission and a fusion bomb. It is not unlikely that public reports about the Bravo shot gave new impetus to Soviet scientists in their search for an efficient design for a high-yield thermonuclear bomb.

By that time, Soviet scientists had realized that the Tube and boosted versions of a single-stage configuration of the Layer Cake held no promise and had come close to the idea of the Teller – Ulam configuration. A two-stage scheme had already been developed, incorporating many elements of the Teller – Ulam configuration, but its pivotal principle — the use of radiation from a primary atomic bomb to compress the

thermonuclear unit — was yet to be recognized and formulated. As I note in [1], a good deal of thought, assessment of all the available information, and the experience by then accumulated to date had finally led to the goal in March-April 1954. The new compression mechanism — the implosion of the secondary thermonuclear unit by the radiant energy of a primary atomic bomb — had been discovered. The promise held by the radiant energy of a primary atomic charge as a means of compressing the thermonuclear unit symmetrically was also recognized.

All those involved in the project recollect how suddenly new ideas erupted. This is vividly described by L P Feoktistov, one of Zel'dovich's closest coworkers, who directly worked on the RDS-37, the first Soviet two-stage thermonuclear charge: 'New ideas dawned upon us suddenly like light in a dark kingdom, and it was clear that the instant of truth had come. Rumors ascribed these fundamental thoughts in Teller's spirit now to Zel'dovich, now to Sakharov, now to both, or to somebody else, but always in some indecisive form: likely, possibly, and so on. By that time, I had come to know Zel'dovich quite closely, but I never heard a direct confirmation from him on that score (as, indeed, directly from Sakharov)' [14].

It is likewise a fact that there are no documents or reports associated with the new ideas, which could tell us who was the first or the originator.

My opponents interpret my articles [1] as the intention on my part to prove that the Soviet analog of the Teller – Ulam configuration, the Third Idea in Sakharov's words, was a direct product of the intelligence service. Actually, I uphold an entirely different view in [1]: in tracing the history of the Third Idea, I clearly state that a good deal of thought, an assessment of all the available information, and the experience accumulated by then led to the goal. I thus stress both the creative nature of the discovery, the importance of soviet experience which was, at the time, quite substantial, and the role of the information gathered in R&D work which, too, was very large in scope. However, in view of the documentary evidence referred to above on what the intelligence reports carried and how they were accepted in the Soviet Union, it cannot be ruled out that Fuchs' 1948 document could have helped in the discovery of the Third Idea as well. But one thing must be stressed quite definitely. Even if Fuchs' document (the copy sent to Khariton in 1948 was still kept in Arzamas-16 in 1954, so Fuchs' information turned up for Soviet nuclear physicists to see before 1954) did help the discovery of the Soviet analog of the Teller – Ulam configuration, this does not belittle the importance and intellectual value of the idea breakthrough that took place in the Soviet work on a two-stage thermonuclear charge in 1954.

The same can be said about the discovery of the Teller – Ulam configuration in the United States in 1951, which was facilitated, most probably, by the ideas generated in the United States as early as 1946. In the Soviet Union, six years went by between the day Fuchs passed on to the Soviet Union the document describing the idea and scheme of radiation implosion and the discovery of the Soviet analog of the Teller – Ulam configuration in 1954. In the United States, 5 years passed from the inception of the idea and scheme of radiation implosion in 1946 to the discovery of the Teller – Ulam principle in 1951. Even for the United States where, in contrast to the Soviet Union, the discovery of the Teller – Ulam configuration was preceded by the preparation of the George test, the physical scheme of which was identical to Fuchs' scheme using radiation-implosion, this time span was considerable†.

It is relevant to note that in his memo, *Comments on the History of the Bethe Thermonuclear Program*, dated August 14, 1952, Teller writes: 'Radiation implosion is an important but not a unique device in constructing thermonuclear bombs ... [referring to bombs, Teller had in mind the Teller – Ulam configuration — Author].' He also stresses that 'the main principle of radiation implosion was developed in connection with the thermonuclear program [referring to the classical Super program — author] and was stated in a conference on the thermonuclear bomb, in the spring of 1946'' [9, 16].

In the United States, the following definition of the key principle of a thermonuclear weapon has been officially declassified: 'In thermonuclear weapon, radiation from a fission explosive can be confined and used to transfer energy to compress and ignite a physically separate component containing thermonuclear fuel' [11]. This definition applies equally to Fuchs' scheme, passed on to the Soviet Union in 1948, and to the Teller – Ulam configuration and its Soviet analog. However, the Fuchs scheme and the Teller – Ulam configuration use significantly distinct modifications of the radiation-implosion idea. In the Fuchs scheme, the radiation confined by the casing would be used for the ionization compression of the radiation-heated thermonuclear core which would be a liquid deuterium – tritium mixture. In the Teller – Ulam configuration it serves to generate a shock wave which compresses a thermonuclear core of a more elaborate design, not heated by radiation [1].

The idea of the Teller – Ulam configuration and of its Soviet analog would hardly have been conceived if a thermonuclear core of special design had not been proposed and if it had not been comprehended and confirmed that it would be able to function normally under conditions significantly different from the physical conditions in which the secondary unit of a deuterium – tritium mixture would operate in the Fuchs scheme. Therefore, there was not, and could not be, a direct transition from the radiation implosion idea in the Fuchs scheme to the new radiation implosion idea in the Teller – Ulam configuration.

Speaking of how 'K Fuchs' proposal' stands vis-a-vis 'the Teller – Ulam configuration,' my opponents 'hope US scientists themselves will speak on the matter.' But, at the same time, they equate the essence of these proposals. They write that if Fuchs passed on information to the Soviet intelligence service about 'a two-stage design operating on the radiation implosion principle,' then this 'implies that as of the spring of 1948, nuclear physicists in both the United States and the Soviet Union were in about the same starting position as regards the underlying ideas (necessary to design a thermonuclear charge operating on the radiation implosion principle). Hence, one may conclude that they already possessed knowledge necessary to solve the problem immediately.'

Reality did not confirm this conclusion. It took a fairly long time to switch from Fuchs' ideas to the Teller – Ulam configuration and its Soviet analog. That was so above all

† The connection between the discovery of the Teller – Ulam configuration and Teller's work on the George shot in the Greenouse test series is noted, in particular, by Bethe and Rosenbluth, a theoretical physicist who worked on the Mike shot. As Bethe writes, when Teller was working on the new concept he was probably influenced by his thoughts on the George shot [15]. Rosenbluth definitely asserts that, in his opinion, Teller was led to the concept by preparation of tests for the Greenouse test series [13].

because of the extreme complexity of the physical processes that must be considered in the assessment and substantiation of thermonuclear charge designs. Therefore, the early ideas could not evolve in either the United States or the Soviet Union until mathematical modeling had achieved a sufficiently high level and these subtle physical processes were understood. Note that when the Teller – Ulam configuration was discovered in the United States in the spring of 1951, questions arose there similar to that of my opponents. They were prompted because those involved in the US H-bomb project became aware that Fuchs' scheme and the new configuration were close in their basic idea.

This gave rise to a debate in the United States, centered on two questions. First, why did the transition from the 1946 ideas to the ideas of the Teller – Ulam configuration discovered in 1951 take so much time? Second, could the information about the superbomb Fuchs passed on to the Soviet Union lead to the discovery of the Teller – Ulam configuration in the Soviet Union earlier than in the United States? The second question was especially poignant because Teller did not rule out that Fuchs could have communicated the radiation implosion idea to the Soviet Union [9, 16]. Without going into all aspects of the debate, I would like to note two relevant statements of Teller. In his 1952 memo mentioned above, Teller said: 'It is a miracle that the new concept were not conceived sooner' [9]. A decade later, in 1962, Teller wrote: 'If the Los Alamos Laboratory had continued to function after Hiroshima with a full complement of such brilliant people as Oppenheimer, Fermi and Bethe, I am convinced that someone would have had the same idea much sooner — and we would have had the hydrogen bomb in 1947 instead of 1952' [17].

Not all participants in the US atomic and thermonuclear project held this view. In his *Observation on the Development of the H-Bomb* of May 23, 1952, Bethe wrote, among other things, that in his opinion the discoveries of key principle of the Teller–Ulam configuration had been largely accidental: it could not be assumed that intensive work on the early ideas would lead in a straightforward way to the Teller – Ulam concept. For this reason and also because the 'classical Super' concept failed, as was demonstrated in 1950, Bethe believed there was ample reason to think that, although Fuchs had given away information on the superbomb, the Soviet Union was not ahead of the United States in its development [9, 16].

Norris Bradbury, director of the Los Alamos Laboratory from 1945 to 1970, said in 1954: 'We would have spent time lashing about in a field in which we were not equipped to do adequate computational work. We would have spent time exploring by inadequate methods a system which was far from certain to be successful — I cannot see how we could have reached our present objectives in a more rapid fashion [than] the mechanism by which we went' [17]. These words can be fully applied to the early work on the superbomb in the Soviet Union.

As I note in [1], the superbomb information Fuchs passed on to the Soviet Union did not lead to the discovery of the Teller – Ulam configuration in the Soviet Union earlier than in the United States. Teller's misgivings on that matter did not come true. However, the discovery of the Teller – Ulam configuration at a later time than in the United States was more than made up for by the development of the Layer Cake where lithium-6 deuteride was to be used as thermonuclear fuel. The theoretical work and creation of the Layer Cake served as the basis for a practical embodiment of the soviet analog of the Teller – Ulam configuration after its principles were discovered in the spring of 1954. That predetermined the fast progress of the Soviet Union in further thermonuclear work.

Research and development obey an internal logic which requires, as a rule, a detailed analysis and a thorough comprehension of every previous step before the next can be made. When they commenced their work on the 'classical Super' and its Soviet analog, the Tube, scientists in the United States and the Soviet Union ran into a multitude of complex physical processes under extremal physical conditions that have no analogs in any other branch of technology. The first radiation implosion scheme, too, proved hard to analyze. US scientists were not able to start their analysis of such a scheme by numerical methods until October 1949 although it was proposed in early 1946. In the Soviet Union, Fuchs' scheme was not subjected to mathematical analysis at all because of its complexity. Of course, when an underlying solution to the problem of a two-stage hydrogen H-bomb was found in the Soviet Union in 1954, Soviet scientists were in a position to analyze it theoretically and to do the necessary calculations using relatively simple technical facilities. But before that, a huge amount of work of the highest theoretical class had to be done and the necessary experience acquired. That was why I felt it legitimate to describe the creation of the thermonuclear weapon which scientists in the United States and the Soviet Union achieved in the 1950s as one of the most challenging problems mankind has ever faced in its history. Quite probably, this statement is too emotional, but it reflects the real complexity of the task.

The keynote of my opponents' letter [6] is the desire to depict the creation of a two-stage H-bomb in the Soviet Union as 'a natural step' in improving the thermonuclear weapon after the Layer Cake was tested on August 12, 1953, in which 'the entire sequence of ideas and arguments was covered in two or three months in early 1954' and 'with no impulses from without.' This treatment of history does not fit the documentary evidence presented above. When, instead of a description of real events related to the birth of the two-stage thermonuclear bomb in the Soviet Union, they offer an artificial construction entirely divorced from documentary evidence in the form of three stages that swiftly followed one another, my opponents take special care in [6] to stress that Soviet physicists were fully independent in their work on a two-stage H-bomb and picture the situation as if the work of 1954 had not been preceded by any ideas from domestic or foreign sources and that it was completely isolated from any external events.

To prove the total independence of the Soviet effort, they give a quotation from Sakharov's *Memoirs* [12] which, as they believe, supports their point: 'Apparently, several workers from our theoretical departments came upon the Third Idea at the same time. I was among them. It seems I got an understanding of its physical and other aspects rather early. Because of this and also because I already carried some authority, my role in the acceptance and implementation of the Third Idea was possibly particularly decisive. But, undoubtedly, the role of Zel'dovich, Trutnev and some others was as important; quite possibly, they were aware of and foresaw the prospects and limitations of the Third Idea no less than I. But there was no time for us (in any case, for me) to think of who was the first, the more so that it would have looked like selling the bearskin before catching the bear,

and it is impossible, if at all necessary, to reconstruct all details of the discussion in retrospect.'

Let us see how V I Ritus, one of Sakharov's closest coworkers and a direct participant in the work on the Layer Cake and RDS-37 (the first Soviet two-stage thermonuclear charge) comments on the above quotation (I, too, spoke along similar lines on Sakharov's statement in the intermediate report my opponents cite in their letter): "When he describes the advent of the Third Idea, Sakharov uses the words 'apparently', 'it seems', 'possibly' and 'quite likely' on four occasions in four phrases, but he does not name the particular persons who conceived the Third Idea, and speaks more of his comprehension of the idea. Sakharov sees his role in the acceptance and implementation of the Third Idea. Sakharov thought it impossible and, indeed, unnecessary to comment on the issue of 'who was the first.' Why?"

On the other hand, Sakharov is entirely outspoken on his and Ginzburg's priority in time when it comes to the First and Second Ideas — the idea of the Layer Cake and the use of lithium-6 deuteride as thermonuclear fuel. When he recollects in *Memoirs* his participation in the Soviet thermonuclear project, Sakharov qualifies that he will describe his life in 1946 to 1968 with some omissions because of secrecy. Was Sakharov in a position when he wrote his *Memoirs* to discuss the role of intelligence data? Note how careful he is when he speaks of the origin of the Truba design: 'Now I suspect that the underlying idea of the design on which the Zel'dovich group worked was 'a solid piece of plagiary,' that is, based on intelligence reports. But I cannot prove the suspicion. It came to my mind quite recently, and I gave no thought to it at that time.'

Moreover, to support their view, my opponents refer to Khariton's words that 'Soviet scientists developed the H-bomb fully independently.' But, undoubtedly, when he spoke, Khariton had three indisputable facts in mind: first, that Soviet physicists created the H-bomb steering their own original course; second, that the first Soviet thermonuclear charge Layer Cake was an entirely independent development; and third, that Soviet scientists were not supplied with the Teller – Ulam configuration by the intelligence, so they discovered its Soviet equivalent, the Third Idea, on their own.

Indeed, once they comprehended what the Third Idea had in the offing, Soviet scientists pursued it in 1954 despite the official stand of the Ministry of Medium Machine Building. My opponents write: "... it is unlikely that in those years Minister Malyshev could so furiously oppose the implementation of the Third Idea if it had been the product of the intelligence service. But he did and, as Sakharov recollects, brought matters to a point where Kurchatov, who supported the initiative of the Arzamas-16 nuclear physicists, received a 'strict Party reprimand' for his 'anti-state conduct.' But the crux of the matter was that the Third Idea (not to be confused with the radiation implosion idea of the Fuchs scheme) was not the product of the intelligence service, and Malyshev saw his primary task in fulfilling the resolution the Government adopted on his initiative in November 1953, *On the Creation of a New Type of High-yield Hydrogen Bomb*, where the new hydrogen bomb meant the boosted version of the single-stage Layer Cake. Aware of the difficulties that had arisen in the course of work on this design, Malyshev feared that the Arzamas-16 scientists engrossed with the Third Idea might fail to fulfill the Government's resolution which ruled that the new H-bomb was to be tested at the end of 1954. For both Malyshev and those involved in the work at Arzamas-16 were aware that it would be impossible to develop and test a thermonuclear charge based on the Third Idea by the stipulated target date, and Malyshev hardly thought it possible to move the target date to a later time. Moreover, did Malyshev have any reason to believe in the success of the effort at the early stage of work on the Third Idea? It is unlikely that, in justifying his support of the initiative taken by the Arzamas-16 scientists, Kurchatov referred to Fuchs' materials, nor would this reference be convincing for Malyshev because what could be inferred from the Fuchs material was not identical to the Third Idea.

In response to my opponents, I cannot pass unanswered their assertion that I 'said nothing' in [1] about the article by Khariton, Adamskiĭ, and Smirnov [5] which 'was the first publication about the evolution of Soviet thermonuclear ideas.' I disagree with the assertion that [5] was the first publication on the subject. The first publications which broke the veil of secrecy over the physical essence and, to a certain degree, the evolution of Soviet thermonuclear ideas were the articles by Ritus [2] and Romanov [3]. As to my failure to mention [5] in [1], I would like to explain that I included in the list of references only those which I used as primary sources.

I do not think it is my task to criticize [5]. However, I must note that it contains inaccuracies and so cannot be used as a reliable source of historic facts unconditionally. I will give two examples. A very important piece of evidence bearing on the history of the thermonuclear program in the Soviet Union — a hand-written memo to Khariton, dated January 14, 1954 and entitled *About Using the Gadget* [*atomic bomb*] *for Implosion of the Supergadget* [*H-bomb*] RDS-6s, with a theoretical assessment of a two-stage thermonuclear charge — is presented in [5] as a note written by Zel'dovich. Actually, this memo was in two parts making up an integral whole. One part was written by Zel'dovich and the other, by Sakharov. As is indicated on the last page, the memo was executed by Zel'dovich and Sakharov. May one refer to the memo as a note written by Zel'dovich alone?

Khariton, Adamskiĭ, and Smirnov [5] assert that 'the Mike experiment influenced the Soviet hydrogen weapon program only as a fact of a high-yield thermonuclear shot.' They go on to say that 'although, owing to its strong neutron flux, the 1952 Mike shot was evidence that a high density of thermonuclear fuel was achieved in the exploded device, a radiochemical analysis of samples could not, in principle, give any clue about the concrete arrangement of the device.' That is a twisting of the facts. Actually, more or less reliable information about the yield of the Mike shot was nonexistent in the Soviet Union in 1952 – 1954. The directors of the Soviet thermonuclear project interpreted the Mike test which the United States carried out on November 1, 1952 (most probably on the basis of the open publications, including papers in the November 29, 1952 issue of the *US News and World Report*) as a Layer Cake-type thermonuclear charge similar to the RDS-6s single-stage thermonuclear charge which the Soviet Union was preparing to test. Moreover, Soviet scientists knew nothing at the time about the high neutron flux and the high density of thermonuclear fuel achieved in the Mike shot†.

† Information that a very high neutron flux density had been achieved in the Mike shot appeared in the August 1955 issue of *Physical Review* in a letter on the detection of the new chemical elements einsteinium ($Z = 99$) and fermium ($Z = 100$) in the explosion products. (*Physical Review* **99** 1048 (1955)).

Going back to the letter [6] of my opponents, I wish to note that it gives an inaccurate notion about some important aspects of the US thermonuclear program. Contrary to the assertion in [6], work on a boosted fission charge using thermonuclear reactions went on in the United States concurrently with research on an H-bomb from the outset and was not an intermediate stage on the way to the latter. An important point in the US program was the thermonuclear test, known as the George shot, in May 1951, where a version of radiation implosion was used — the Teller – Ulam configuration was discovered during preparations for that test.

Lastly, about the creation of the British H-bomb. In this connection, I wish to draw the reader's attention to C Hansen's remark in [11] that 'the British were the next nuclear power to discover radiation explosion.' Hansen quotes Bradbury who, some time after the Mike shot in November 1952, found out in a meeting with his British counterparts: 'It was perfectly obvious they were discussing that we had gone through great pains in inventing by Teller and Ulam and others. And they'd invented it too. Whether or not there was technical leakage was never learned.' Therefore, the origin of the British idea of the hydrogen H-bomb is a far more intricate problem than its opponents imagine from the only evidence they have chosen [18].

I wish to thank the Editor of *Uspekhi Fizicheskikh Nauk* for the chance to answer the critical letter of my opponents and to throw light in its pages on important and interesting aspects in the history of the Soviet thermonuclear project.

References

1. Goncharov G A "American and Soviet H-bomb Development Programmes: Historical Background" *Usp. Fiz. Nauk* **166** (10) 1095 (1996) [*Phys. Usp.* **39** 1033 (1996)]; Goncharov G A "Thermonuclear Milestones" *Physics Today* **49** (11) 44 (1996)
2. Ritus V I "If Not Me, Who Else?" *Priroda* **8** 10 (1990)
3. Romanov Yu A "The Father of the Soviet H-Bomb" *Priroda* **8** 21 (1990)
4. Romanov Yu A "Recollections of the Master" *Usp. Fiz. Nauk* **166** (2) 195 (1996) [*Phys. Usp.* **39** 179 (1996)]
5. Khariton Yu B, Adamskiĭ V B, Smirnov Yu N "On the Creation of the Soviet Hydrogen (Thermonuclear) Bomb" *Usp. Fiz. Nauk* **166** (2) 201 (1996) [*Phys. Usp.* **39** 185 (1996)]
6. Adamskiĭ V B, Smirnov Yu N "Once Again on the Creation of the Soviet Hydrogen Bomb" *Usp. Fiz. Nauk* **167** (8) (1997) [*Phys. Usp.* **40** 855 (1997)]
7. Gershtein S S "From Reminiscences about Ya B Zel'dovich" *Usp. Fiz. Nauk* **161** (5) 170 (1991) [*Sov. Phys. Usp.* **34** 455 (1991)]
8. Gurevich I I, Zel'dovich Ya B, Pomeranchuk I Ya, Khariton Yu B "Utilization of the Nuclear Energy of the Light Elements" *Usp. Fiz. Nauk* **161** (5) 17 (1991) [*Sov. Phys. Usp.* **34** 351 (1991)]
9. US Congress Joint Committee on Atomic Energy, Policy and Progress in the H-Bomb Program: A Chronology of Leading Events, US Govt.Printing Office, Washington, DC (1953)
10. Frenkel', Ya I "Atomic Energy and Its Release" *Priroda* **5** 7 (1946)
11. Hansen C *US Nuclear Weapons: The Secret History* (New York: Orion Books, 1988)
12. Sakharov A *Memoirs* (New York: Chekhov Publ., 1990) p. 242
13. Rhodes R *Dark Sun*: *The Making of the Hydrogen Bomb* (New York: Simon and Schuster, 1995)
14. Feoktistov L P "The Hydrogen Bomb: Who Gave Away the Secret" *Newspaper Kurchatovets* (6-7) (1996) RNTs-KI p. 4
15. Bethe H A "Observation on the Development of the H-Bomb" (1954), in York H F *The Advisors. Oppenheimer, Teller, and the Superbomb* (Stanford: Stanford University Press,1989)
16. Holloway D *Stalin and the Bomb* (New Haven: Yale University, 1994)
17. York H F *The Advisors. Oppenheimer, Teller, and the Superbomb* (W H Freeman and Company, 1976)
18. Hirsch D, Mathews W "The H-Bomb: Who Really Gave Away the Secret?" *Bulletin of the Atomic Scientists* (**1 – 2**) 22 (1990) [Translated into Russian *Usp. Fiz. Nauk* **161** (5) 171 (1991)]