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On the making of the Soviet hydrogen (thermonuclear) bomb

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Abstract. The May 1991 issue of 'Uspekhi Fizicheskikh Nauk' ('Physics – Uspekhi), devoted to the 70th birthday of Andreĭ Dmitrievich Sakharov, included a translation of an article by D Hirsch and W Mathews, 'The H-bomb: Who Really Gave Away the Secret?' It was the Editors' hope that, prompted by this polemic and in many respects controversial publication, our physicists and nuclear weapons developers would speak out too. However, it is only now, five years later and with a number of formal restrictions lifted, that the Journal has received an article on the creation of the Soviet H-bomb written by Arzamas-16 (All-Union Research Institute of Experimental Physics) specialists (of whom Yu N Smirnov worked under A D Sakharov in the 1960s). The Editors are confident that this article will be of interest to our readers. What makes the article particularly authoritative is the co-authorship of Yuliĭ Borisovich Khariton, a patriarch of national nuclear physicists. His brilliant debut dating back to the 1920s, in the N N Semjonov group and at the famous Cavendish Laboratory (then under E Rutherford), from 1946 to 1992 Yu B Khariton was a permanent scientific leader of the Arzamas-16 Nuclear Weapons Centre. It is primarily to I V Kurchatov and Yu B Khariton that we owe the creation of our nuclear weapons which laid the groundwork for the country's powerful defence potential. Editorial Board

In 1990, an article by D Hirsch and W Mathews, titled 'The H-bomb: Who Really Gave Away the Secret?' was published in the USA [1]. That the USSR had profited from American secrets when developing the H-bomb seemed an indisputable fact to the authors and was indeed emphasised in the title. This view was commonly held for a long time in the West.

According to D Hirsch and W Mathews, it is the radiochemistry data on the U.S. explosions of the early 1950s which suggested to the Soviet scientists the need to strive for high compression of thermonuclear fuel. In fact, an H-bomb explosion is accompanied by an ejection of a large amount of various radionuclides into the atmosphere, whose analysis can provide information about the degree of thermonuclear fuel compression. In the 1960s, the American, Chinese and French explosions were monitored by us. Air samples were taken, then their radiochemistry analysis was carried out and theoretically interpreted, and finally, hypotheses on the tested design were formed. It is, however, only in the late 1950s that this service was put right in our country. It proved useful in monitoring the American Johnstone Island test in 1962. By the time of the 1952 'Mike' test — the first American thermonuclear explosion, in the form of a 65-t device with liquid deuterium as thermonuclear fuel — we had not had such service yet. Thus, the Mike experiment affected the Soviet Hydrogen Weapons Programme only to the extent that the fact of a high-yield hydrogen explosion became known.

The progress of thought and the interplay of ideas at the time were of such a nature that the Soviet nuclear weapon developers did not actually need to be prompted by the high density idea. At issue was not to clarify whether high compressions were needed (no one doubted that) but rather how to achieve them.

Today, following a series of domestic publications [2], there is wide recognition that Soviet scientists not only designed the hydrogen bomb independently, but even were ahead of their American counterparts in some respects.

True, in November 1953 the Americans were the first ever to conduct a thermonuclear explosion. Its yield was in excess of 10 Mt, and the neutron flux was so high that American physicists who studied explosion products even managed to discover two new transuranium elements, dubbed Einsteinium and Fermium.

The device exploded was not compact enough to be called a bomb, however. Rather this was a vast ground-surface laboratory structure the size of a two-storey house, the thermonuclear fuel being in the liquid state close to the absolute zero of temperature. The experiment became an intermediate step on American physicists' road to the hydrogen weapons. Soviet scientists, however, did without this overcomplicated and costly experience.

On August 12, 1953, based on the scheme proposed by A D Sakharov's and called the layer-cake (or 'sloyka' in Russian), a successful test of the first ever real hydrogen charge was conducted in the USSR. At V L Ginsburg's suggestion, lithium, in the form of a solid chemical compound, was used as the thermonuclear fuel. This enabled an additional amount of tritium to be obtained in the course of thermonuclear (explosion) reactions, thus increasing markedly the charge yield.

The thermonuclear charge tested in the USSR was ready for use as a transportable bomb, i.e., was the first hydrogen weapons specimen. The charge was somewhat heavier than and of the same dimensions as the first Soviet atomic bomb, tested in 1949, but its yield was 20 times as high (the yield of the August 12, 1953 explosion was about 400 kt). The important point is that the thermonuclear reaction contribu-

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tion proper to the total yield was close to 15 – 20%. The experiment was a great, priority achievement of our physicists, particularly A D Sakharov and V L Ginzburg. Mention should also be made of I E Tamm, then (and until 1954) head of a group of theoretical physicists engaged in this field.

At that time, in the USA there existed nothing similar as far as thermonuclear weapon is concerned. There can be no equality sign between the Soviet 1953 explosion and the US 1951 experiments on small amounts of tritium and deuterium, designed, in Bethe's words, 'primarily to confirm ... the burning of D-T [deuterium and tritium], about which there had never been serious doubt' [1]. Even less one can compare the Soviet success with the 1952 American explosion, whose nearly-absolute-zero liquid thermonuclear fuel ruled out the production of sufficiently compact and transportable thermonuclear charges.

Preceding the history of Soviet thermonuclear weapon development, whose major stages are presented below, is an important event which should be considered as its inception.

In 1946, a joint project written in the form of an open report was submitted to I V Kurchatov by I I Gurevich, Ya B Zel'dovich, I Ya Pomeranchuk, an Yu B Khariton. Clearly, if the report had been based on intelligence materials, it would have been given a top security stamp automatically. The idea of the project was to use an atomic explosion as a detonator to secure an explosive reaction in deuterium. These were, in other words, the first Soviet estimates concerning the possibility of a thermonuclear explosion.

According to I I Gurevich, deuterium reactions with light nuclei interested him and I Ya Pomeranchuk as a possible source of stellar energy. The two discussed the problem with Ya B Zel'dovich and Yu B Khariton who, in turn, realised that the thermonuclear fusion of light nuclei may prove feasible under terrestrial conditions provided deuterium is heated up by an atomic explosion shock wave.

The scientific report of the four was typewritten as unclassified, has never been classified as top secret, and until now has been kept in the open department of the Kurchatov Institute's Archives. I I Gurevich recollected: 'Here is an eloquent proof that we were fully unaware of the American developments. Otherwise, you can readily understand what secrecy stamps the report would have and how totally inaccessible it would be ... I guess we were simply waved away at the time. Stalin and Beriya were bending over backwards to produce the atomic bomb, and, without even an experimental reactor on, here are these high-brow pundits pestering people with projects whose feasibility is anybody's guess [3]'.

The report of I I Gurevich, Ya B Zel'dovich, I Ya Pomeranchuk, and Yu B Khariton was first published as recently as 1991, in the 'Uspekhi Fizicheskikh Nauk', and is a historic document today [4]. Not only does it contain a proposal how a thermonuclear reaction could be realised by an atomic explosion, but it also demonstrates the awareness that a nuclear reaction in deuterium 'will proceed undamped only at very high temperatures of the entire mass involved.' It was emphasised that 'it is desirable to have the highest possible deuterium density' and that to facilitate the occurrence of nuclear detonation, massive shells for slowing down scattering are useful.

Curiously, practically at the same time, in April 1946, a secret conference was held at the Los Alamos Laboratory, with Klaus Fuchs's participation, to review the results of American H-bomb efforts from 1942 on (it was not until four years later, in 1950, that Americans saw the technological realisation of that line of research to be in fact wrong). Some time after the meeting, Fuchs passed research materials to Soviet intelligence, later to be handed over to Soviet physicists. As D Hirsch and W Mathews write in the article mentioned above, 'The ... Teller's conception of thermonuclear weapons from 1942 until 1950, was essentially a cylinder of liquid deuterium† ... heated ... by a very large exploding fission A-bomb.' The mathematician Stanislaw Ulam and his assistant Cornelius Everett carried out some calculations at Los Alamos, which revealed that the Super would require by far more tritium than suggested by Teller. Later, Hans Bethe noted in his 1952 memorandum that the theoretical calculations by Fermi and Ulam in 1950 had shown the probability of a propagating thermonuclear reaction to be very low. Thus, the Los Alamos scientists came to the conclusion that the 'tube' project was actually going nowhere. Later, H Bethe was quite definite about that situation: 'We were on the wrong track ... [realised that] the hydrogen bomb design we thought would work best would not work at all' [1].

In early 1950 Klaus Fuchs was arrested and, quite naturally, the Soviet physicists remained unaware of the dramatic conclusions their American counterparts had come to.

The situation in Russia evolved as follows.

In June 1948, under the Government's resolution, a special group was founded at the FIAN, with I E Tamm as the head and A D Sakharov one of its members, to investigate the feasibility of the hydrogen bomb. One of the group's tasks was to verify and specify the results of the Zel'dovich Moscow group at the Institute of Chemical Physics. It should be noted that at that time, both Ya B Zel'dovich's Moscow group and his Arzamas colleagues devoted part of their efforts to the 'tube' concept — in accordance with the information passed over by K Fuchs.

However, as Yu A Romanov recalls, "after only a couple of months, Andrei Dmitrievich formulated some basic ideas which determined the direction of the entire problem's development. Until that point, Zel'dovich group considered liquid deuterium (possibly mixed with tritium) as fuel for the thermonuclear facility. Sakharov proposed a heterogeneous structure — named the 'layer cake' by him — with alternating layers of light elements (deuterium, tritium and their chemical compounds) and heavy elements ^{238}U" [5].

Thus, from 1948 on, two lines of research, the 'tube' and the 'sloyka', evolved in parallel, the latter being clearly preferred due to its obvious advantages and producibility. It is the 'sloyka' concept, as already mentioned above, which was successfully realised in the Soviet thermonuclear charge test on August 12, 1953.

The 'tube' efforts continued, however. Indeed, by the early 1950s, along with Zel'dovich's Arzamas and Moscow groups, a few young colleagues of D I Blokhintsev, in Obninsk, were mobilised to probe into certain aspects of this idea. They were charged with solving the problem of energy transfer by neutrons for the case when thermonuclear ignition occurs in the tube; as well as research into detonation wave propagation in deuterium.

Although there were many physically interesting and intriguing problems, the tube group were becoming increas-

†In Russia, this container was traditionally called a 'tube'. (*Note by the authors*).

ingly aware of being somewhat off the mainstream. The basic component of their work, hydrogen isotopes in the liquid phase, was the factor which alone rendered their prospects bleak. The calculations were of sufficiently high accuracy, and one could see that if the neutrons released all the energy locally, everything would be fine. Instead, however, they carried the energy over large distances along the tube. No idea of any promise was forthcoming. Calculations with more optimistic initial conditions did show some promise, though. In a word, with no positive solution guaranteed and results highly sensitive to the choice of initial parameters, the problem appeared nebulous and of no practical interest.

By early 1954, rather a bizarre situation had arisen in the theoretical departments of Arzamas-16 Institute, in which, after the successful August 12, 1953 explosion, both the sloyka and tube trends persisted in the thermonuclear charge development work.

Now the sloyka concept had some potential for improvement. The charge yield could be increased to a megaton, and so a higher-yield sloyka modification was being studied. Some feeling of dissatisfaction had materialised, however, by the very fact of the sloyka construction being very cumbersome. In addition, the sloyka tested on August 12, 1953 contained a considerable amount of tritium. The charge was therefore very costly, and its service life was relatively short (half a year or so). These two drawbacks were completely overcome, however, and on November 6, 1955, another — totally tritium-free — sloyka version was successfully tested in the USSR. There was, naturally, some reduction in yield as compared to the prototype. The hydrogen bomb was dropped from a plane flying at a 1-km height and was the first of its kind ever. If, for one reason or another, the two-stage charge idea (realised in the Soviet test on November 22, 1955 and somewhat earlier in the USA) had proved infeasible in principle, still, with the experiment of November 6, 1955, the USSR would have had rather viable, relatively cheap and transportable thermonuclear weapons at its disposal.

In early 1954, a momentous meeting on the tube project was held at the Ministry of Medium Machine Building, with the Minister, V A Malyshev, being involved. This strategy had already been widely debated previously, but this meeting proved to be the last one. Among the attendees were I E Tamm, A D Sakharov, Ya B Zel'dovich, L D Landau, Yu B Khariton, D I Blokhintsev, and D A Frank-Kamenetskiĭ. Igor' Vasil'evich Kurchatov opened the meeting and chaired it in his typically clear-cut, second-by-second, remarkably purposeful and pressing manner — but without sacrificing delicacy or correctness. After a few introductory remarks by D I Blokhintsev himself, his even younger Obninsk co-workers took the floor, to be followed by V B Adamskiĭ's Arzamas-16 report. From Obninsk, the report on neutron transfer in deuterium, by B B Kadomtsev, attracted most attention, because it is the spatially extended neutron energy release which, along with comptonisation — another subject of Obninsk studies — made detonation unrealistic.

A discussion followed, with I E Tamm the last to speak. In all of the reported schemes, he pointed out, the tube detonation mode, if it at all exists, is limited to a very narrow range of determinant parameters such as the tube diameter. That is, the probability for the detonation mode to occur under tube conditions is very low. This, in his view, was a sufficient proof that the detonation mode is simply nonexistent and so there is actually no need for other variations of parameters to be sorted out. That, he went on, reminded him of the perpetual motion situation, when the French Academy of Sciences decreed the idea to be wrong and refused to consider any specific designs which might arise.

When the meeting was over and most participants were dismissed, after some discussion a decision was taken by the remaining top figures that this line of research had absolutely no future potential — exactly what Americans had realised back in 1950. The liquid hydrogen concept was thereby jettisoned, and the meeting in the Ministry thus was destined to be its — even though first-class — funeral.

As further developments showed, the search focused on using the atomic explosion energy to its full extent to secure the maximum density of the H-bomb thermonuclear fuel, something neither the sloyka nor, less still, the tube could secure. There was a strong theoretical group under Ya B Zel'dovich ready to get on the bandwagon, now free from the work which, although interesting and surely raising high-energy high-temperature skills, had no future ahead. Nor was the sloyka group exactly overbusy. Thus, there was a team well ready to launch a 'brain storm' as soon as a good idea requiring their efforts came along.

The idea of employing the atomic explosion as a means for compressing and subsequently igniting the thermonuclear fuel was one persistently advocated by Viktor Aleksandrovich Davidenko, head of the Institute's Experimental Nuclear Physics Department. A frequent visitor to the theorists, he repeatedly appealed to them, primarily to Zel'dovich and Sakharov, to engage themselves in full earnest in what we then termed the 'atomic implosion' (AI) concept. It is in this connection that on January 14, 1955, Ya B Zel'dovich, with his own hand, wrote a Note to Yu B Khariton, whose covering letter read: 'Enclosed are a preliminary scheme of a Super AI device, and some estimates of its performance. The AI idea is due to V A Davidenko.' (Underlined by Ya B Zel'dovich).

Thus it is clear that Soviet physicists did not need to be prompted as to the importance of the high compression, i.e., of high density, of the thermonuclear fuel for securing its detonation. On the other hand, even though the American Mike explosion, with its high-yield neutron flux, did show that a high density of the thermonuclear fuel had been achieved in the exploded device, still radiochemistry analysis could not in principle give any information about the actual design of the device.

Chronologically, though, the first impetus to translate the fission-compressed thermonuclear fuel from a platonic idea to practice was a suggestion by A P Zavenyagin, deputy Minister of Medium Machine Building, well versed in what was then being discussed among theorists, that one should implode the thermonuclear fuel by an atomic explosion in the same way as by ordinary explosive. The proposal was studied for two weeks or so and then a more sensible idea materialised. Under the new scheme, explosion products and structural material should be used to implode the basic charge. In order that the explosion products not directed straight onto the basic charge also be made to work for implosion, use of a massive casing was provided for, in the hope that the scattering material particles would be partially reflected from it, to contribute to the implosion of the main charge. It took two or three weeks to consider this scheme.

And then, one day, Zel'dovich burst with a eureka cry in the study of two young theorists G M Gandel'man and

V B Adamskiĭ right opposite his own study: "Not this way, no, let us release the radiation out of the ball charge instead!" No later than in a day or two the calculation instructions were sent to Moscow, to Tikhonov's computer centre which then provided services for the Sakharov group, to find out whether radiation actually leaves the atomic charge and exactly how the process depends on the materials used.

The crucial question — indeed the one determining the viability of the idea — was whether or not the surface of the casing absorbs most of the energy released in radiative form: if it does, the remaining energy will be insufficient to implode the charge effectively. The simple and elegant estimates performed by Sakharov demonstrated that even though the losses through the absorption by the casing walls were high, still they were not high enough to prevent the implosion of the basic charge. No less serious was the question of the specific mechanism by which the radiation energy could be used for the effective implosion of the thermonuclear unit. Yu A Trutnev made important suggestions as to how to solve the problem. All these ideas were tried thoroughly in numerous joint discussions which followed.

To gain insight into the physical processes occurring in the new charge, many interesting physical problems were to be solved. If at the atomic weapon development stage neutron physics and gas dynamics (or the hydrodynamics of compressible liquid) were the dominant scientific directions, the work on thermonuclear weapons widened significantly the range of the physical disciplines involved. The high temperatures involved in thermonuclear reactions led to the appearance and development of high-pressure high-temperature physics, a special discipline whose problems seem to have their analogies only in stars and are treated in astrophysics.

With the enthusiasm and highly concerted efforts of the theorists, the work was indeed very much like a brain storm, with all the participants keen on completing their task as soon as possible to get to the test stage. The work inspired the development of a number of computer programmes, to form the basis of the firmware which is now available. The first computer codes were developed, and on their basis first calculations performed, at the Institute of Applied Mathematics, in Moscow. Our mathematical department was then entrusted with auxiliary work. Increasingly, however, as the work on the new thermonuclear charge progressed, efficiency considerations stimulated re-orientation to our mathematical department. Our department was greatly extended, and it was already immediately after the first thermonuclear charge test that it became our main mathematical base, which not only secured computations but, somewhat later, the development of mathematical techniques as well.

There was no place for indifference to the job. This kind of work would lead nowhere. At the executive level, complete self-denial on the part of each and everyone was a *sine qua non*.

In a natural way, a team of work-crazy theoretical physicists was formed. Formally, there were two theoretical departments at VNIIEF at the time, one under Sakharov and the other under Zel'dovich. Actually, there was nothing to separate them by that time. The thought-absorbing side-to-side work brought the people still closer together. With his own particular job to do, every one contributed to the general progress and participated in the discussion of the problem as a whole: an 'all-nation project', Zel'dovich jokingly repeated the then all too familiar catchword for irrigation canals and similar public construction objects which usually demanded an assault-style effort of a large number of people. The management of the project was entrusted to E I Zababakhin, Ya B Zel'dovich, Yu A Romanov, A D Sakharov, and D A Frank-Kamenetskiĭ. The members of the executive group ranged from academicians to the degreeless and included V B Adamskiĭ, V A Aleksandrov, E N Avrorin, Yu N Babaev, B D Bondarenko, M D Churazov, N A Dmitriev, G A Dvorovenko, V P Feodoritov, L P Feoktistov, D A Frank-Kamenetskiĭ, G M Gandel'man, G A Goncharov, V N Klimov, G E Klinishov, B N Kozlov, T D Kuznetsova, I A Kurilov, E S Pavlovskiĭ, N A Popov, E M Rabinovich, V I Ritus, V N Rodigin, Yu A Romanov, A D Sakharov, M P Shumaev, Yu A Trutnev, Yu S Vakhrameev, E I Zababakin, V G Zagrafov, Ya B Zel'dovich.

In his Memoirs, Andrei Dmitrievich Sakharov refers to the idea of using an atomic explosion for imploding thermonuclear fuel (atomic implosion) as the 'Third Idea'. In his words, "Several of us in the theoretical departments came up with the Third Idea at about the same time. I was one of them, and it seems to me that my early understanding of the Third Idea's physical and mathematical aspects, together with the authority I'd acquired, enabled me to play a decisive role in its adoption and implementation. True, Zel'dovich, Yuri Trutnev, and others, undoubtedly made significant contributions, and they may have grasped both the promise and the problems of the Third Idea as well as I did. At that time, in any case, we were all too busy (at least, I was) to worry about who received credit. Any assigning of honours at that time, moreover, would have been 'skinning the bear before it was killed.' Now it is too late to recall who said what during our discussions. And does it really matter that much?" [6].

By the early summer of 1955, computations and theoretical work were completed and a report issued. But is was not until the fall that the preparation of the experimental charge was over. The production requirements were more stringent than before and involved high-precision part manufacture and a special level of purity for some materials.

This experimental thermonuclear charge, which gave a new direction to the domestic thermonuclear charge development, was successfully tested on November 22, 1955. The test had to be conducted with part of the thermonuclear fuel replaced with an inert material to reduce yield in order to secure the safety of the plane and of the residential town about 70 km away from the explosion site.

Thus, the following major stages can be recognised in the course of the work that terminated with development and test of a two-stage thermonuclear charge in November 1955:

(1) Work on development and test of the one-stage thermonuclear charge (sloyka), 1953.

(2) Work on a higher-yield sloyka charge. Dissatisfaction with this design, 1953.

(3) Recognition of the lack of prospect, and cessation, of the theoretical research into the possibility of the stationary detonation of deuterium in a long cylinder (tube), 1954.

(4) Early primitive thermonuclear charge developments using atomic explosion energy for basic charge implosion.

(5) The idea emerges to use radiation, rather than explosion products, to implode the basic charge.

(6) Brain-storm thermonuclear-charge research that terminated with a successful airdrop test of an air-bomb-like charge on November 22, 1955.

From the successful realisation of the idea in these tests to development of serial specimens, there was a bumpy road of special design competition between the Arzamas-16 and

Chelyabinsk-70 Institutes until at this latter Institute (founded in 1955) an armed-force-ready thermonuclear bomb was soon designed. Its main developers were E I Zababakhin, Yu A Romanov and L P Feoktistov.

Somewhat later, a significant improvement in the charge design was proposed by Yu N Babaev and Yu A Trutnev which was successfully refined in 1958 and foreordained the current appearance of the domestic hydrogen charges. According to A D Sakharov, this achievement "was the most important invention which determined the whole subsequent line of works at the facility."

The work on the improvement of the charge design continued, and already a younger generation, disciples of Yakov Borisovich and Andrei Dmitrievich, theorists, mathematicians, and experimenters, developed the modern thermonuclear weapon where new idea and achievement were born no less dramatically. We expect that future publications will present further features of and insights into the history of early Soviet thermonuclear charge development.

The development of Soviet thermonuclear weapons through the research and design effort of A D Sakharov and Ya B Zel'dovich and their teams is perhaps the brightest page in the history of the Soviet atomic project. It is the possession of these weapons both by the Soviet Union and the United States of America which made a war between the two superpowers impossible.

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