

To the Reader



When I tried to express my ideas to explain the mechanism of the so-called cold fusion, I encountered a problem in determining the audience for which it is intended and where it will be understood.

Yes, of course, this audience is included in a community of all the nuclear physicists—theorists, experimenters, and engineers on our planet. It is somewhere around 200,000 people or more. We can call this community group A.

To understand our subject, we also must have a knowledge of physics of elementary particles, the hierarchy of types of interactions (charges)—weak, electromagnetic, and strong interactions, and associated carriers of these interactions (here we exclude gravity). It is commonly called the Weinberg-Salam Standard Model. Our audience is already narrowed to about 10,000 participants. It is not possible to continue the explanation of the cold fusion process without the participation some of these people. We shall call this community group B.

The process of cold fusion is under discussion, beginning about 25 years ago, after the experiments of chemists M. Fleischmann and S. Pons; the anomalous energy release at saturation of deuterium in a metallic palladium was observed. It became clear that after the very first attempts to explain the observed effect and understand the nature of the effect, one has to be familiar with the specific rules of crystalline structure.

The number of crystallographers in the world is high now; the entire semiconductor industry requires very deep understanding of the common physical processes that occur in the crystal. However, if you look at the composition of the necessary 10,000 participants (group B), which was acknowledged in the previous section, it appears that relatively a few of them have any knowledge or interest in the processes in crystals. Of course, we can remind the Mossbauer effect, which is of interest to both particle physicists and crystallographers. However, our group of scientists of interest is dramatically reduced. We call this community group C. We believe that approximately 1,000 participants of our interest are left in this group.

As it turns out, chemical catalysis is directly involved in these processes. One needs to know the chemistry of this phenomenon and understand how it may affect the process, such as obviously nuclear fusion. Physics (chemistry) of electron shells of admixture deuterium atoms in conducting crystals? Group D consists of approximately 100 participants.

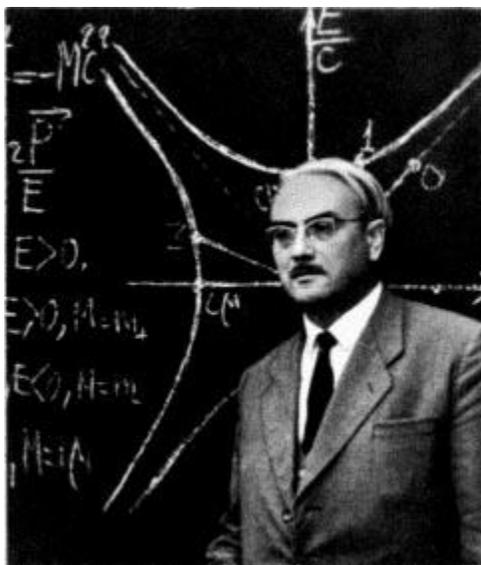
In terms of the foundations of quantum mechanics and the uncertainty relation $\Delta E \times \Delta t \geq \hbar$, one may ask: Why we need it here? However, without a transfer of excess nuclear energy in the crystal lattice, the cold fusion process is simply forbidden by the well-known laws of conservation. How many people from group D would pay attention to this?

Under the normal conditions of the development of science, all of these selections, A, B, C and D would be overcome in the reasonably foreseeable future. However, learning the secrets of the cold-fusion process inevitably would create fundamental changes in the world energy industry, mainly in the oil and gas industries. Distrust of the cold-fusion process is felt in the traditional nuclear power industry, which is gradually strengthening its position now, despite the obvious hazard of this direction. Namely, these social circumstances are the main difficulties for the development of cold nuclear fusion. At the moment, it is difficult to predict how long this unspoken taboo will last. We hope that this process will be short-lived.

Development of the idea of cold fusion

In 1950, when I was interviewed for admission to the Physical-Technical Department of Moscow University, a professor asked me, “Why do you want to enter on the structure of matter specialty?” I replied, “I want to make a bomb.” The professor laughed. It seems to me that this post-war feeling that was close to the oath, “Nobody will see us weak anymore” will never disappear. Of course, for more than half a century, the sharpness of this feeling slowly decreased but can never be gone completely. “Not forget at that time, not forgive, and do not neglect...” Vladimir Vysotsky.

During the third year of my education in the Physics Department of Moscow State University, I realized that our group is prepared to work at Russian nuclear facilities. Our undergraduate practice was held at the Department of Professor I.M. Frank at the Physical Institute of Academy of Science. We measured the distribution of neutrons in a size of two floors, large graphite cube. Subsequently, I began a practical acquaintance with nuclear physics. My examiner of 1950 neatly fulfilled my wish. However, during this time, the main development of nuclear weapons was almost over. Further progress in mastering the nuclear laws was promised by the high-energy physics. So I found myself in Dubna at the Joint Institute for Nuclear Research in 1956. I never regretted this. However, oddly enough, the basics of nuclear physics happen to be useful for me quite recently. “Just how old but powerful weapon ...” And just as suddenly, after 50-plus years, I suddenly recall in detail also quantum electrodynamics, which was enthusiastically taught to us by Ya.P. Terletsky.



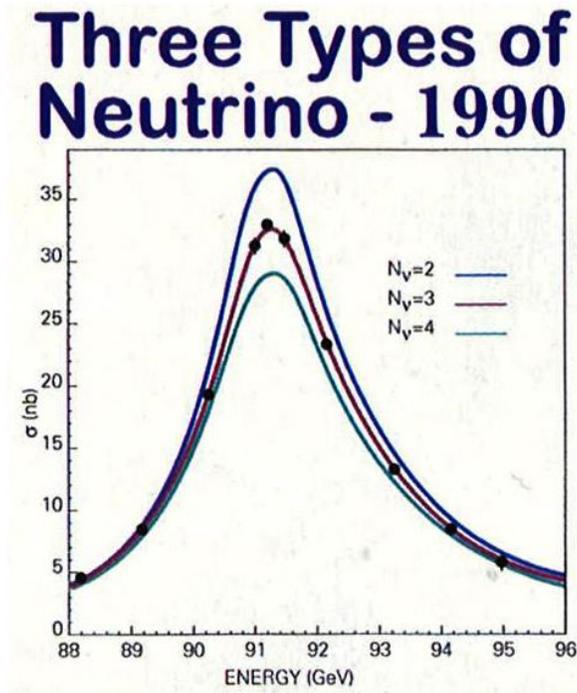
Ya.P. Terletsky

I'm proud of my pioneering experiments on the measurement of electromagnetic sizes π^- and K^- mesons in the scattering of these particles from hydrogen electrons at the Serpukhov and Fermilab. These experiments started in 1970 and were the first Soviet-American cooperation in science. I have reason to think that without me, these experiments would not take place. In 1979, I reported the results of a series of these experiments at the Conference on High-Energy Physics in Tokyo. It was, as it could say, my finest hour.



Speech at the conference in Tokyo

In the DELPHI experiment at the European Organization for Nuclear Research (CERN), which I joined from 1984 to 1992 (attended by approximately 500 physicists), we significantly pushed the limits of the non-point behavior of quarks and leptons. In this experiment, I took part in determining the number of neutrino species (i.e. determining the number of families of fermions). If suddenly it turned out that the number of types of neutrino is four, rather than three, the theory of physics would be in trouble.



One of the main results of the LEP complex at CERN

In each experiment, there are critical and emotional aspects. One of the very emotional aspects in my practice happens to be experiments with bent crystals. Crystal... A little more on this.

In early 1976, our group was engaged in the preparation of Fermilab drift chambers, produced in Dubna for Fermilab experiments on the scattering of π^- and K^- mesons by electrons in a beam with energy of 250 GeV. We were advised by Dr. Fabio Sauli of CERN, who came to the Fermilab for a short time. At this time he, together with Georges Charpak (Nobel Prize in Physics laureate), participated in the studies of high-energy particles channeling in crystals. Sauli enthusiastically talked about these experiments.

The intriguing processes of correlated interactions of high-energy particles with atoms in the crystal captivated me for some time. The behavior of a particle in the potential well between the crystal planes is very recalling of the light path in the planar waveguide. This similarity was immediately tempted to bend the crystal to control the trajectories of charged particles.

Calculations showed that this method of particle deflection will be much more effective than the deflection of particles by magnetic fields, due to the very strong electric fields inside of the atom. One particular observation is that the higher the energy of the particles, the more effective the method.



Argonne National Laboratory, 1978. Author of the memoirs is first left in the second row.



Fermilab, 1978.



The signing of agreements on cooperation in the field of nuclear energy between the USSR and the USA, 1978. Photo by the author.

I remember that Sauli took my suggestion with skepticism. He told me that uncontrolled bends of crystal foil in their experiment were causing them a lot of troubles, complicating the precise orientation.

Sauli's pessimism towards my idea to redirect particles with bent crystals was really surprising and discouraged me; it seemed to be an obvious idea to me. Shortly thereafter, I was approached by a Fermilab physicist, Dick Carrigan, who suggested the use of our spectrometer drift chambers for the experimental study of channeling π^- mesons with energy of 250 GeV in the crystals. Our spectrometer with drift chambers from Dubna at this time had the best spatial and angular resolutions. I agreed and asked him to extend this experiment, adding to the goals the deflection of the beam by bent crystal. After thinking for a few days, Dick said that he personally does not believe in this proposal, and he advised to discuss this in the laboratory management, collecting a special commission of experts.

Two weeks later, in the summer of 1976, a panel of six experts developed, in which I presented my ideas and gave quantitative estimates. The panel included well-known experts in channeling, particularly Professor D. Gemmell from the Argonne National Laboratory. He is the author of a wonderful review of the channeling process, and I particularly relied on his support. About an hour after my speech, the commission announced that it did not find my arguments to be convincing and did not recommend a proposed experiment.

Now, it is hard to express my reaction to this decision. It was something like, "Yeah, they are just idiots! How is that possible?"

I decided to publish my ideas. I sent the manuscript to the journal *Physical Review Letters*. There was a pretty fast response—a polite refusal, followed by two negative (anonymous)

reviews. I wrote a “very angry” letter to the editor with a request to find more qualified reviewers. About one month later, I received a review from Professor Alexei Maradudin at the University of California at Irvine; Maradudin is a famous physicist in the field of channeling. He reported that the refereed article is unscientific fakes of the author; the manuscript is of no value and cannot be accepted for publication. The review sounds something like this: it cannot happen, because it is pure nonsense.

After reading the review of Maradudin, I decided it is necessary by all means to carry out the experiment on the accelerator. No other arguments can convince physicists to engage in the processes of channeling this science, which emerged in the study of phenomena at very low energies. Crystalline physicists present this as something immutable, once and for all time. It was a taboo, imposed on a bent crystal. It was necessary to present the conclusive experimental facts. I was sure I was right, and I wanted to punish by the facts so-called classics.

It is now clear that this case is similar to the situation that is happening now with the phenomenon of cold fusion. We can discuss this in greater detail later.

At JINR things did not go very smoothly at first. After a two-year trip to the United States, the group almost broke, the premises “sailed”, and nearly all the equipment “dissolved”. The remaining few people fell on the task of processing the experimental data on the scattering of π^- and K^- -mesons by electrons in a beam with energy of 250 GeV performed at Fermilab. It was necessary to collect a new team for the channeling experiment.

The experiment with bent crystals was supported at the very beginning by the JINR Director N.N. Bogolyubov, an outstanding physicist of the 20th century. As the director of the Laboratory of High Energy A.M. Baldin told me, his friends from the Moscow Institute of Physics of Crystallography believed that the experiment is doomed to fail. For me, it was not surprising. In September 1977, the CERN Courier journal published an editorial that literally stated, “Some propose to deflect beams using bent crystal, but they just do not understand the process of channeling.” This was a clear answer to my technical memorandum on the subject, which I offered to write by the director Fermilab R.R. Wilson.

Before the beginning of the experiment in autumn 1978, as I could say, a “summit meeting” under the chairmanship of A.M. Baldin was held in his office. At this time, Moscow State University hosted an international conference on channeling, and a group of participants arrived in Dubna. There was a meeting in Baldin’s office with the founder of the channeling theory Professor I. Lindkhard, the recognized head of the experiments with crystals at high energies, Professor E. Uggerhoy, Professor A.F. Tulinov from MSU, and other “authorities”. The possibilities of success for our experiment were discussed. The verdict of the meeting was very categorical; bent crystal will not keep the channeled particles.

Nevertheless, due to the persistence of N.N. Bogolyubov, JINR directorate decided to conduct this experiment. It was necessary to determine the location of the spectrometer on the beam of the synchrotron. Several options had to be discarded – too crowded. Furthermore, the LHE chief engineer Leonid G. Makarov offered for us to stay in the unfinished building: “Well, nothing terrible that it is not finished. Electricity we’ll throw, we will send the beam to

you, and you do not need anything more. But look how much is space – hectare under the roof! Roof is reliable, windows we are glazing. No crane – your equipment you care by hands. No heating – electronics only will work better. Go for it! And at the same time we will report that the building conveys the physics in operation.”

The idea was good. In November 1978, we passed up the system. In December, we ran the first session on the extracted proton beam. The beam with energy of 8.4 GeV was not yet fully formed because the hall 205 has no magnetic elements; there was no water, but this was not so important. The more substantial difficulty turned out to be the cold. Outside it was minus 20 centigrade with the wind. To work in the experimental portacamp, we had to put it in 11 oil heating batteries, the only ones that we could find in LHE. We also had to heat (incubate) our drift chambers. We had to perform the detector installation in winter conditions. In the experimental hall, it was also minus 20 centigrade, but the truth is there was no wind.

I must say that the experiment happen to be not as simple as it seemed at first. Beam that was detected by the system of scintillation counters have to be necessary put in a crystal with accuracy better than tenths of a millimeter. Semiconductor detector, built at the entrance of the crystal, allows us to select particles trapped in the process of channeling by the low value of ionization losses. Precision goniometer allowed orientation of the crystal in two dimensions with an accuracy of 0.001 degrees.



First results of the experiment on beam deflection in LHE

We bend a crystal of two-millimeter thickness by using a rather primitive device, a screwdriver. Using a laser beam, we bent the crystal up to 0.5 mrad – and beam deflected correspondingly! 1 mrad – go! 3 mrad – go! 4 mrad! Victory! When we tried to set the bending angle 6 at milliradians, the sample broke. We used a thin crystal, bent it to 12 mrad, and it broke again. We took a thinner crystal and bent the angle to 26 mrad. 26 mrad bend is OK! The magnetic field that is required for such deflection is about 80 Tesla.

When we tried to increase the bending angle, this sample also broke. We rearranged the apparatus for detecting the angle of the 45 mrad, placed the last, thinnest 0.5 mm crystal. Suddenly, there was a shot of thunder, and everything stopped. Lightning hit the cable that connected us with the ES-1040 computer in the physical building of the LHE. This cable went straight through the trees. Outside, it appears, was already a summer, June 1979. Our communication equipment with the computer was burnt. Then, it was morning. No, it is not about Scheherazade; it ended the session with the accelerator. Every physicist is familiar with this feeling of the end of the séance, which is very similar to the feeling of rough and premature parting. However, the séance ended with our convincing and complete victory.



After the session ended

Shortly after the publication of the results of our experiment, the deflection of particles bent crystals was confirmed at CERN, Gatchina, then at Fermilab. Many interesting proposals

appeared for the use of bent crystals in high-energy physics. The so-called crystal optics appeared—deflecting elements, focusing elements, devices for beam splitting into several directions, etc. As said about us by A.M. Baldin at the time: “They can whisk the beam!”

I must say that I really do not want to “bounce to the side” of high-energy physics. The era of the colliding beams has begun, and the standard model of elementary particles has made the first steps with the opportunity “to loosen up the train.” The superconducting Tevatron colliding beams complex was actively developing at Fermilab, the installation, which later received the name of CDF, was taking the first steps. High-energy physics was calling us back. As part of my efforts to switch the experimental cooperation of JINR with CERN from fixed targets to colliding beams physics, my “lobbying” of the colliding beams physics came to fruition.



Proposal of JINR participation in the DELPHI experiment.

In 1984, our group already intensively prepared the DELPHI experiment with colliding electron-positron beams on accelerating-storage complex LEP. However, another “bounce” in the direction of the bent crystal had taken place. One of the tempting proposals involved beam extraction from accelerators, using bent crystal. Over the past five years, no one was going to implement this proposal. It was time to do it ourselves.

I must say that the LHE synchrotron represented the accelerator, which is not the easiest in demonstrating this extraction. There was only one place in the second quadrant of the accelerator, where on the inner movable holder the target with bent crystal would be placed. The small outlet from the accelerator vacuum chamber was spaced from the deflecting crystal at a distance of about 50 meters. To put the beam into this hole, you had to cut and bend the crystal with extremely high precision. The situation was aggravated by the fact that the position of the

target outlet of the accelerator vacuum chamber, in general, was not accurately known. Fine adjustments can be made to the trajectory of the deflected beam on the way to this outlet deflector crystal inside the chamber of the accelerator, which can be remotely moved and rotated using the mini-goniometer.

I cannot describe all of the details of these studies on the accelerator. The time on the accelerator was given to us in a fierce competition with interesting physical LHE tasks, as a chance for the first time to try to extract a beam from the Synchrotron using bent crystal. I must say that the group brilliantly coped with this task. There was the first beam from the accelerator using bent crystal!

There is one comic (at least for me) detail. At this time, the State Committee for Inventions and Discoveries of the USSR held consideration of our proposal for the discovery of the phenomenon of deflection of charged high energy particles using bent crystals. The verdict was: refuse, as these results do not contain any element of novelty; this is probably true. Still, this somehow reminded me of satire by Kozma Prutkov: "If you will be asked: what is more useful, the sun or the moon? Respond: the moon. Because the sun shines during the day, when there is already light, but the moon shines at night."

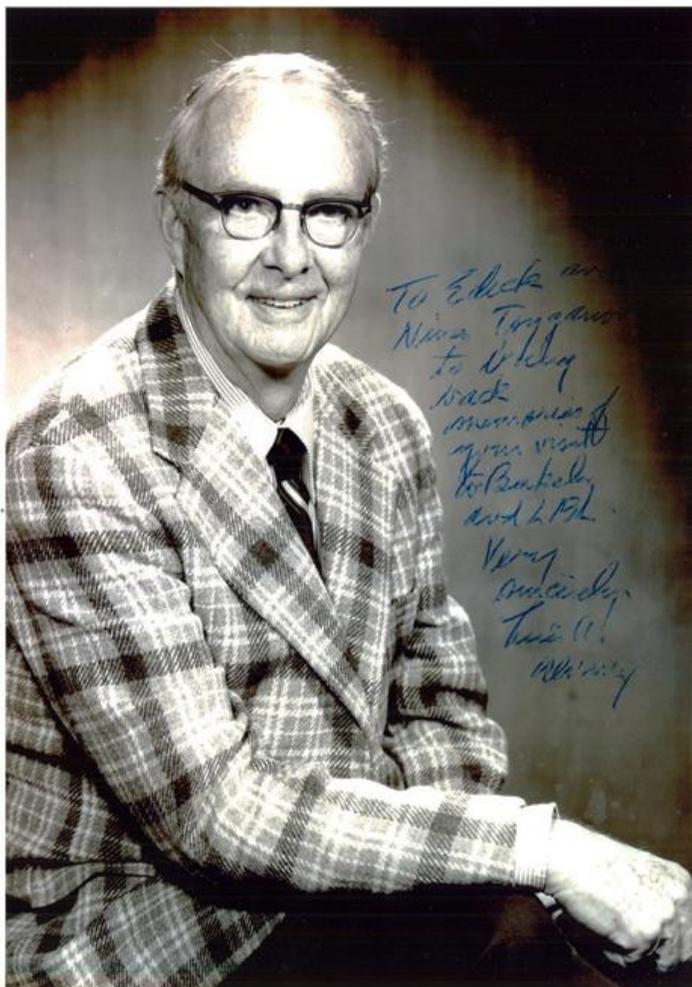
At the Conference on High Energy Physics in Dallas in 1992, CERN Director Professor C. Rubbia reported the beam extraction from the SPS accelerator, using bent crystal, as a main result of CERN for the year. I remember that when the audience of 2,000 physicists applauded Rubbia, I felt like the hero of the day, though no one paid attention to me.

How it was happen that I again taken out of the high energy physics to this applied research called the cold fusion? However, this applied research promised a jump in the generation of energy by the factor of 10^7 (!), compared to the all-chemical processes. This breathes new life on our planet! We have enough cheap energy in excess for billions of years!

I was fortunate to attend the seminar of M. Fleischmann at CERN in December 1989. We were commissioning the DELPHI experiment on LEP at this time. I believed in Fleischmann's results immediately. During this seminar by Fleischmann, I decided that I would try to understand this cold fusion, when I finish with the DELPHI experiment. Back then, I considered myself to be a great expert in crystals, due to the recent discovery of the deflection of high-energy beams, using bent crystals. I promised myself to clarify this cold fusion process indeed.

My meetings with the legendary physicist Luis Alvarez in 1976, during my visit to the Lawrence Laboratory in Berkeley, came to my memory. Alvarez told me about the first experiments on μ - catalysis reactions with DH-fusion, discovered by his group in 1957 in the liquid hydrogen bubble chamber. I have already heard about the nuclear μ - catalysis. Apparently, I believed that this way will not lead to the practical application of cold fusion because of technical difficulties in obtaining beams of μ -mesons that are intense enough.

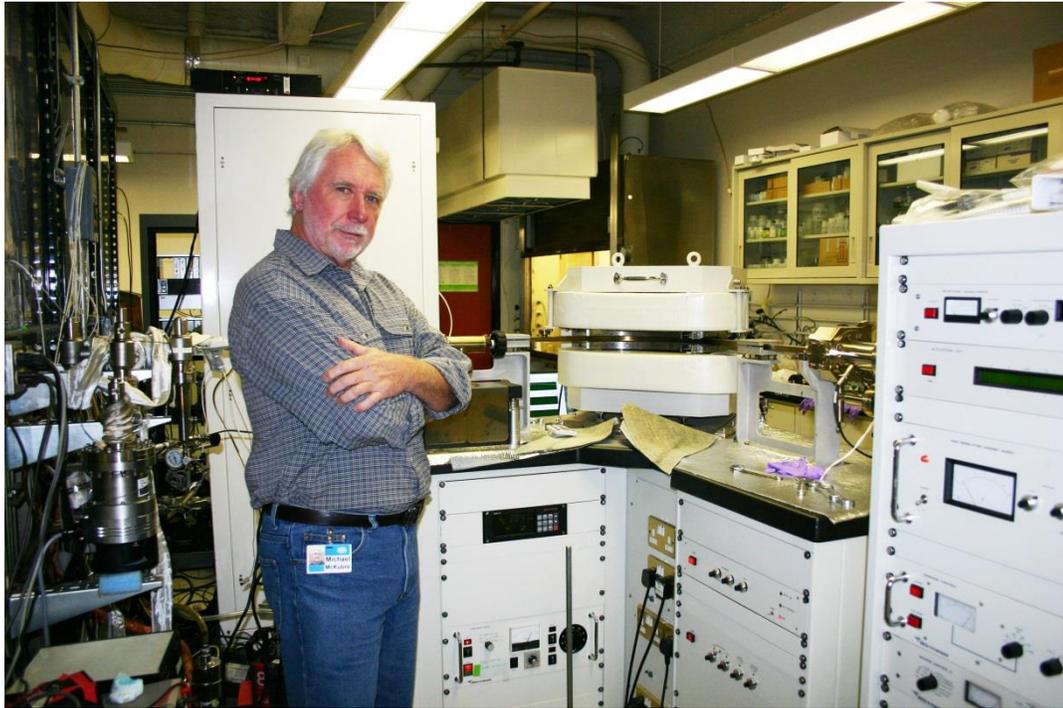
Here again are the ideas of cold fusion, but this time in crystals!



Luis Alvarez

Immediately after Fleischmann's seminar at CERN, I could not imagine that in order to understand a cold nuclear fusion, it would take me more than 20 years. Shortly after the 1989 events rolled in so quickly that only a few years ago I was able to return to the issue.

In early 2011, I met with Dr. McKubre of the International Research Institute at Stanford in his laboratory. I am convinced that the work performed by a group of McKubre satisfies all of the stringent requirements of the physical experiment. The observed effect comes out on about 100 experimental errors.



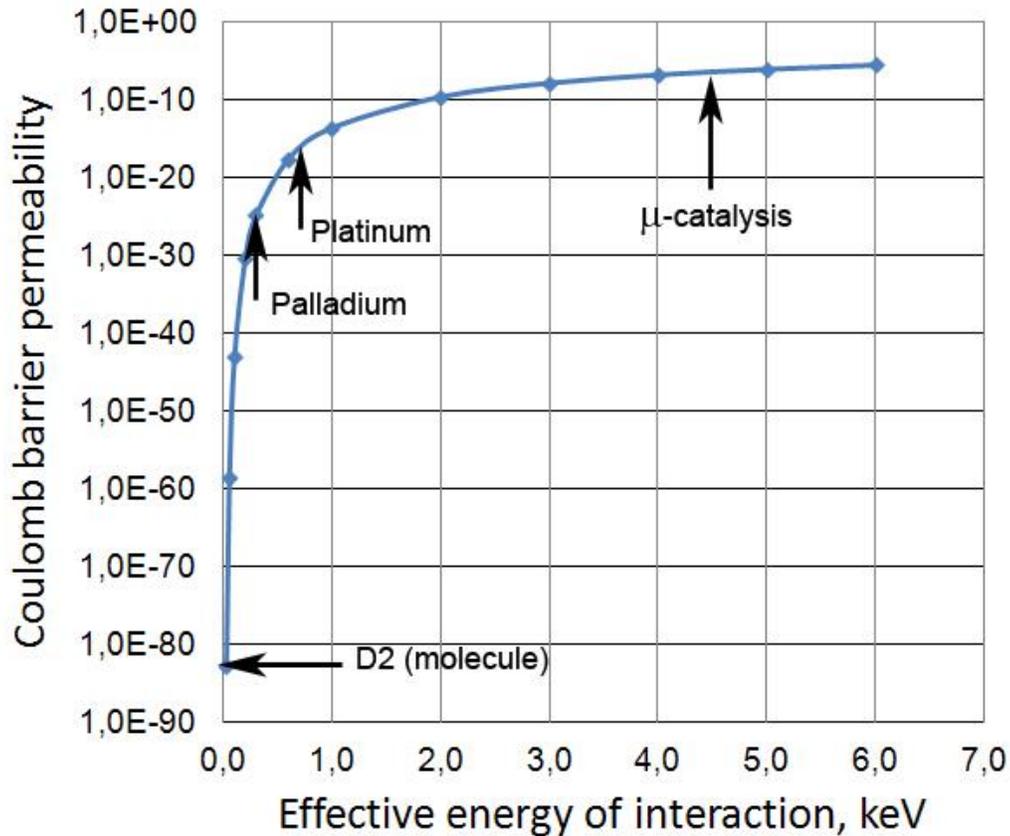
Dr. McKubre in his laboratory. Photo by the author.

My attempts to explain the observed effect of cold fusion for a long time did not bring the desired results. The experiments of the McKubre group could somehow still be explained with the “hot” fusion mechanism, supposing that a mechanism of pumping of long-wavelength lattice vibrations to individual atoms of deuterium implanted into the crystal, by the “domino” effect in the hyper-channels of the lattice; the experiments of a Arata group in Japan with palladium microcrystals did not fit into this mechanism.

A breakthrough in my understanding of the mechanism of cold fusion occurred after my acquaintance with the experiments on traditional “hot” fusion at low-energy accelerators by Rolfs’ group (LUNA collaboration) and Czerski in Berlin. As it turned out, similar experiments were carried out before by their Japanese counterparts. It was noticed that in these experiments, the so-called electronic screening potential, which in the case of free deuterium atom collisions equals to 27 eV for the reaction of the DD fusion in conductive crystal environment, could reach the value of 300-700 eV. The electron screening potential is a measure of the distance to which the deuterium atoms can approach each other without Coulomb repulsion.

Accelerator experiments have shown that under conditions in conducting crystal DD fusion takes place by such a way that the deuterium atoms can approach each other without experiencing Coulomb repulsion to a distance of 1/10 or 1/20 of the nominal size of the deuterium atom. In the papers of Assenbaum and other theorists, it has been shown that the electron screening potential can be treated as an additional kinetic energy of the particle. If we

apply this interpretation (it is presumably valid if the additional potential is much smaller than the amplitude of the potential barrier), then for the two deuterium atoms, located in the same potential niche of a conducting crystal, the transparency of the barrier would be as follows:



Transparency of the potential barrier for DD fusion.

I cannot express my emotions after I drew this chart! In these calculations, there is nothing but absolutely reliable quantum mechanics and well-known parameters (sizes) of the deuterium nucleus. The transparency of the potential barrier for the two deuterium atoms, located in one potential niche of conducting crystal, is increased by ~60 orders of magnitude, compared to the free molecule of deuterium! Archimedes, in a similar situation (“Eureka!”), ran out of the bath naked. Our task was noticeable and more difficult than that of Archimedes. My hands were trembling mildly for several hours.

The first barrier in the explanation of cold fusion collapsed.

Opponents could immediately ask, “But where are the neutrons? Where is the tritium? These products arise during DD fusion.” We have to assume that at low excitation energy of the compound nucleus ${}^4\text{He}^*$ (total thermal energy of the initial interacting deuterons) lifetime constant of nuclear decay modes of composite intermediate state ${}^4\text{He}^*$ increases, slowing it decays. We need to assume that state ${}^4\text{He}^*$ in cold fusion became metastable. The answers of

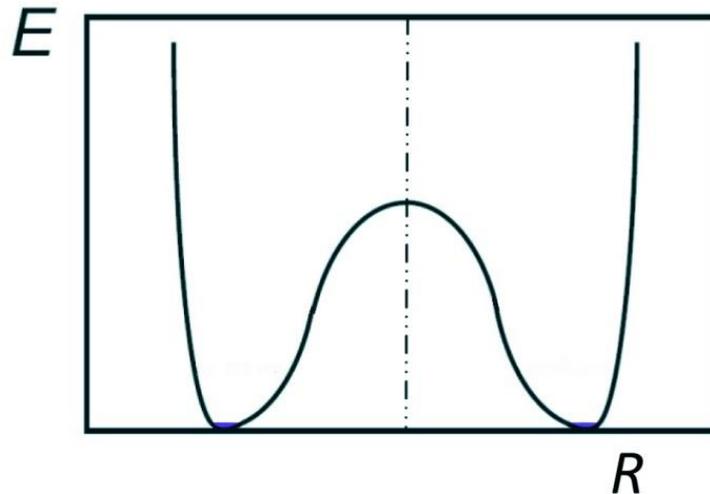
opponents: “Do not worry; we’ll wait for the intermediate nucleus decaying! How much we have to wait? And what then?” You have to wait until virtual photons emitted by the intermediate compound nucleus ${}^4\text{He}^*$, flew to the lattice atoms and gave them out all the excess energy of ${}^4\text{He}^*$, which is 24 MeV. This time is no more than 10^{-15} seconds. The objection of opponents: “So you are caught, finally! The photon emission for nucleus ${}^4\text{He}^*$ is not possible, since the spin of the photon is equal to one, and the orbital angular momentum of the resulting compound nucleus ${}^4\text{He}^*$ equal to zero!” Our answer: You are right in the case of real photons. However, our photon is virtual, and the space component of its spin is zero, when the spin is directed along the axis of time. “And how long does your virtual photon exist?” This depends on its energy because $\Delta E \times \Delta t \geq \hbar$. 10^{-18} seconds is enough for the virtual photon to reach the boundary of the atom, meet the electron there and do not come back.

The opponent pauses to reflect...

In June 2011, I attended and made a presentation about cold fusion for a workshop at MIT, under the direction of M. Swartz. The PDF file of my report at the MIT workshop is presented on this website. My presentation was listened to with attention. There were a few questions. I remember one of them: “Tell me, please, why you think that the traditional modes of decay of the intermediate compound nucleus ${}^4\text{He}^*$ may be prohibited under cold fusion? This would contradict the theory of nuclear physics! Where does this ban come from?” My answer was: first, the mutual Coulomb repulsion of the nuclei of deuterium, penetrated in the potential well of the strong interactions, is not going away. This repulsion, in my opinion, is the reason for the formation of a metastable state of ${}^4\text{He}^*$ at low excitation energies of composite intermediate nucleus in cold fusion. This is not a ban; it is just the slowing down of such decays. Secondly, with all of my deep respect to nuclear physics, I believe that the theory of nuclear physics does not exist yet. What you call a theory is today no more than a set of phenomenological rules of thumb.

In my opinion, the reaction of the audience to my answer was quite ambiguous. “Is the assertion not too firm? Is it not too far from the objective?”

The figure below illustrates very schematically the situation that arises in the first moments after the penetration of the deuterons of thermal energy in the potential well of strong interactions. Coulomb barrier height between the two deuterons may be of fraction of keV, and their total kinetic (thermal) energy is about 0.040 eV, i.e., by 3-4 orders of magnitude less than that of the barrier. This, apparently, is the cause of the metastable state of the system, which slows down its nuclear decay to 10^{-15} - 10^{-16} seconds. This is sufficient, if using virtual photons, to emit an extra energy of 24 MeV in the reaction $\text{D}+\text{D}\rightarrow{}^4\text{He}+24\text{ MeV}$.



Scheme of the potential well for DD cold fusion.

One can assume that the Coulomb potential barrier within a common well of the strong interactions is no longer a factor in holding neutrons, and neutrons can “almost freely” move from one deuteron to another in the nuclear well. In this case, the metastable dd-system transfers into a metastable pt-system. However, this “almost freely” costs about additional 2 MeV due to binding energy of deuteron. Remember, in our case kinetic energy of deuterons in the well of the strong interactions is very low, some tiny fraction of eV. These processes, of course, need to be considered in more detail quantitatively.

The virtual absence of neutrons in a cold fusion reaction $D+D \rightarrow {}^3\text{He}+n$ is a proven experimental fact. The experimental fact that is also recorded is that there is some output of tritium from cold fusion reactions $DD \rightarrow {}^3\text{H}+p$, although this output is very small.

The cold fusion ideas, based on calculations of this effect, using the potential of the electron screening in metals, formed the basis of the materials presented on this webpage. These include reports by me and my colleagues in 2011-2013 (MIT, Stavropol, Channeling 2012 Conference, Joint Institute for Nuclear Research in Dubna, Russia, Russian-American Scientific Society Conference (RASA) 8-10 November 2013, Clearwater Beach, Fl.), as well as our publications in this period (Nuovo Cimento 2011, Frascati preprint 2011, Journal of Nuclear Physics 2012, XI International Conference “Solid State Chemistry: nano-materials and nanotechnologies” – Stavropol, Russia, 2012, NIM B 2013, Engineering Physics, Moscow 2013, Channeling 2012 – a collection of MEPhI, 2013).



After the meeting on cold fusion at MIT in June 2011. Photo by the author.

When summarizing this discussion of cold fusion, you can consider the following conclusion:

Currently, humanity has come to a stage of development in which the struggle for energy resources is becoming especially important, since all known energy sources in the near future will not be able to fulfill our needs. Moreover, chemical energy is limited to the so-called greenhouse effect. Nuclear energy is based on the use of fissile materials, which is also not a solution, because the stocks of these materials are limited. Initial expectations about the controlled thermonuclear fusion process have not yet materialized. Technical difficulties in obtaining sustainable superhot plasma and the damaging effects of the enormous neutron flux arise as a result of thermonuclear reactions, which pushes this task to the more distant and uncertain future.

Recently, the belief that the problem of controlled nuclear fusion can be solved in a completely different way has developed. It has been shown experimentally that the cross section of sub-barrier fusion processes is highly dependent on the physical state of matter, in which reacting atoms are placed. The approach distance of two deuterium nuclei with the Rydberg mechanism in the crystal cell of metals is an order of magnitude smaller than the size of the free atom of deuterium. Coulomb barrier permeability in such a process very much increase DD fusion (by ~ 60 orders of magnitude), compared to the permeability of the free molecule of deuterium.

We are currently discussing the possibility of experimental detection of the “cold” DD fusion detection, using the registration of low-energy electrons, which are the result of the fusion reaction of two deuterons in palladium crystals at very low (thermal) excitation energies of the intermediate compound nucleus ${}^4\text{He}^*$. This type of process is possible through the exchange of the compound nucleus with electrons of the crystal lattice by virtual photons.

The details of our research on cold fusion are presented on this website.

E.N. Tsyganov