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A few comments on superconductivity research

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Abstract. The content of this note was presented at the opening, on 18 October 2004, of the Fundamental Problems in High-Temperature Superconductivity conference near Moscow. For coverage of conference issues see the paper by B I Belyavskiĭ and Yu V Kopaev in this issue.

Superconductivity was discovered in 1911 in Leiden, which was preceded by obtaining liquid helium in 1908. This event can be thought of as the origination of low-temperature physics, although low-temperature studies had certainly begun before that. It would be out of place to dwell here on the history of the development of low-temperature physics and, in particular, the study of superconductivity. Some information on the subject can be found in my paper [1] and paper 6 in book [2]. Now I would only like to stress the fact that in an example of the study of superconductivity one can clearly see how radical the changes in science have been over the past century. One can even say that this has all happened within only a century, that is, within a time comparable with the human lifetime and very small, say, compared to the period after the golden age of science in ancient Greece (2 – 3 thousand years ago), to say nothing of the age of *homo sapiens* (50 – 100 thousand years).

But at the beginning of the last century the pace of the development of physics and science as a whole was incomparable to what we face today. Suffice it to say that before 1923, i.e., for 15 long years (on a contemporary scale) liquid helium had been produced in Leiden only, and in that period only a few dozen studies had been performed in the helium temperature range. Within the ten years after the discovery of high-temperature superconductivity (HTSC) in 1986 – 1987, nearly 50,000 publications were devoted to this subject, that is, 10 – 15 reports appeared every day.

The stile of research also changed rather radically. For example, we have learned only recently [3] that the superconducting transition was first observed quite clearly and definitely by Gilles Holst, who conducted measurements at the Leiden laboratory. Holst was a qualified physicist (later, the first director of the Philips Research Laboratories and Professor of Leiden University). Kamerlingh Onnes, however, did not even mention his name in his paper [4], where he reported those measurements. I cannot imagine anything like this happening now in a civilized country (though this is a disputable question, may be I am wrong). At that time, in the early XXth century, this was obviously the norm in German and congenial universities ('Herr Professor', head of research, could be considered to be its only author). Such a conclusion seems to me to be well-grounded because, as mentioned in [3], Holst himself had not apparently thought of such a slight by Kamerlingh Onnes as being unjust or unusual. The times were different, and to avoid misunderstanding I will stress that I have no grounds to cast aspersions on Kamerlingh Onnes and his undoubted achievements (which are described in more detail in [1, 2]).

In the 1930s, when I myself began working, low-temperature physics had already occupied an important place in physics in the whole world and especially in the USSR. In our country, as far as I can judge, this was primarily associated with the activities of L V Shubnikov (1901 – 1937). He graduated from the Leningrad Polytechnical Institute in 1926 and then worked for several years in the Leiden cryogenic laboratory, where he carried out a number of world-famous research works (suffice it to mention the Shubnikov — de Gaas effect), and from 1931 up to his untimely demise in 1937 (more precisely, up to imprisonment a few months before) he was head of the Cryogenic Laboratory in Kharkov Physicotechnical Institute[1]. There he obtained liquid hydrogen already in 1931 and liquid helium — for the first time in the USSR — in 1932; liquid helium was only available in a few laboratories in the world (to the best of my knowledge, the second liquefier began operating in 1923 in Toronto and W Meissner put into operation the helium liquefier in 1925 in Germany). Shubnikov and his students and colleagues accomplished a lot within only a few years, and I should specially mention his studies of superconducting alloys and a factual discovery of type II superconductors (these studies are cited in [6, 7]; see also [5]). I am sure that Shubnikov would have achieved even greater success in science, and one cannot but feel bitterness about his untimely (at the age of only 36!) and quite guiltless death under the ax of Stalin's terror.

Along with the studies by Shubnikov and his school, research work in the field of low-temperature physics began in the 1930s in Moscow at the Institute of Physical Problems of the USSR Academy of Science. At that institute, in 1938 and a little earlier and up to the beginning of war in 1941

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[1] In reference book [5] one can read that L V Shubnikov died in 1945. In the Soviet times I happened to hear this false tale, too. But after the break-up of the USSR some materials were declassified and we learned that Shubnikov with some of his colleagues was shot as far back as 1937, soon after his arrest. He was, of course, fully rehabilitated posthumously. L D Landau was prosecuted for the same 'case', but he managed to leave for Moscow and was arrested only in 1938. He escaped death literally by a miracle (he was released in 1939; for more details see, e.g., [2], paper 10).

P L Kapitza investigated superfluidity of helium II[2] and in 1940 – 1941 L D Landau formulated his theory of superfluidity [12]. Both in the prewar years and after the war many interesting things were done in the USSR in low-temperature physics, but it is hardly pertinent to mention them here (I believe, on the contrary, that it would be most appropriate to devote a special paper or a monograph to this subject).

It seems to me that low-temperature physics occupies in a sense an especially important place in the physical studies that were carried out in the USSR. Suffice it to say, I think, that Soviet (Russian) physicists received six Nobel prizes in physics, of which half concerned low-temperature physics. In 1962 Landau received a prize "for the pioneering theories for condensed matter, especially liquid helium." In 1978 Kapitza received a prize (more precisely, half of the prize) "for his basic inventions and discoveries in the area of low-temperature physics." And, finally, in 2003 A A Abrikosov, A Leggett, and I were given an awarded "for pioneering contributions to the theory of superconductors and superfluids" [13 – 15]. The other three prizes were given in 1958 to I E Tamm, I M Frank, and P A Cherenkov for the discovery and explanation of the Vavilov – Cherenkov effect, in 1964 to N G Basov and A M Prokhorov (half of the prize) for studies in the field of quantum electronics and, finally, in 2000 to Zh I Alferov (part of the prize) for information and communication technology. Incidentally, I am far from attaching too much importance to Nobel prizes, as is now done by mass media. I have always been of this opinion, but before I received the Nobel prize I had not placed emphasis on it for I would have been suspected of envy and so on. This is in fact quite clear for one who is aware of the conditions for awarding the prize and knows who was awarded it and who was not (for some more details see paper 21 in [2] and note [16]). At the same time, Nobel prizes are indicative of the state of a corresponding science in the country. And therefore I believe that what has been said above shows the particularly high rank of low-temperature physics among the physical research works carried out in our country. I allow myself to accentuate this fact here because from it I shall draw some conclusions about the prospects of the development of physics in Russia. I shall return to this at the end of the note.

Since high-temperature superconductors were obtained [17, 18] in 1986 – 1987 it is naturally their study that has been at the center of attention in the area of low-temperature physics. Almost 20 years have passed since that time, but in spite of the numerous studies, the mechanism of HTSC in cuprates is not yet clear enough, even at the level of understanding. This situation was elucidated in a number of reviews [19 – 21]. It was popular for a long time to think of superconductivity in HTSC cuprates as being induced by some exotic mechanisms and, at any rate, as being considerably different from superconductivity in 'conventional' or low-temperature type I superconductors, for which it is well known that the BCS theory [22] was successfully applied. In this theory, in the simplest case and for 'weak coupling', the critical temperature T_c of superconducting transition is determined by the expression

$$T_{\mathrm{c}} = \theta \exp\left(-\frac{1}{\lambda}\right), \tag{1}$$

where $k_B\theta$ is the energy range near a Fermi surface, in which conduction electrons are mutually attracted and λ is the coupling constant which in the weak coupling case is small, i.e.,

$$\lambda \ll 1. \tag{2}$$

If the interelectron (quasi-particle) attraction is due to the phonon mechanism (the virtual exchange of phonons), then

$$\theta \sim \theta_{\mathrm{D}}, \tag{3}$$

where θ_D is the Debye temperature of metal. The quantity $k_B\theta_D$ is known to be on the order of the maximum energy of the participating phonons. Normally we have $\theta_D < 10^3$ K with the exception of, say, metallic hydrogen. Hence, it becomes immediately clear why in ordinary metals for electron – phonon coupling with $\lambda < 1$

$$T_{\mathrm{c}} \lesssim 30-40\ \mathrm{K}. \tag{4}$$

These simple arguments are well known and have recently been repeated, for example, in [14].

If one assumes materials with $T_c > T_{b\,N_2} = 77.4$ K (the liquid nitrogen boiling temperature at atmospheric pressure) to be high-temperature superconductors, then from (4) it is clear that for the weak coupling (2) the electron – phonon mechanism will not lead to HTSC.

That is why already in 1964 Little [23] and then I myself [24] suggested the idea of using the (theoretically possible) attraction between conduction electrons caused by their interaction with bound electrons in the same metal. In vivid language one can speak of the replacement of phonons, i.e., excitations in a crystal (ionic) lattice, by electronic excitons, i.e., excitations in a system of bound electrons. Note that they certainly also include the so-called plasmons and polaritons. Such a mechanism can be called the electron – exciton or simply the exciton mechanism.

The exciton energy $E_{ex} = k_B\theta_{ex}$ in a metal does not exceed the Fermi energy $E_F = k_B\theta_F$ in the order of magnitude. As is known, $E_F \lesssim 10$ eV, that is, $\theta_F \lesssim 10^5$ K. For the exciton mechanism in (1)

$$\theta \sim \theta_{\mathrm{ex}}. \tag{5}$$

Therefore, already at $\theta_{ex} \sim 10^4$ K even for weak coupling (2) the temperature T_c can assume room values [e.g., at $\theta_{ex} = 10^4$ K and $\lambda = 1/3$ we have $T_c = 500$ K according to (1)].

Of course, these are now only words because it is not yet clear how, if at all, one can realise an effective exciton mechanism. A group of theoreticians at the Physical Institute of the USSR Academy of Sciences (FIAN) made considerable efforts to investigate the exciton mechanism of HTSC or, more precisely, the whole HTSC problem. The results of these studies are presented in monograph [25]. In line with [24], we underscored in [25] the expedience of using quasi-two-dimensional layered compounds. As is known, this conclusion was later confirmed. Another important result was the

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[2] The studies of the properties of helium II began in Leiden as far back as 1911, i.e., the same year that superconductivity was discovered. The landmarks on the long way that led in 1938 to the discovery of superfluidity [8, 9] are mentioned, for example, in paper 6 in [2]. The early stage of the study of superfluidity is described at length in papers [10] and in book [11].

establishment, in the known approximation, of metal stability conditions. The point is that one of the main dangers, if not the basic one, is that the lattice and thus the material (crystal) itself may not withstand attempts to raise T_c and might break. In [25] and in references therein it was shown that high T_c values are generally quite agreeable with lattice stability (a somewhat more thorough description of this can be found in lecture [14] recently published in *Usp. Fiz. Nauk*; for this reason I shall not give the details here). The fear that high values of the coupling constant λ for the electron – phonon interaction are unrealizable also turned out to be groundless, and the so-called strong coupling with

$$\lambda \gtrsim 1 \tag{6}$$

does, in fact, take place, in particular, in cuprates.

Moreover, the Debye temperature θ_D for cuprates is relatively high. Formula (1) is of course invalid in the case of strong coupling, but it nevertheless shows that T_c grows with increasing θ and λ. That is why the values $T_c \lesssim 200$ K can be readily obtained in the case of the electron – phonon mechanism, too (see [19] and papers 6 and 7 in [2]). It is another matter that, certainly, T_c is only one of the characteristics of a superconductor. An account of the electron – phonon interaction alone is insufficient to explain all the properties of HTSC cuprates, and obviously one should also allow for the nonsphericity of the Fermi surface and generally the distinction between the conduction-electron spectrum in the crystal and the free-electron spectrum (in BCS theory [22] the conduction electrons or, more precisely, the corresponding quasi-particles were assumed to be free with the exception of their interaction with phonons). In the past, the role of the electron – phonon interaction in the case of HTSC cuprates was frequently considered to be insignificant, in particular, because of the smallness of the isotopic effect (specifically, T_c changes little when the isotope ^{16}O is replaced by the isotope ^{18}O). However, for example, in angle-resolved photoemission spectroscopy the electron excitation spectrum shows a clearly pronounced isotopic effect caused by the electron – phonon interaction in cuprates [26]. Other grounds also exist for such a conclusion, and at the present time there is no doubt that the electron – phonon interaction plays an important, and even perhaps a decisive, role in cuprates.

In any case, if we do not speak of metallic hydrogen (for it $\theta_D \sim 2000 - 5000$ K), the obtaining of which for practical use is absolutely unrealistic at the present time[3], the possibilities of using the electron – phonon interaction for reaching high, say, room T_c values (that is, the creation of room-temperature superconductors — RTSCs) now clearly seem to be quite limited because the values of the Debye temperature θ_D is in most cases less than several hundred degrees. The same refers to the spin interaction because the Curie temperature θ_C and the Néel temperature θ_N are also typically less than, say, 10^3 K (we are speaking, of course, of substances at low pressures). But nonetheless, attainment of $T_c \sim \theta_C$ or $T_c \sim \theta_N$ and thus creation of RTSCs on the basis of spin interaction is not excluded. At the same time, as has already been said, the electron – exciton interaction is characterized by the temperature $\theta_{ex} \lesssim \theta_F \lesssim 10^5$ K. That is why I think that if room-temperature superconductors can actually be created, it can most likely be possible only with the use of the exciton mechanism.

True, I should make an important reservation. Namely, above I have rested upon a BCS type theory considering the formation of 'pairs' in the s-state to result from the fact that quasi-free conduction electrons exchange boson type excitations (phonons in the case of the electron – phonon mechanism and excitons for the exciton mechanism). The formation of 'pairs' in p, d, and even in other states is possible if the nonsphericity of the Fermi surface is taken into account and the band theory of metals is used. This is well known in the example of the superfluidity of ^{3}He [15]. The characteristic temperature determining the binding energy of such 'pairs' and, thus, T_c, the same as for θ_{ex}, is lower than or on the order of θ_F. Therefore, even if we forget about he role of phonons and spin interaction, RTSC can in principle be attained not only as a result of the exciton mechanism.

Generally, these is no doubt that an unprejudiced approach to the creation of RTSCs is needed. Clearly, at the present time the creation of RTSCs is a typical problem of so-called fundamental science (physics in this case) when we speak of reaching the goal, which is obviously possible in principle, but may be unrealistic. The creation of RTSCs has, of course, great potential regarding their practical use. However, such prospects should not be overestimated, as was the case of HTSC with all the boom around it (personally I am not to blame for this). The HTSC materials turned out to be technologically difficult to use, and their application is still rather limited, although some progress has already been made (see, e.g., [29])[4].

It is, of course, not yet time to think about the application of RTSCs, but their creation, I believe, is quite a clear (and, if you like, a very important) task faced by solid state physics.

How can one solve this problem? This is, of course, an enigma. I can only say how I myself would seek such materials if only I could. I remain a follower of the old ideas [24, 25]. Namely, I would seek, or rather create, quasi-two-dimensional layered materials with alternating, at the atomic level, well conducting (metallic) planes-layers and dielectric or, in any case, poorly conducting layers. Such compounds are HTSC cuprates and artificially created layered materials [31]. In addition, one should strive to attain a possibly rich electron exciton spectrum in the system. These excitons must, I repeat, replace phonons, the virtual exchange of which provides, in the case of electron – phonon interaction, attraction between conduction electrons and their 'pairing'. What has been said is certainly rather vague and not concrete enough. Unfortunately, I was engaged in the study of the exciton theory long ago [32], and now do not follow its development. However, I am aware of its many achievements. The corresponding information should be mobilized and used in attempts to create the materials discussed above.

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[3] As a matter of fact, metallic hydrogen has not yet been obtained even in the laboratory. At the same time, we should point to recent progress in the theoretical study of this substance [27, 28].

[4] After the anticipated start in 2007 of the LHC (Large Hadron Collider), CERN is planning the construction of an International Linear Collider (ILC) [30] with a length greater than 30 km. This machine, whose construction will probably begin in 2009, will cost five to seven billion dollars. It will produce two counter-propagating 500-GeV electron or positron beams. I am writing here about it because in the adopted project [30] superconducting magnets are planned to be used at a temperature of 2 K, and these will be conventional superconductors rather than high-temperature superconductors. Hence, the latter cannot in this case compete with conventional (low-temperature) superconductors in spite of the fact that the cooling of the giant machine with liquid helium is much more expensive than with liquid nitrogen.

One should also take into consideration some already known and experimentally established regularities that relate the critical temperature $T_{\rm c}$ to other measurable parameters characterizing superconductors (see, in particular, [33]).

As has already been said, the creation of RTSCs is a clearly outlined and very important goal in solid state physics. Not many other areas of physics can thus boast (of the existence of such problems). I shall permit myself to note in conclusion that in Russia close attention to the RTSC problem would be especially justified. This conclusion is grounded, first, in view of the tradition of active research in the field of superconductivity in the USSR, which was mentioned at the beginning of the note. Second, the corresponding research even at the most up-to-date level requires tens of millions and not billions of dollars that are necessary to create modern ITER, LHC, or ILC type plants [30]. I would like to hope that what has been said above will not pass unnoticed.

In concluding, I take the opportunity to thank Yu V Kopaev and E G Maksimov for their discussions.

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[6] This communication is also published as a supplement in paper [1].