# REMEMBERING NIKOLAI NIKOLAEVICH

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## 1. Personal Impressions

### *1.1. The late forties*

My first impression dates back to the spring of 1947, when N.N. read a special course of lectures on dynamic equations of statistical physics. Recall that Bogolyubov became a Professor of Moscow State University (MSU) in 1943 after returning from Ufa, where the Ukrainian Academy was evacuated during the war. In this period, he shared his time between the Institute of Mathematics in Kiev and the Faculty of Physics of Moscow University. At the end of 1947, N.N. was awarded the Stalin Prize for two treatises on theoretical physics, one of which was his monograph *Dynamic Equations of Statistical Physics.*

Rather short, in an elegant grey suit and a bow tie, portly and in his prime, lively and buoyant, he enthusiastically lectured following, in general, his above-mentioned book that had been published shortly before that. It was obvious that he enjoyed both the subject of the lecture and the contact with students. It was somewhat unusual, beyond the slightly aloof manner of lecturing maintained at the Faculty of Physics at that time, which was impressive in itself and evoked a sort of liking.

At first, the subject did not seem interesting to me (shortly before that, Smith's report on the atomic bomb test had been published in Russian, and my imagination was occupied with "deeper-concealed" mysteries of the Universe); however, the personal charm of the young (short of forty years old) and already known Professor, Corresponding member of the Academy of Sciences, and his clear and precise style made their effect, and I listened the entire course.

At the end of the next year, my student-fellow Valentin Nikolaevich Klimov (with whom we worked side-by-side under N.N.'s supervision for about five years and who later tragically died in the snow avalanche at the Caucasus) told me that NN had got a small theoretical department at the Institute of Chemical Physics of the Academy of Sciences of the USSR, and graduate students engaged on their diploma work are needed.

By that time, following the advice of my older friend Yura Shirokov, I had been in this position under Dmitrii Ivanovich Blokhintsev for about half a year, but was disappointed by my status. At that time, D.I. was the head of the then secret project for the construction of the atomic power plant in Obninsk and only occasionally came to Moscow. To meet him required time to be spent for telephoning and quite plenty of persistence. For all that time, I had not been given any problem to deal with except a simple "test" problem. Thus, I agreed to write my graduation thesis under N.N.'s supervision without much hesitation. It turned out that, in addition to the theoretical department that had long existed at the Institute of Chemical Physics (ICP) and was headed by Prof. Aleksandr Solomonovich Kompaneets, another one related to the Atomic Project was established there. After I came to the department, it comprised N.N., Boris Valentinovich Medvedev, and two undergraduates–laboratory assistants–Valya Klimov and me.

Here, coming to my mind is a scene that took place in the office of Academician Semenov, Director of the institute, later a Nobel Prize laureate. N.N. came to him with Klimov and me to make arrangements for our official position at the ICP. We were supposed to have a half-pay part-time job of laboratory assistants. However, the invited personnel manager said that it would take quite a lot of time to go through the formalities of our registration as part-time workers: it was necessary to apply to the Presidium of the Academy of Sciences, then to the All-Union Qualifying Committee to which Moscow University was subordinated at that time, then to agree upon the matter with the University's and faculty's administrations, *etc.*; finally, in the case of positive resolution, the papers were to go back in the same long way. Then, after a minute's common confusion, there came a question from N.N.: "Well, and if we take them as full-time workers?" This option turned out to raise no formality problems for the personnel department and no objection from Semenov. And, by the order of the Director, two young pikes were immediately thrown into the river.

The theoretical department occupied one room of moderate size. In the middle and opposite to each other, there were two desks. Two sofas served to seat visitors and occasionally to allow the hosts to have a nap. An indispensable accessory was the tea making and drinking outfit. There were also chess and a chess clock. For any visitor to find us working hard, the entrance to the room was closed, due to the organizational effort of B.V., by a double door with a small entry pass between them. For the "sake of secrecy", both doors were always locked, and while one of us was unlocking them to a knock from outside, the other was clearing away the chess and cups from the table.

The security conditions also implied that the scientific creative work should be over no later than 17:45 because all calculations, including preliminary rough ones, must be carried out only in tied sealing-waxed notebooks which were handed in at the security department. Nevertheless, the most fruitful time was the evening, when we were not disturbed by the neighbor scientists or inspection raids of fire or security officers, *etc.* We often sat late up to the last trolleybus. N.N. showed a completely quiet attitude towards our chess playing (though he did not play himself) and a liberal behavior. He appreciated the working qualities and the results obtained.

The chief assigned me the task to simplify the kinetic transport (i.e., neutron diffusion and slowing-down) equation. This beastly awkward integro-differential equation for the distribution function involves three independent variables even in the spherically symmetric geometry. In the general case, it allowed only tedious numerical calculations. The known approximations (one-velocity, diffusion, age-diffusion) were too rough for dealing with real problems under consideration.

From my present-day point of view, the remarkable fact is that N.N. only formulated the problem for the student without making even a hint at any lines of attack. The problem in question was interesting technically and very important in essence: any serious advance allowed a hope for the appreciable economy in numerical calculations, which led to a gain in time.

At the time before the advent of computers, numerical calculations of complicated equations were carried out with desk-top electromechanical calculators, a sort of cumbersome modified Mercedes or Rheinmetall comptometers supplied from defeated Germany as reparations. These machines were usually operated by girls united into a calculation team supervised by professional mathematicians. The latter prepared difference schemes suitable for paralleling, analyzed their stability, degree of accuracy, *etc.* Calculation pools like that could by no means be found in any institute – the calculation of more or less complicated problems was expensive and took much time. And the factor of time heavily weighed upon our activities. The first Soviet atomic bomb was tested only in August of the next year, 1949.

Within a few months, serious advances were made in solving the problem. I used a simplification of the integral operator kernel, the so-called scattering indicatrix, as a basis for the new approximation. The main idea came to my mind during the Moscow University Komsomol (Young Communists' League) conference. Having specially taken my seat at the gallery, as far from other physics department delegates as possible, I was deep in thought amidst the murmur of the reporting. . .

Details omitted, it can be mentioned that, in the mid-1950s, when the pure theoretical part of my investigations was unclassified, two papers on the method of the so-called synthetic kernel in the theory of neutron diffusion and slowing-down were published in the journal *Atomnaya Energiya* (*Atomic Energy*). One of them corresponded to the graduation thesis written at the ICP in 1949; the other, involving the generalization to a more complicated case of neutron transport in media containing hydrogen nuclei, to the candidate of science's (Soviet and Russian PhD equivalent) thesis defended in May 1953.

About ten years after, both papers were fully reproduced in the monograph by Davison. The method of approximate scattering indicatrix transformation was the subject of the chapter "Shirkov's Method". I was informed about it at the tennis court in Dubna by Bruno Pontecorvo, who, being a student of Fermi, was keeping up with all publications in the subject of his great teacher that came to the JINR library. It became clear that the American colleagues failed to come up with anything equivalent to it. Availability of powerful computers panders to the "have a computer, needn't have the wit" philosophy. This collision of the Russian native wit with the spoiled American theorists took place again in the mid-1970s; it involved my students and concerned the calculations of three-loop Feynman diagrams in gluodynamics.

Along with this, so to speak, major activity, I began attending N.N.'s seminar at Steklovka (Steklov Institute of Mathematics, Academy of Sciences of the USSR, Moscow). This institute was accommodated in a lavishly glazed building slightly protruding into Leninskii Prospekt (Lenin Avenue), right opposite to what was the Presidium of the Academy of Sciences at that time. The seminar was held once a week, and when N.N. was absent, it was conducted by Sergei Vladimirovich Tyablikov. Among the things studied at the seminar was, for example, Schwinger's known series of papers.

An extremely helpful tradition at the seminar was the review of publications. At the end of each meeting, the conductor of the seminar looked through a recent issue of a journal, the *Journal of Experimental and Theoretical Physics* or the *Physical Review*, pointed out interesting articles, and gave them out to the young colleagues. At the next meeting, the main report was preceded by one or two five-minute essays on the previously given topics.

This system yielded two results: first, all participants were regularly briefed on the news; second, the audience was not divided into those active and those passive. Attending the seminar, you can kindly work and show with your essay what you know and how critical you can be about somebody else's results. My first essay was about the "sensational" statement that there existed classically stable electron orbits around the positively charged nucleus, which was published in the *Physical Review.* The mistake was that the quadrupole and higher multipole radiations were neglected. I could easily get to the core of the problem, because by that time I had already managed to pass the examination on field theory administered by Lev Davidovich [Landau] himself. That essay enhanced my status among the participants in the seminar.

It should be mentioned that, at that time in the late 1940s, N.N. had just turned from statistical physics to the theory of particles. (These turns in the subject of studies were typical of Bogolyubov when he, having ultimately solved a complicated problem, lost interest in that topic forever). His first papers on the covariant formulation of the Schrödinger equation were published in 1951.

The seminar was held at the end of the weekday; then N.N. and the participants went out, and, after walking about half a kilometer, the company turned to a shop under the sign "Ararat" at the shorter side of the dwelling house that belonged to the Academy (where later a baker's was located for a while). There, one would have not only a bottle of Armenian cognac uncorked but also glasses and sliced lemon served. And that was the actual finale of the seminar.

In 1948–1949, N.N. lived with his family in Kiev and regularly came to Moscow staying at the Moskva or Yakor (in Gorky Street) hotels. His arrivals and departures were like small festivities celebrated in restaurants where N.N. invited all his co-workers, including students. My first time in a restaurant was just on that occasion. Generally, and especially in those younger years of his, N.N. was a very cheerful and actively amicable man. He liked to enjoy life and share this joy with others.

Two strong impressions of N.N.'s personality at that time (to the student's eye): devotion to his science and a high level of culture. Scientific work seemed to be the major point of his life and the main source of joy. He did not play chess or cards and did not go in for sports. For him, a good pastime meant good intellectual work. A relevant reminiscence from the 1960s: to my question, "Did you have a good rest?", which I asked N.N. after his coming back from a Caucasian sanatorium, there followed the answer, "Yes, excellent. Two works are finished."

Contacts with N.N., giving rise to a liking and an involuntary wish to imitate, led to a change in the scale of life values—intellectual activity did not merely moved to the first place, it took top priority. N.N.'s immense erudition in history, linguistics, and literature amazed me, a well-read boy from a professor's family. Those impressions aroused continuously, got embedded in serious scientific discussions, were enhanced by some satirical chords. His wise, calm, and somewhat ironical attitude towards life was based on, so to speak, stable invariants formed in his youth. Though N.N. never spoke about religion, his moral rules, conveyed to the students in some hidden ways, complied with the Christian commandments. Whereas the main source of humor for the Moscow intelligentsia was the novels by Ilf and Petrov, N.N. read by heart Shchedrin, Roman authors, Jules Romain, who is still poorly known in our country, *etc.* When cited by him, eternal motives and characters produced by classic authors got implicit both in quite simple psychological situations and in unexpected turns of world policies. Based on the experience in contacting with a lot of eminent scientists gained for more than 50 years of my academic life, I can now add that this impression of N.N.'s intellectual and moral exclusiveness has only enhanced with time.

The described period ended in the spring of 1950, when our group was transferred from Moscow to where, according to a graphic phrase, "Khariton drove the calves"[1].

### *1.2. At the installation*

One fine day in March, I was called to the ICP security department and informed that I was to be transferred from the institute to "Lab-2". This lapidary name

[1] The full verse by A.S. Kompaneets:
"I will be quick as a flea and slippery as a triton
To keep away from places where drives his calves Khariton".

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was given to what is now "Kurchatnik" (Kurchatov Institute). However, at that time, Lab-2 disguised the PGU–First Chief Directorate at the Council of Ministers of the USSR, which was in charge of the atomic matters and was later transformed into Sredmash (Ministry of Medium Machine-Building of the USSR). The PGU was located in a huge building in the north-west part of Moscow. In the personnel department of this institution, I was flabbergasted at the news that I was to leave for the new place of work within a week. I was not informed about either the name of the place or how far it was from Moscow.

A departure to the Installation (the colloquial name for our town at that time, "obiekt" in Russian) proceeded as follows. The personnel department of the Large House sent the novice to one of the central squares where he was "to enter the unlighted arch of building No. NN, turn to a shabby door without a sign, go several meters along a narrow corridor in utter darkness, grope for a door on the left and open it". On fulfilling all these instructions, you found yourself in a lamp-lit room standing in front of a man who sat at the table and immediately addressed you by your name as if you were acquaintances.

He handed me a travel document and directed that the next day I "arrive at the Vnukovo airport with the belongings, sit on an indicated bench in the waiting hall at an indicated time". The answer to all my questions about any details or explanations was: "I can't tell you that, but don't worry". The next day, an unknown man came up to me in Vnukovo at the assigned time, called me by name, and told me that the boarding would begin in a few minutes and I should follow him. A quarter of an hour later, without any announcement of the flight, a group of people led by a guide crossed the airfield without passing the ticket control and embarked a twin-engine, apparently 12-seat cargo version of the Douglas aircraft with aluminum seats along the sides, which immediately taxied to the runway. Owing to fair weather at the time of landing, I could approximately determine the place of destination. On leaving the plane, I caught the sight of control officers in the militia uniform and... Valya Klimov, who came to meet me.

The term "Zvonkovoe" was proposed by N.N., who took it from an operetta popular in the pre-war time. A byword from it, "Come to see us in Zvonkovoe", was particularly in place for our top-secret town situated in a vast forest and surrounded with barbed wire fences. N.N.'s group in "Zvonkovoe" originally included V. Klimov and me. Quite soon Dmitrii Nikolaevich Zubarev joined it, and in 1951 there came Yurii Aleksandrovich Tserkovnikov and Vasilii Sergeevich Vladimirov.

In terms of work, our group was closely linked with the group of Igor Evgenievich Tamm. Both teams were simultaneously transferred from Moscow to the Installation by the same secret decree of the Council of Ministers in the spring of 1950 for intensification of the work on the making of the hydrogen bomb.

In terms of everyday life, our teams were a single whole. Beginning with the autumn of 1950, several theorists without families (including those whose families stayed in Moscow) were lodged in a two-storeyed two-flat cottage built to the standard of the atomic agency as of the late 1940s. Similar cottages have still remained not only in Sarov but also in Dubna, the Chernaya Rechka (Black River) area. Upstairs, in the two-room flats of each half of the cottage, there lived the Corresponding members of the Academy Igor Evgenievich (I.E.) and N.N. Downstairs, under I.E., one room was occupied by Andrei Dmitrievich Sakharov, who was replaced, after his wife Klava with daughters came, by Tserkovnikov, and the other room was occupied by a young theorist from FIAN (Physics Institute, Acad. Sci. USSR) Yura Romanov; in the other half-cottage, under N.N., there lived Valya Klimov and me.

The residents of the theoretical cottage together with a few young theorists from I.E.'s and N.N.'s groups who resided in the nearby hotel set up an informal household association "United Theorists Organization". The UTO members have common board: non-resident cooks–elderly and cheery Aunt Sonya and younger Valya–made and served breakfast and lunch. Young people stored up food. Wholesale purchases were made using the Pobeda passenger car attached to the theoretical department, which usually brought us to work a couple of kilometers away by nine o'clock in the morning and back home for lunch in the afternoon.

On some days-off (at that time, a week was a six-day period and each date that was a multiple of 6 was a day-off), we went for wholesale "foraging" to a large village of Diveevo (to the church of which, relics of Serafim Sarovski were moved in the post-Soviet time) situated outside the Zone. To leave the Zone, one had to get permission and a pass for the entry control point. The Zone was an area of several hundred square kilometers surrounded, in full compliance with the penitentiary camp norms and standards, by barbed-wire fences, an exclusion zone, watchtowers, searchlights, *etc.*

Holiday markets were abundant in goods, the customers from the Zone were lavish, and it was rumored among the local people that there, "behind the barbed wire", an experiment on building the communism was going on.

The chiefs, i.e., I.E. and N.N., liked to sleep longer in the morning and usually came to work by eleven o'clock or so. N.N., who spent about half of his time at the Installation, made regular reports to review the news in "unclassified" science, mainly in quantum field theory. It was remarkable that immediately after lunch N.N. – 40-year old at that time (!) – had a customary rest, "washed the brain", as called it. After 4 p.m., music was heard from the radio above. This meant that the chief got up, had tea, sat down to work. At that time, he was already allowed to be disturbed.

The actual working hours of the theorists were regulated by the security department. The work on the main topic, including rough calculations and drafts, could only be done in special personalized tied and sealing-waxed notebooks slightly larger than the A4 size with each page numbered. Each of us had a special briefcase to carry the notebooks and a personal seal with a number. The briefcase was either with his owner or, sealed by him, in the security department. It could be taken from or handed to the department only within the working hours. Unlike the case in the ICP, it was prohibited to stay in the working room in the off-hours. Only sometimes, before a regular test at the far testing ground, an all-out effort order would be issued for some departments. Therefore, after 6 p.m. and on days-off, it was possible to think over and discuss working topics only during the walk in the forest after making sure that you could be heard by nobody except birds. Under these conditions, it was quite natural to be occupied with unclassified science in the evenings, especially under continuous influence of such figures as Tamm and Bogolyubov. It was just at those years that I began seriously studying quantum field theory in my off-hours.

A couple of illustrations to the psychological portrait of N.N. I remember the day when the Korean War began (June 25, 1950). It was reported in the morning news that the troops of South Korea, the United States' satellite, had suddenly crossed the 37th parallel, which was the boundary at that time, treacherously invaded peaceful democratic North Korea, broke the frontier defense line, and moved several tens of kilometers into the area. However, the valiant North Korean army succeeded in regrouping, overthrew the aggressor on the same day, and carried the war into the enemy's territory.

On that day N.N. came by air from Moscow. Valya and I met him. Sitting in the car with N.N., I am excitedly telling him all this official propaganda rubbish. As if he does not hear anything, N.N. starts telling us what is new in Moscow. We come home and begin helping him with his luggage. N.N. puts the kettle on the stove and turns on the radio. Here comes another summary of victories gained by the North Korean army. And the chief's face suddenly gets distorted as if by pain... It becomes clear that he took my words in the car for an attempted practical joke and probably wondered inwardly at our intellectual awkwardness. And it turned out that it was not the rubbish but the reality, the responsibility for which rests with much higher-ranking persons who govern our way of life.

N.N. did not like abundant words. Thus, the paraphrase "First think, and then speak" of the remark which Tamm put into Dirac's mouth,[2] is fully applicable to him. This is well illustrated by the above episode. Another man of the similar kind was the scientific supervisor of the Installation and the chief designer of nuclear weapons Yulii Borisovich Khariton. Any time when N.N. needed help or advice of Yu.B., he went to see the latter and gave him the gist of the matter. As a rule, Yu.B. never gave an immediate answer. After a short pause, he changed the topic. At another meeting in a few days, he could return to the problem and propose a solution. And could not, either. Like a Japanese who avoids saying "No". To characterize the procedure of inputting information into Khariton's head and gradually arriving at a fully shaped solution, N.N. used the verb "to kharitonize".

Finally, one more episode, now from the 1970s.[3] The scene is laid in the office of the JINR director. The secretary tells N.N. that an Academician X, director of one of the JINR Laboratories, has suddenly come to see him. The Academician enters and effusively explains that a major discovery recently made at his Laboratory did not win the recognition of Western colleagues. He proposes to discuss the situation at the nearest meeting of the Scientific Council of JINR and ask the Council to make decision as to the reality of the discovery. N.N. listens to his emotional interlocutor with perfect calm and... offers a cup of tea. While having tea, he speaks about some scientific news. Tea drinking is finished, and X returns to his problem. Then N.N. says, "I have just tried to imagine that the director of the Mathematical Institute Academician Vinogradov makes a proposal to the members of the Scientific Council of Steklovka Kolmogorov, Pontryagin, Aleksandrov... to consider this or that theorem proved..." Without listening to the whole, X rushes away from the office.

[2] In full, the remark made by Dirac to Niels Bohr, of which I.E. was a witness, is as follows: "In my childhood, my mother taught me first to think and then to write".

[3] Cited according to the witness, a student of Bogolyubov.

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### *1.3. Bogolyubov and Lavrentiev*

I think of my quite intimate, almost family-like acquaintance with two remarkable people, Bogolyubov and Lavrentiev, as being the greatest heaven's gift.

In outward appearance, Nikolai Nikolaevich and Mikhail Alekseevich were a rather contrasting pair. Somewhat portly, medium tall N.N. and thin, very tall M.A. A handsome face of N.N., with slightly wavy thick hair even in his declining years, and a rather long face of M.A., with scanty hair. "Outwardly clumsy, sometimes even awkward" (as Boris Evgenievich Paton put it) Lavrentiev and dandified and elegant, artistically looking, often in a bow tie, Bogolyubov. Similar in their appearance were large foreheads and the expression of the serious eyes.

They became friends in Kiev as far back as the 1930s when the ten-year difference in age was yet significant. Mikhail Alekseevich had known Bogolyubov's teacher N.M. Krylov well and used to call tenderly his friend Kolyasha behind his back. In science-like conversations, N.N. often gave examples and episodes which involved Mikhail Alekseevich, whom he loved and esteemed. As a result, without seeing him, I had already a myth of M.A. formed in my mind.

And there came the time we met. It was in Sarov in May 1953. I just returned from Moscow where I got the PhD degree in the Scientific Council of Lab–2 presided by Igor Vasilievich Kurchatov himself. Traditionally, a small banquet was given in our theoretical cottage to celebrate the successful defense of the thesis. We had already drunk a couple of toasts when the chief, who was a bit late, said while sitting down to table, "Lavrentiev has come to our Installation". To my remark, "It would be nice to invite him," there followed, "No problem, here he goes away from our house." I rushed out, came up with Mikhail Alekseevich, introduced myself, and immediately invited him to our place. He agreed without hesitation, and we came back together.

As was already said, I had worked about three years in Sarov by that time. That period was associated with the development of Sakharov and Tamm's "puff", for the participation in which I was given my first state award.

The innocent acquaintance at the celebration table entailed serious consequences. In the autumn of 1953, after finishing the work on the "puff", N.N. (as well as Tamm) returned to Moscow having "handed" me to Lavrentiev, in whose team I worked on the nuclear filling of the atomic artillery shell for the next three years.

Scientifically and technologically, the problem was to turn a spherically symmetric structure of one meter in diameter with about 10 kg of uranium–235 or plutonium (the first American bombs thrown on Hiroshima and Nagasaki, and the first Soviet ones as well) into a sort of melon with the cross dimension allowing it to go in a cylindrical shell of caliber not larger than 400 mm.

The breaking of the spherical symmetry makes it appreciably more difficult to calculate the now non-synchronous ignition of detonators, the hydrodynamics of the convergence of shockwave to the center of the article, and the process of development of the nuclear chain reaction.

The joint work with Lavrentiev, which ended in a successful test at the Semipalatinsk testing ground in March 1956 and was marked by the Lenin Prize, resulted in close contacts with M.A. within the second half of the 1950s. On returning to Moscow, Mikhail Alekseevich ("Ded" (Old Man), as he was called by the close friends of the Lavrentievs) devoted himself to a new grandiose patriotic activity of establishing the Siberian Branch of the Academy of Sciences. It was already at the Installation that he began looking for those who would help him with developing Siberia. In the late 1950s, working at Steklovka and in Dubna, I several times travelled on business to Novosibirsk and the site of the future Akademgorodok. Nikolai Nikolaevich rendered his support to the Siberian project by participating in the Commission of the Presidium of the Academy of Sciences for the establishment of the Siberian Branch.

In 1958 M.A. acquainted me with one of his major associate Sergei Lvovich Sobolev, who started the Institute of Mathematics in Novosibirsk, and offered me to become the head of the Theoretical Physics Department at that institute. I began selecting the staff. At the first election of Academy members for the Siberian Branch in 1958, I was included in the Corresponding Member ballot list, but the success came only at the second election two years later when I was jointly nominated by Academicians Bogolyubov, Lavrentiev, and Sobolev.

In the autumn of 1960, I moved to Akademgorodok near Novosibirsk. One of the first vivid impressions was the celebration of Lavrentiev's 60th birthday in unexpectedly cold November with severe frosts. Nikolai Nikolaevich was among the guests.

Lavrentiev could influence people and get them into his activities. Nikolai Nikolaevich used to say that M.A. is "a master of playing chess, human chess". To implement his plans, e.g., in the establishment of the Siberian Branch, Mikhail Alekseevich looked for those who shared his ideas and was willing to help among the professionals in various fields of science, art of organization, journalism, civil engineering... Among them, he especially valued those who were similar to him in the main feature – service to the cause. And he treated them as friends. Yet, he could abruptly change his good treatment for the opposite.

M.A. was a rather good actor and stage director. He used these talents not only for organization of entertainments and celebrations, for which he was an enthusiast, but also for... clarifying some features of his co-workers.

As an organizer, when searching for the currently needed "chessman", Lavrentiev could resort to strong pressure decorated with some theatrical gesture.

The most vivid example is related to my short administrative carrier. The Rector of the new university in Akademgorodok, the known mathematician Academician Ilya Nestorovich Vekua offered me the position of a pro-Rector, in which I served, on a part-time basis, about half a year. However, the atmosphere of "high administration" was not to my liking. I failed to overcome the repulsion to the bureaucratic work; I literally got sick from the morning twice a week, when I had to go to the office and fulfill my pro-Rector duties.

The culmination was the conversation with Lavrentiev in his office at the Institute of Hydrodynamics when, angry at me, M.A. flung the cue-like pointer, with which he liked to go to and from the blackboard, and broke the window. Only much later, being a witness rather than a participant in other similar scenes, I could calmly appreciate the stage direction talent and the acting ability of the great man.

Eventually I managed to escape the high position. In return for that I had to agree dealing with the school Olympiad and the physics and mathematics boarding school under University.[4] Note that I was fulfilling these two "organizational duties" for almost ten years with ever increasing interest and now remember all those activities with pleasure.

## 2. Joint Work

### *2.1. Quantum field theory*

Nikolai Nikolaevich began to study hard the problems of quantum field theory (QFT) at the end of the forties undoubtedly under the influence of the well-known papers by the founders of the modern covariant field theory. Anyhow, his first quantum-field publications appeared in 1950-51, three of them being devoted to equations in variational derivatives of the Tomonaga–Schwinger type. The latter were based on the axiomatic definition of the scattering matrix as a functional of the Bogolyubov function of the interaction region $g(x)$ generalizing the Schwinger surface function $\sigma(x)$.

In the first half of the fifties, N.N. was actively involved in the rapidly developing field, the renormalized quantum field theory. Moreover, he "entered" it on the side of mathematics, nonlinear mechanics, and statistical physics, already having results of a world level. He went into it longer and grew into it deeper than other scientists migrating to QFT from mathematics (Gelfand) and other more classical fields of theoretical physics. To some extent, N.N. can be compared with the English-American theorist Freeman Dyson. It is known that Nikolai Nikolaevich created his renormalization method on the basis of the Sobolev–Schwartz theory of generalized functions. Recall that the Bogolyubov renormalization method appeared approximately at the time of writing our book—in the mid-1950s. Nikolai Nikolaevich and his fellow workers (Ostap Stepanovich Parasiuk and, later on, V.S. Vladimirov) were led to considerably elaborate the works by Sobolev and Schwartz as applied to QFT needs, in particular, to introduce a class of functions that made possible the Fourier transformation and the determination of a procedure of multiplication of singular functions. Within his approach, one does not need to introduce "bare" fields and particles and can avoid a physically unsatisfactory picture of infinite renormalizations.

Nikolai Nikolaevich used to give talks from time to time at the Sarov theoretical department with the review of large parts of novel QFT such as "renormalization", "functional integral", or "surface divergences". Listeners to these reviews were impressed by that N.N. "saw" those so different fragments from a single point of view and perceived them as a part of one picture. Recall that that was the time when textbooks on particle theory were prewar editions of "Quantum Radiation Theory" by Heitler and the book by Wentzel issued at the beginning of the forties. "Quantum Electrodynamics" by Akhiezer and Berestetsky (1953), as well as the first volume of "Mesons and fields" by Bethe, Hoffman, and Schweber (1955), were awaiting for their appearance.

One day in the autumn of 1953, being impressed by one of his lectures, I asked, "Nikolai Nikolaevich, why do not you write a book, a textbook, on QFT??" The reply was, "Not a bad idea. Probably, could we realize it together?" First, I did not take this suggestion seriously. The list of N.N.'s papers shows that, by his 25th

[4] Looking back, I may note that I remember this episode with regret. Because of my independent nature, I could not put up with direct pressure and because of being young – some 33 years old – I could not realize the difficulty of my esteemed chiefs' position and render them the adequate help.

anniversary, N.N. was the author and coauthor of several monographs, whereas it was something new for me. However, not only bad habits are contagious, and ten years later my disciples Ginzburg and Serebryakov, who were under 30, became coauthors of the book on dispersion relations. Later on, this situation was repeated with Belokurov. To justify my reaction, I should like to note that only in May of that unforgettable year, one of the coauthors of a future book had defended his Candidate Sc. Dissertation (PhD) on the theory of neutron transfer and had none of the papers on quantum field theory (QFT), whereas the other became Full Member of the Academy of Sciences in October.

However, in a week, the conversation was resumed, and we began to discuss the details of the project. The time frame of those events is reliably determined, first, by the fact that the above dialog took place in a car when we were going to N.N.'s apartment in the Shchyukin Street (the region of the Kurchatov Institute), i.e., before Nikolai Nikolaevich moved to the MSU skyscraper on the Lenin hills at the end of 1953. Second, by the time we sent a proposal to Gostekhizdat at the beginning of 1954, the book by Akhiezer and Berestetsky had just been published. At the same time, the first version of the consistent presentation of the Bogolyubov axiomatic $S$-matrix was submitted for publication in *Uspekhi Fizicheskikh Nauk* at the end of 1954.

The first draft of the book, except for the introductory part expounding the Lagrangian formalism of relativistic fields and the Schwinger quantization scheme, included an original axiomatic construction of the scattering matrix mainly built on the Bogolyubov causality condition, his renormalization method based on the theory of distributions, as well as the method of functional integral and the generalized Tomonaga–Schwinger equation.

The work on the book followed the scheme "gasoline is ours – ideas are yours". Most part of the work was done at N.N.'s home in the main building of Moscow State University on the Lenin hills, where we talked for an hour-two making the outline of yet another section. Then I wrote the first version of the text that was discussed at out next meeting and very often was considerably revised. The thoroughly rewritten manuscript, if finally approved by the chief, was put on the upper left corner of the big wardrobe, from where it was taken and typed by Evgeniya Aleksandrovna. Slightly embossed paper of several colors was used for typing. This type of paper, produced by the Riga factory, was specially bought for our work. Nikolai Nikolaevich liked it very much. Different paragraphs of the typescript had different colors: blue, yellow, light-green, violet ... . Three copies were typed at a time. I took the typed paragraphs from the opposite upper right corner of the wardrobe to write down the formulae. The third copy of colored paragraphs stitched into chapters was intended for the critical reading by collaborators of N.N.'s department in the Steklov Institute. This reading was the first "running in". Two extensive papers submitted to *Uspekhi Fizicheskikh Nauk* were meant as the second one. Therefore, the text of the book issued in 1957 was for the most part rather well "ironed", and, except for the fresh material of the last two chapters on renormalization group and dispersion relations, it displayed, in a sense, the "third approximation". Looking back from the vantage point of my subsequent experience, I must say that this monograph over 30 printer's sheets long, was written rather quickly. The decisive reason, in my opinion, was that N.N. had a clear-cut plan in his head from the very beginning, and he kept the whole written text in his mind afterwards.

### *2.2. The birth of the Bogolyubov renorm-group*

In the spring of 1955, a small conference on quantum electrodynamics (QED) and elementary particle physics was held in Moscow. It took place at the Lebedev Physical Institute (FIAN) in the first decade of April. Among the participants, for the first time in the post-war period, there were several foreigners, including the well-known theorists Ning Hu from China and Gunnar Kallen from Sweden. My short presentation concerned consequences of the Dyson finite transformations for renormalized Green functions and matrix elements in QED. The central event of the conference was the talk "Basic problems of QFT" by Landau, in which he dwelled upon the ultraviolet behavior in the local quantum field theory. Not long before, the problem of behavior at small distances in QED was considerably advanced in a series of papers by Landau, Abrikosov, and Khalatnikov. They succeeded in constructing a closed approximation to the Schwinger–Dyson equations which turned out to be compatible with both renormalization and gauge covariance. This so-called "three-gamma" approximation admitted an explicit solution in the massless limit that is equivalent, in modern terms, to the summation of leading ultraviolet (UV) logarithms. The most remarkable fact was that the solution appeared essentially self-contradictory from a physical point of view, as it contained an unphysical ("ghost") pole in the renormalized amplitude of a photon propagator – the problem of "zero charge". Landau's verdict was pessimistic: forget about the local quantum field theory and the Lagrangian. Just this thesis was advocated by Isaak Yakovlevich Pomeranchuk, Dau's (Landau's) coauthor in "zero-charge", in a conversation with me. In the name of this thesis, he even closed his seminar on quantum field theory at the Institute of Theoretical and Experimental Physics (ITEP) and recommended younger colleagues to change their field of interests. In those days, our meetings with N.N. were regular and intensive, as we were busy with the preparation of a rather advanced text of our book. N.N. was very curious of the results of the Landau group and posed a task for me to evaluate their reliability by constructing, for example, the second approximation (including, in modern terms, the next-to-leading UV algorithms) to the Landau-et-al. solution of the Schwinger–Dyson equations for verification of the stability of UV asymptotics and the existence of a "ghost" pole.

At that time, I often met Abrikosov, a good friend of mine since we were students. Soon after the FIAN conference, Alyosha let me know of the just published paper by Gell-Mann and Low. The paper dealt with the same problem but, as he said, was rather complicated for understanding and difficult to be combined with the results obtained by their group. I looked through the paper and shortly informed my teacher of its method and results that included rather complicated functional equations and some general statements of scaling properties of the distribution of electron effective charge at small distances from its center. The scene that followed my report was quite impressive. N.N. immediately claimed that the Gell-Mann and Low approach was correct and very important, it represented the realization of the group of normalization discovered a couple of years before by Stueckelberg and Petermann (published in French!) while discussing the structure of finite arbitrariness in matrix elements that arose after the removal of divergences. That group was an example of continuous transformation groups studied by S. Lie. It followed that the group functional equations, similar to those derived by Gell-Mann and Low, should hold in the general case, not only in the ultraviolet limit. Then N.N. added that the most potent tool in the Lie group theory was differential equations corresponding to infinitesimal group transformations. Luckily, I was acquainted with the fundamentals of the group theory that was beyond the curriculum of the Physics Faculty of MSU. Within the next few days, I managed to reformulate the Dyson finite transformations for the electron finite mass case and derived the sought functional equations for scalar propagator amplitudes of QED corresponding to group transformations, as well as the relevant differential equations, i.e., Lie renormalization group equations. All the derived equations contained a specific object – the product of the electron charge squared by the transverse amplitude of a dressed photon propagator. We called this product the invariant charge. From a physical point of view, it represents an analog of the so-called function of electron effective charge first considered by Dirac in 1933 and describing the charge screening effect due to quantum vacuum polarization. We also introduced the term "renormalization group" in the first of our publications in *Doklady Akademii Nauk* in 1955 (and *Nuovo Cimento* in 1956). In the second simultaneous publication (after two line calculations) ultraviolet and infrared asymptotics of QED at the one-loop level were reproduced which were in agreement with the above-mentioned results of the Landau group, as well as a new two-loop solution for invariant charge was obtained which made it possible to discuss if the problem of "zero charge" is real.

## 3. Bogolyubov and Landau

The relationship between two great scientists is undoubtedly a very delicate subject. A lot of different things had long been piled up around these relations, both personal and at the level of their Schools. Since Lev Davidovich was my first teacher in modern physics, I think it appropriate to share my impressions and my insight in how these relations developed.

I will start with the description of the personality of Dau whom I got to know in 1946 as a second-year student. After a brief phone talk with the famous scientist, I was invited to his place for an entrance mathematical interview on his well-known theoretical minimum. A penetrating and cheerful glance, an aquiline profile and a curly forelock, promptitude of speech, quick reaction and agility, as he flew upstairs to his study on the second floor, leaving me alone to think over the next question, made a deep impression on me. I proceeded to the study of "Mechanics" (Landau and L. Pyatigorsky were the authors of the first pre-war edition) and started to attend theoretical seminars in "Kapichnik". Dau was very artistic by nature, liked and knew how to produce an effective impression. Leading the seminar, at which he was by head and shoulders above all other participants, he did not miss the opportunity to amuse the audience by bright *mise en scenes* immediately going deeply into complicated original constructions of the author (the authors were sometimes rather well-known scientists) and very often by a few remarks making mincemeat of the speaker. I was a witness of a scene like that suffered by Gelfand. However, only the closest colleagues of Dau knew that, to obtain admittance to the seminar's platform, a pretender had to pass the "purgatory", i.e., to present his work to Dau himself.

Unlike many eminent theorists, Dau fully realized the importance of mathematics for theoretical physics and often used it masterly. It is indicative that his theoretical minimum included two maths-related subjects as the first thing, including the examination on the qualitative theory of differential equations with special emphasis on the analysis of singularities. Dau's maxim "physics begins where a singularity occurs" is well known.

### *3.1. Three episodes*

The first episode happened in October 1946, when N.N. reported on his work on the theory of helium superfluidity at a general meeting of the Physics and Mathematics Division of the USSR Academy of Sciences. By that time, Dau had been a classic of superfluidity for five years, the author of the well-known phenomenological theory qualitatively explaining the phenomenon due to the presence of a collective linear branch of sound vibrations in the spectrum, as well as employing the notion of quanta of elementary excitations (torsional vibrations), rotons, introduced by him. Participants of the meeting recall that Landau harshly polemized with the speaker, whose reasonings were based on the physical hypothesis of a crucial role of condensate, i.e., an essentially collective effect. However, Lev Davidovich quickly digested and evaluated what he had heard, as two or three weeks later he submitted for publication a short article [10], in which he suggested a curve with flexure for a spectrum of excitations. Of an object independent of the spectrum of sound excitations, the roton spectrum turned into a part of the whole curve.

Landau's phenomenological curve results from N.N.'s formula under the assumption of the form of interaction of helium II atoms. Landau's article finishes with the phrase being a paraphrase of Bogolyubov's conclusion made in his report and the related publication [9]. However, any reference to Bogolyubov's report or paper was missing in Landau's short article. True enough, later on in a more detailed paper [11] (see also [12]), he manifestly pointed out Bogolyubov's priority, "It is worthwhile to note that N.N. Bogolyubov, making an ingenious use of second quantization, has recently succeeded in determining, in the general form, the energy spectrum of Bose–Einstein gas with weak interaction between particles."

The second "round" took place in 1955 in connection with the issue of "zero charge" in quantum electrodynamics. Without going into details, I should like to note that the analysis of this problem made by N.N. with the help of his just developed renormalization group approach [13] suggested that the conclusion of Landau and Pomeranchuk about the internal inconsistency of the local quantum field theory had no status of a rigorous result independent of perturbation theory. In a certain sense, a psychological scheme of the 1946 conflict occurred again: a rigorous mathematical reasoning at a deeper level led to a more general and precise picture than the previous semiintuitive scheme. As is well known, 10-15 years later on, the local Lagrangian perturbation theory recovered its status of the basic method of investigations in particle theory. However, the rigidity of the prominent physicist's conclusion [14] had a serious effect. It hindered the development of QFT and entailed some dead-end constructions like the "bootstrap" theory.

To the most serious test, Dau's ambition was put in 1957, when N.N. suddenly intruded into the theory of superconductivity. The phenomenon of superconductivity discovered in 1911 had been a painful challenge to leading theorists since the late 1920s. It was clear that superconductivity was a macroscopic manifestation of the laws of quantum mechanics. It was intensively studied by experimentalists, though the key point to theoretical understanding was difficult to comprehend. Dau had been working in this field since the mid-30s and together with V.L. Ginzburg constructed a phenomenological theory of superconductivity on the basis of the two-component order parameter in 1950.

An initiating pulse for Nikolai Nikolaevich to start working out the theory of superconductivity was a short note by Leon Cooper. N.N. immediately saw an analogy with the phenomenon of pair correlation of the boson type that he had discovered in creating the theory of superfluidity. Using the Fröhlich Hamiltonian of interaction of electrons with phonons (excitations of the ion lattice) as a basis and modifying his $(u, v)$-transformation from the theory of superfluidity for fermions, Bogolyubov applied [15] the condition of compensation of possible singularities in the vicinity of the Fermi sphere surface and derived an expression for the energy gap of the type of the Cooper formula with nonanalytic dependence on the Fröhlich coupling constant (see below).

At the time, when N.N. finished his investigation and began to talk on it at seminars, a thick preprint by Bardeen, Cooper, and Schrieffer was reported to appear in the West. However, this preprint did not reach Moscow yet. As far as I remember, Dau quickly evaluated Bogolyubov's work. It was even agreed upon organizing a joint Bogolyubov–Landau seminar on the theory of superconductivity. At the first meeting, after N.N.'s talk, Dau said: "Nikolai Nikolaevich, I do not know what there is in the paper by Bardeen and others, but I do not think that they have got such a beautiful and convincing result like yours."

This episode shows that at the time described Dau already considered N.N. an outstanding theoretical physicist, having subdued his own emotions. The Landau–Bogolyubov seminar existed not long and ceased after the appearance of the Physical Review issue with the paper of the three authors who proceeded not from the Fröhlich Hamiltonian, but rather from an approximate model Hamiltonian postulating effective attraction between electrons with opposite momenta and spins in the vicinity of the Fermi surface. Dau's words turned out to be prophetic.

Nevertheless, the mention of Bogolyubov's papers is seldom made by representatives of the Landau School in their publications on superconductivity, microscopic theory of superconductivity is called the BCS theory, and the term "theory of superfluidity" is bound up with the name of Landau only.

### *3.2. Supplementing each other*

Spontaneous symmetry breaking (SSB) is the subject of the 2008 Nobel Prize in Physics. This topic, in a sense, united two outstanding theoretical physicists Bogolyubov and Landau by their joint contribution to the explanation of the mechanism of phase transformations in large quantum systems followed by spontaneous symmetry breaking. The case in point is the systems that are described by mathematical expressions having some symmetry, whereas a real physical state of the system corresponding to a particular solution of equations of motion has no this symmetry. A situation like this appears when the lowest of the symmetric states does not provide the system with absolute energy maximum and is unstable. In this case, the particular lowest state is not the only one and their set is symmetric. A real reason for the symmetry breaking and the transition of the system to one of the lowest nonsymmetric states is an arbitrarily small nonsymmetric perturbation.

For a simple illustration, we turn to classical mechanics. Let us take a system consisting of an empty vessel with the concave bottom (a champagne bottle) and a small ball. Put this vessel, which is a body of rotation, in a vertical position and above it a small ball exactly along the axis. A system like this is symmetric with respect to rotation along the vertical axis. Let the ball go to the bottom. Upon reaching the bottom, the ball will not rest on the central convexity and will slide to the brim of the vessel. Thus, the initial conditions are symmetric, whereas the final state is nonsymmetric.

The initial material of physics, data of observations, has to be put in order and interpreted. The way of ordering consists in constructing a phenomenological scheme based on a representation, a physical picture, of the nature of a phenomenon expressed in a mathematical form, a form of a physical law. An important criterion for a system and its representations to be successful is not only the description of available data, but also a possibility to predict results of new experiments and show the way of their carrying out. This is the way of a theorist-phenomenologist, "from a phenomenon to a theoretical scheme" and back.

At the same time, many considerable results in constructing a physical theory have been achieved by another, more speculative way. Remember the unification of the force of terrestrial and celestial gravity, electricity and magnetism, as well as the recently discovered principle of dynamics from symmetry that led to the construction of the theory of electroweak interactions and quantum chromodynamics. Adherents of this way, trying to proceed from deeper physical representations, initial principles, *ab initio*, are often called "reductionists". This means that they try to reduce the description of the diversity of observed phenomena to a small number of simple and general notions and principles. In statistical physics, "reductionists" are, as a rule, authors of a microscopic approach.

I would like to cite the definition given by Bogolyubov in 1958 in his paper "Basic principles of the theory of superfluidity and superconductivity" [16]:

> *"The goal of the macroscopic theory is the derivation of equations of the type of classical equations of mathematical physics that would reflect the whole set of experimental data related to macroscopic objects studied. ...*
>
> *The microscopic theory poses a deeper problem of understanding the inner mechanism of a phenomenon following the laws of quantum mechanics. ... In this case, in particular, one also has to obtain relations between dynamic quantities resulting in equations of the macroscopic theory".*

However, one should not take a great accent in opposing these two ways of thinking. An important thing is that there is an interval, a logical gap, between the equations, for example, classical equations of mechanics or Maxwell equations in medium and the laws described by the sequence of events such as the laws of planetary motion of the solar system or the Meissner law in a superconductor. These are situations where phenomenology manifests itself most strongly. Therefore, the efforts of phenomenologists and reductionists supplement each other. The explanation of the form and the essence of electroweak interaction and the phenomena of superfluidity and superconductivity are vivid examples of modern quantum theory.

Bogolyubov and Landau made a pivotal contribution to the creation of the theory of macroscopic quantum phenomena, the phenomena of superfluidity and superconductivity accompanied by spontaneous symmetry breaking at the quantum level.

*3.2.1. Superfluidity*

The history of creation of the theory of superfluidity gives a good example of interconnection of phenomenological constructions and physical ideas. The original explanation of the phenomenon of superfluidity given by Landau was based on a general notion that superfluid properties of liquid $^4$He at low temperatures are defined by a linear spectrum of collective excitations (phonons) rather than by a quadratic spectrum of excitations of individual particles (atoms). It follows from this assumption that, in moving with a velocity not exceeding a certain critical value, it is impossible to slow down the liquid by transferring the energy and momentum from the wall to individual atoms, because a linear form of the phonon spectrum does not allow one to keep simultaneously the laws of energy and momentum conservation. The need for agreement between the form of the spectrum and the thermodynamic properties of liquid helium made Landau introduce excitations, in addition to phonons, with a quadratic spectrum beginning with a certain energy gap, excitation, which he called rotons.

Bogolyubov's theory is based on a physical assumption that, in a weakly non-ideal Bose gas, there is a condensate akin to the ideal Bose gas. The existence of the Bose condensate leads to the common wave function of the whole system, the collective effect. Therefore, the presence of an interaction as weak as is wished transforms single-particle excitations into the spectrum of collective excitations. To calculate this spectrum, Bogolyubov inferred that, at low temperatures, the Bose condensate contains a macroscopically large, of an order of the Avogadro number $N_A$, number of particles $N_0$. As a result, the matrix elements of the creation and annihilation operators of particles in the condensate are proportional to the square root of the "large" number $N$, and the main contribution to the system dynamics comes from the processes of particle transition from the condensate to the continuous spectrum and back to the condensate. The simplified system of quantum mechanical equations based on the afore-said has an exact solution, and the derived spectrum of collective excitations (bogolons) unified phonons and so-called Landau rotons. Bogolyubov's courageous intuitive guess of the important role of the condensate was experimentally verified only fifty years later.

It is also important that, in the Bogolyubov picture, there arises a natural, though not very transparent, answer to the question of the nature of symmetry violated in the phase transition of $^4$He into a superfluid state. This is the phase symmetry of a quantum Bose system that (with the help of the Noether theorem) is responsible for the conservation of the total number $N$ of particles, i.e., helium atoms in the system considered. Collective quasiparticles, bogolons, do not correspond to a definite number of atoms HeII representing a superposition of an infinite set of particle pairs with zero total momentum.

*3.2.2. Superconductivity*

Another example of a phase transition in a quantum system accompanied by spontaneous symmetry breaking is the phenomenon of superconductivity, where the phase invariance violation occurs, as in the case of the phase transition to a superfluid state. Though superconductivity was discovered in 1911 (much earlier than $^4$He superfluidity), a theoretical insight into the phenomenon of superconductivity was gained much later than the explanation of superfluidity.

A breakthrough along this line was a phenomenological theory suggested by Ginzburg and Landau (the G-L theory), in which a superconducting state was described by an effective wave function of a group of electrons playing the role of a two-component order parameter. The G-L theory successfully described the behavior of a superconductor in the external magnetic field and some other important properties. At the same time, the nature of a superconducting transition remained unclear.

The microscopic theory of superconductivity was developed only in 1957 by Bardeen, Cooper, and Schrieffer (BCS), and Bogolyubov. Bardeen, Cooper, and Schrieffer considered a simplified model, in which an interaction of electrons due to the exchange by phonons is substituted for the effective attraction of electrons near the Fermi surface. The BCS theory includes the thermodynamics and electrodynamics of a superconductor, the calculation of the temperature of a superconducting transition, and gives a universal relation between the gap in the spectrum at zero temperature and the temperature of a superconducting transition. A gap in the spectrum arises due to the formation of bound states of electron pairs with opposite momenta and spins, "Cooper pairs", and is proportional to the exponent $e^{-1/\lambda}$, where $\lambda$ is the intensity of electron attraction.

Before the appearance of a detailed paper by BCS, Bogolyubov succeeded in constructing a microscopic theory of superconductivity for the complete Fröhlich electron-phonon model. With the new Fermi amplitudes, he carried out the compensation of the so-called "dangerous diagrams" corresponding to the production of electron pairs with the opposite momenta and spins. The Bogolyubov equations for the gap and superconducting temperature coincide with the results of the BCS theory with the coupling constant $\lambda = g^2_{\rm Fr}$ directly determined by the Fröhlich coupling constant in the Hamiltonian with the electron-phonon interaction.

Bogolyubov's quasiparticles (sometimes called "bogolons") give a clear physical picture of the spectrum of quasiparticle excitations as a superposition of a particle and a hole that have a gap in the spectrum on the Fermi surface. Based on the Bogolyubov representation of quasiparticles, it is easy to calculate thermodynamic and electrodynamic properties of a superconductor. The Fermi version of the Bogolyubov canonical $(u, v)$ transformation is widely used in solving the present-day problems in the theory of superconductivity.

Bogolyubov came to the conclusion of the unity of these two macroscopic quantum phenomena: It is the superfluidity of Cooper pairs that creates a superconducting current. Here is the citation from Bogolyubov's review [18] of that time: "*The property of superconductivity may be treated as a property of superfluidity of a system of electrons in metal.*"

The unity of the phenomena of superfluidity and superconductivity has recently been confirmed in experiments with ultracold fermion gases in traps.

## 4. Scientist and Teacher

### *4.1. Features of Bogolyubov's creativity*

In conclusion, I shall sum up some observations following from the analysis of Bogolyubov's scientific creative activity only in theoretical physics in the 1950s.

Over that decade, NN contributed to about a dozen of scientific areas:[5]

| | | | |
|---|---|---|---|
| 1. | Tomonaga–Schwinger equation with area function | (4) | 1950–1952 |
| 2. | Plasma in magnetic field | (8) | 1951–1952 |
| 3. | Functional integral representation | (1) | 1954 |
| 4. | Causality condition and scattering matrix | (3) | 1955–1956 |
| 5. | Multiplication of singular functions and $R$-operation | (5) | 1955–1957 |
| 6. | Renormalization group | (4) | 1955–1956 |
| 7. | Physical dispersion relations | (3) | 1956–1957 |
| 8. | Subtleties of proving dispersion relations | (7) | 1956–1958 |
| 9. | Superconductivity for the Fröhlich model | (4) | 1957–1958 |
| 10. | Model Hamiltonians and pair correlations | (4) | 1959–1960 |
| 11. | Indefinite metric in QFT | (2) | 1958 |
| 12. | Quasiaverages | (2) | 1960–1961 |

A total of about 50 works and in addition five monographs.

It is worth mentioning that NN worked on each topic no more than two to three years, on the average; in some years, he published papers in 4–5 areas. The especially fruitful period was the mid-1950s.

*Speaking figuratively, in those years Bogolyubov was a fountain of fundamentally important scientific discoveries. Benevolence towards people, generosity of his nature resulted in that this fountain fertilized everybody who had wished to approach it and managed to imbibe vivifying water.*

Nikolai Nikolaevich set up the Laboratory of Theoretical Physics as a part of the Joint Institute for Nuclear Research in Dubna[6] and the foundation of his school in particle interaction theory was laid.

For comparison, we may look at such multitalented luminaries as Heisenberg and Landau. A cursory glance at their lists of papers reveals that each of them returned to the same topic within a period of more than a decade.

The motto best suited for NN's creative style is *"Veni, vidi, vici"*. He addressed himself to the problem, exhaustively solved it, and switched to another problem.

### *4.2. Teacher*

Unlike Landau, NN never erected a barrier between himself and a neophyte in the form of sophisticated entrance examinations. As seen from the afore-cited fragments of my formation as a scientist, he valued not the initial

[5] In parentheses, we indicate the number of publications.

[6] The above quotation is from my article published with other collected papers [19] dedicated to the 50th anniversary of the Bogolyubov Laboratory of Theoretical Physics.

background but rather the ability to enter promptly into the range of new ideas and especially the ability to carry on original work. I repeat that, in the episode with the diploma thesis, NN "threw me to learn science swimming" straight in deep water. According to the legend, Rutherford followed the same practice. However, in the case of failure, NN never repudiated the novice but gave him a simpler problem to deal with. Partly for this reason, the core of Bogolyubov's scientific school in quantum field theory formed quite soon, in the second half of the 1950s.

The decisive element of his teaching was scientific generosity: we published the first three papers on the renormalization group in the *Doklady Academii Nauk SSSR* (1955) [6, 8, 12] under two our names. However, having thoroughly analyzed both ultraviolet and infrared asymptotics within QED and concluding that the argumentation of Landau and Pomeranchuk [3] on the zero charge lacked a proving power, NN's interest in the renormalization group slightly cooled down and, assigning me a task in the meson–nucleon theory, he switched to other problems (see the previous section). And he decidedly refused to be a co-author of the next publication [20].[7]

Another teaching method was the involvement of a young colleague into a large activity such as the joint work on a book. Finally, the third method of cultivating independence consisted in the accelerated training of the young co-author in the art of reporting on a joint research. For example, apart from seminars, I had to make a joint report [21] at the 3rd Mathematical Congress in Moscow in 1956 and a review [22] at the Rochester Conference in Kiev in 1959. In the latter case, the replacement of the speaker came as a surprise from NN just the day before.

The above example of minimum co-authorship illustrates a constituent of scientific scrupulousness of Nikolai Nikolaevich. As a second constituent, I would mention his high (first seemed unreasonable) demands for the thorough citing of predecessors in one or another scientific topic. Finally, the third constituent is the responsibility for literally each line in the scientific paper.

My long experience of co-authorship with NN has made me inclined (which is sometimes noticeably burdensome for me and my co-authors) to clear comprehension and maximum clarity of the formulations in reasoning and results as well as to the clear indication of the reasons why this or that paper is mentioned.[8]

Finally, a few words about human scrupulousness. I do not remember a case where I had to experience any pressure from Nikolai Nikolaevich though he was not only the scientific leader but also the higher-ranking administrative leader. NN usually just only offered a scientific idea or some practical solution to the colleague. He did it gently and never insisted if no positive response was given. "From each according to his abilities". This happened to me when he looked for an assistant in organizational efforts during the establishment of the Laboratory of Theoretical Physics at JINR in 1956 and when a came to LTP again in the early 1970s. This happened many times with me and his other students in scientific topics. NN usually foresaw the way, in which topical scientific ideas would develop, and advised to us to deal with one or another issue. Remembering these cases, we regret that, being carried away at that moment by something else (less important, as time showed), we paid no due attention to his recommendations.

Bogolyubov was not indifferent to personal qualities of young people whom he favored. NN valued the congenial human environment, the moral climate among his co-workers. My memory keeps two cases of ostracism. One of them concerned the then young scientist Y, very gifted but already too free and easy with the colleagues (working on similar problems). A glance at the list of NN's works shows that he often got several people involved in solving a problem. Friendly relations between them were a norm. In the case with Y, however, after several conflicts, the people had to turn to the chief. And he debarred Y from that work. The second episode involved the older colleague Z with a difficult biography warped by repressions of the 1930s. Once during a regular tightening of ideological nuts in the 1970s, Z became a witness of "seditious" political statements in a not very narrow circle of the Physics Department staff members. Fearing than any of other witnesses could report it to the State Security, he did it himself. And this became officially known. The chief was quick to react. Remarkably, NN understood that action from the pure human point of view. He understood, felt sympathy inwardly, and explained the reasons through an old Indian parable. But he did not ever want to deal with this man with whom he fruitfully collaborated for about 20 years.

[7] Already the next year, I applied this "minimum co-authorship" rule to the diploma work of my first graduation students Ilya Ginzburg and Lev Soloviev.

[8] The consequences are dislike to "common graves" in citing and cases of leaving the team of authors because of disagreement on an important element of the joint research.

Most important moral lessons of Nikolai Nikolaevich were learnt not from his reprimands but rather his behavior and the way of actions. So his ability to combine scientific work with civic duty, including scientific and administrative positions, served a good example for some of his outstanding disciples (including 9 Russian and 6 Ukrainian Academicians).

This became apparent in a very complicated post-Soviet period. Unlike many prominent Soviet scientists, representatives of the Bogolyubov School served and will continue serving their Motherland. Thanks to them, the spirit of Nikolai Nikolaevich is still among us.

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