MEMORIES ABOUT THE JOINT WORK WITH M.M. BOGOLYUBOV

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In this article, I want to tell about one of the episodes of my long-term communications with M.M. Bogolyubov. This period lasted for one year, when our work "Mathematical description of equilibrium states of classic systems on the base of canonical ensemble formalism" was written. This work was published in the "Teoret. Matem. Fiz." journal in 1969 and as a preprint at the Institute for Theoretical Physics with the same title[1].

My acquaintance with M.M. Bodolyubov started at my student years. Associate Professor O.I. Sirakomskyi gave us the course of non-linear mechanics by the books of Krylov and Bogolyubov. Professor O.S. Parasiuk gave us the course of qualitative differential equations by the books of Nemytskii and Stepanov, and this is how I familiarized the theorem of Krylov and Bogolyubov on the existence of the invariant measure of dynamical systems.

My friends who were theoretical physicists studied M.M. Bogolyubov's monograph "Problems of Dynamical Theory in Statistical Physics". They studied the equations for correlation functions which are nowadays known as BBGKY hierarchy. As is well known, this is a chain of equations for the infinite sequence of correlation functions. This chain can be written in the form of one equation in the variation derivatives for the generating functional. As I can remember, the students who were theoretical physicists of the fourth year already spoke about the second quantization, Feynman diagrams, continual Feynman's integral. The students of the same year who were learning mathematics and mechanics knew nothing about these mathematical discoveries. I am absolutely confident that the students training in theoretical physics get wider and deeper education than pure mathematicians. My experience, for more than thirty years, of teaching the students learning theoretical physics and mathematical physics confirms this assertion.

During my study in the graduate school (my scientific adviser was O.S. Parasiuk), there were published the books by Bogolyubov and Shirkov on quantum field theory and by Bogolyubov, Medvedev, and Polivanov on dispersion relations. O.S. Parasiuk had these books even in manuscripts and read the special courses based on them. These ones became my reference books, I studied them in great details, and now I know them perfectly. At the beginning, I understood some chapters only in a formal way from the mathematician point of view, for example, the chapter on second quantization. Later on, I filled this gap. My firm belief is that nobody can be considered a specialist on quantum field theory without deep knowledge of these books.

By this time, I regularly met with Mykola Mykolaevych. He often visited Kiev, where his mother lived. During his visits, he was spoken on seminars in the Institute of Mathematics, he was spoken about superconductivity theory on the base of Fröhlich's Hamiltonian and the principles of compensation of unsafe diagrams. By that time, I have already understood that I see the legendary personality and a scientist of genius in front of myself.

By the way, M.M. Bogolyubov was always smartly dressed in a well-sewed costume and the patent-leather shoes. In summer, he dressed the tussore costume of the colonial type with short sleeves.

I well remember my speeches in front of M.M. Bogolyubov. The first time, it was on the international conference at Dubna. That time, the latest fashion was the investigation of analytical properties of the scattering matrix by the energy, momentum transferred, and orbital moment. By that time, I obtained serious results on the analytical properties of contributions of Feynman's diagrams. Using the theorems of the theory of holomorphic functions of many complex variables, I proved the criteria of validity of Mandelstam's representation by the behavior of Landau's curves. I made the speech

[1] This work was also published in the Proceedings of Séminaire Jean Leray (Collège de France, Paris) No.3, 1969, p. 47–73. Nowadays the article is reissued in the book "N.N. Bogolyubov. Collection of Scientific Works", in 12 vol., Moscow: Nauka (Classics of Science), 2006, vol. 6, p. 401 and in the jubilee edition of Ukrainian Journal of Physics "Golden Pages of the Ukrainian Physics", (vol. 53, 2008, p. 168).

on the first day right after the Todorov's speech, which was the first one. The head of the meeting was Mykola Mykolaevych.

My speech was of a great success. Mykola Mykolaevych listened carefully and supportively nodded to me. After the speech when I told my fellows from Kiev about the attentiveness of Mykola Mykolaevich to me, they said laughing that probably that is because Mykola Mykolaevych was afraid that something could happen to me, because all the time during the speech I was holding my left hand on my heart. I still don't know if it was the truth or a joke.

My second speech was on the O.S. Parasiuk's seminar. At the beginning, M.M. Bogolyubov made a short presentation, and then I summarized a short journal paper on the reggistics. After that Ostap Stepanovych told me that Mykola Mykolaevych liked my speech and that he asked Ostap Stepanovych if he has any other students so well prepared. The answer was positive. Ostap Stepanovych Parasiuk with his disciples got the separate department on theoretical physics.

My third speech in front of Mykola Mykolaevych happened at Kaniv, where the first mathematical summer school was held. I also remember my speech on the session of the Division of Mathematics of Academy of Sciences of USSR in Moscow, when Mykola Mykolaevych praised my results. That session was attended by Yu.O. Mytropol's'kyi and A.M. Samoilenko. Later on, there were many speeches, but I will not remark them here.

In 1966, the Institute for Theoretical Physics was established. This was Mykola Mykolaevych's idea. He chose the staff for this institute himself. O.S. Parasiuk headed the department of mathematical methods in theoretical physics. Mykola Mykolaevich and Ostap Stepanovych selected me, V.P. Gachok, A.U. Klimyk, V.A. Yatsun, and I.M. Burban for this department. Other staff of the theoretical physics department left at the Institute of Mathematics. Later on, they bragged that they were the big patriots of the Institute of Mathematics and that's why they didn't pass to the Institute for Theoretical physics.

I note that I began to be interested in statistical mechanics spontaneously. Maybe, some role in this was played by the fact that my education is half mathematical and half mechanical. I was really pulled into statistical mechanics. I studied the monographs and books on statistical mechanics, and I had a solid base in the quantum field theory and functional analysis. I was particulary attracted by the chain of equations for the correlation functions derived by Mykola Mykolaevych (nowadays, these equations are known as BBGKY hierarchy).

I was able to communicate directly with M.M. Bogolyubov. By the time of discussing my PhD, I drew the attention of Mykola Mykolaevych to the fact that the equations for coefficient functions of the scattering matrix, that I derived, are similar to his chain of equations for correlation functions. He told me that long time ago, after the war he and B.I. Khatset derived some results on the existence of equilibrium correlation functions. He expressed his desire for me to look through these results. That was June 1967.

M.M. Bogolyubov's results were presented in a short note joined with Khatset in Dokl. Akad. Nauk USSR and in the Khatset's article in "Zhytomyr Pedagogical Institute's Bulletin" in more details. In the note, only the results were formulated, and no proofs were given. In the "Bulletin," there were results on the existence of a solution of Mayer–Montroll equations for the equilibrium correlation functions in the limits of a canonical ensemble, but no proof of the existence of the thermodynamical limit, i.e. the convergence of a sequence of correlation functions of a finite system to the corresponding sequence of an infinite system, was given. Moreover, only a positive (i.e. repulsive) interaction potential was discussed.

When I looked through these works, I was impressed that Mykola Mykolaevych forestalled the French scientist D. Ruelle for 15 years. D. Ruelle considered the equilibrium classical system in the framework of a simpler grand canonical ensemble. However, he considered a physically more suitable stable potential that he introduced for the first time. This potential also contained the repulsion. Instead of the Mayer–Montroll equations, he considered the equations of Kirkwood–Salzburg. He proved that the solution of these equations exists at the same Banach space that Bogolyubov introduced for the Mayer–Montroll equation. He succeeded to prove the existence of the thermodynamical limit for the sequence of correlation functions.

A couple of words on the matter of the problem. Let us consider the equilibrium system of N particles with the two-body interaction potential $\Phi(q)$, $q \in \mathbb{R}^3$ which are positioned in some domain (a sphere, for example) Λ with the volume V and with the inverse temperature β. The probability distribution density is given in Λ^N by the function

$$Q^{-1}(N,V)\, e^{-\beta \sum\limits_{i<j=1}^{N} \Phi(q_i - q_j)},$$

where

$$Q(N,V)=\int\limits_{\Lambda^N} e^{-\beta\sum\limits_{i<j=1}^{N}\Phi(q_i-q_j)}dq_1\dots dq_N.$$

The sequence of correlation functions, $s\geq 1$, is given by

$$F_s^{(N)}(q_1,\dots,q_s)=\frac{N(N-1)\dots(N-s+1)}{Q(N,V)}\times$$

$$\times\int\limits_{\Lambda^{N-s}} e^{-\beta\sum\limits_{i<j=1}^{N}\Phi(q_i-q_j)}dq_{s+1}\dots dq_N.$$

Let the potential satisfy the conditions of integrability

$$\int|e^{-\beta\Phi(q)}-1|dq=c(\beta)<\infty$$

and stability

$$\sum_{i<j=1}^{N}\Phi(q_i-q_j)\geq -BN,\quad B>0,\quad N\geq 1.$$

It is not difficult to see that $Q(N,V)\sim V^N$. Then $F_s^{(N)}(q_1,\dots,q_s)\sim\frac{V^N}{V^N}$, and we need to prove the existence of the limit of correlation functions as $V\to\infty$, $N\to\infty$, $\frac{N}{V}=\frac{1}{v}$.

I quickly understood that, for getting the analogous results in the frame of a canonical ensemble, one should use the Kirkwood–Salzburg equations instead of the Mayer–Montroll equations. I derived the Kirkwood–Salzburg relation for the finite system.

I note the fact that a canonical ensemble is more complicated than the grand canonical ensemble, because the Kirkwood-Salzburg equations are true for the latter and for the finite systems.

These relations have the form

$$F_s^{(N)}(q_1,\dots,q_s)=$$

$$=a_N(V)e^{-\beta\sum\limits_{i=2}^{s}\Phi(q_1-q_i)}\big(F_{s-1}^{(N-1)}(q_2,\dots,q_s)+$$

$$+\sum_{k=1}^{N-s}\frac{1}{k!}\int\limits_{\Lambda}dy_1\dots dy_k\prod_{i=1}^{k}(e^{-\beta\Phi(q_1-y_i)}-1)\times$$

$$\times F_{s-1+k}^{(N-1)}(q_2,\dots,q_s,y_1,\dots,y_k)\big),\quad s\geq 1,$$

where $F_0=1$, $a_N(V)=N\frac{Q(N-1,V)}{Q(N,V)}$, $|\Lambda|=V$, or, in the abstract form,

$$F^{(N)}=a_N(V)\mathcal{K}^N F^{(N-1)}+a_N(V)F_0,$$

where $F^{(N)}=(F_1^{(N)}(q_1),\dots,F_N^{(N)}(q_1,\dots,q_N),0\dots)$, $F_0=(1,0,\dots)$. If we move to the thermodynamical limit in a formal way, then we will have

$$F_s(q_1,\dots,q_s)=a(v)e^{-\beta\sum\limits_{i=2}^{s}\Phi(q_1-q_i)}\big(F_{s-1}(q_2,\dots,q_s)+$$

$$+\sum_{k=1}^{\infty}\int dy_1\dots dy_k\prod_{i=1}^{k}(e^{-\beta\Phi(q_1-y_i)}-1)\times$$

$$\times F_{s-1+k}(q_2,\dots,q_s,y_1,\dots,y_k)\big),\quad s\geq 1$$

or, in the abstract form,

$$F=a(v)\mathcal{K}F+a(v)F_0,$$

where $a(v)=\lim\limits_{N\to\infty,V\to\infty,\frac{N}{V}=\frac{1}{v}}a_N(V)$. My main contribution to our common work with M.M. Bogolyubov and B.I. Khatset consists in the use of Kirkwood–Salzburg equations for the canonical ensemble and proving the existence of the thermodynamical limit, i.e. the fact that the sequence $F^{(N)}$ converges in some sense to F as $N\to\infty$, $V\to\infty$, $\frac{N}{V}=\frac{1}{v}=\text{const}$.

This problem is an order of magnitude harder than the one in D. Ruelle's work, because the operator $\mathcal{K}^{(N)}$ differs from $\mathcal{K}$. Moreover, we need else to prove, at the same time, the existence of the thermodynamical limit $\lim\limits_{N\to\infty,V\to\infty,\frac{N}{V}=\frac{1}{v}}a_N(V)=a(v)$. In addition, the self-consistence conditions were stated

$$\lim_{V\to\infty}\frac{1}{V}\int\limits_{\Lambda}F_{s+1}^{(N)}(q_1,\dots,q_s,q_{s+1})dq_{s+1}=F_s(q_1,\dots,q_s).$$

The validity of virial expansions was proven, i.e. the convergence in the density for series which represent the correlation functions, as well as the equivalence of the canonical and grand canonical ensembles.

I want to say a couple of words about the conditions of writing that work. Most pressure was fallen on me. But I was familiar with D. Ruelle's work, studied B. Khatset's article from "Zhytomyr Pedagogical Institute's Bulletin," and my experience in quantum field theory and functional analysis allowed me to feel myself free. All the stages of work were reported to Mykola Mykolaevych, when he was in Kiev or when I visited him at Dubna. The work was finished till the new year. Khatset helped me to put the article in order. He was a great master to formulate the results. Probably, he inherited this skill from his father who was a well-known lawyer in Kiev. A part of results of the previous M.M. Bogolyubov's work organically fitted to our common work.

Mykola Mykolaevych went to Paris with the manuscript. He reported the results in Sorbonne and Ecole Normale. His reports were visited by D. Ruelle, who told Mykola Mykolaevych that he didn't know about his work with Khatset and was not able to read the article in "Zhytomyr Pedagogical Institute's Bulletin," because there was no Ukrainian-to-French interpreter. Mykola Mykolaevych ironically called his attention to the fact that he had conversations in Ukrainian in every government institution in Paris.

Later on, Mykola Mykolaevych called my attention to the fact that I should make a correction in one of the lemmas especially because it was needed for the fulfilment of the consistency condition. I was very downtrodden since I polished the article for a long time and believed it was perfect. I asked Mykola Mykolaevych if there were any problems during his reports in Paris. He laughed and answered: "I sold this thing to them in a perfect way." And then he added that he had some vacant evenings in Paris, and he read my manuscript very carefully and in great details. He said then: "If I read something then I do it as I supposed to".

I expanded the formulation of the lemma very rapidly. Then I went to Dubna, where Mykola Mykolaevych agreed with everything. He really insisted on his conditions of consistency:

$$\lim_{N\to\infty, V\to\infty, \frac{N}{V}=\frac{1}{v}} \frac{1}{V}\int\limits_{\Lambda} F_{s+1}^{(N)}(q_1,\dots,q_s,q_{s+1})dq_{s+1} =$$

$$= F_s(q_1,\dots,q_s),$$

$$\lim_{V\to\infty} \frac{1}{V}\int\limits_{\Lambda} F_{s+1}(q_1,\dots,q_s,q_{s+1})dq_{s+1} = F_s(q_1,\dots,q_s).$$

M.M. Bogolyubov considered that these conditions together with the existence of a state which is given by the sequence of correlation functions $F = (F_1(q_1),\dots,F_s(q_1,\dots,q_s),\dots)$ define the measure on the configuration space of the unbounded system. Indeed, $F_\infty(q_1,\dots,q_s,\dots)$ is a measure, and all the correlation functions for a finite number of particles are defined by the measure F_∞ by the consistency condition. I note that the consistency conditions can be independently obtained from the cluster properties (Bogolyubov's weakening of correlations) which were proven for the first time by D. Ruelle.

Mykola Mykolaevych gave a permission to print the paper in English. When it came out of printing, he sent it to J. Leray and wrote in the accompanying letter that this is one of his best results in statistic mechanics made together with his co-workers D.Ya. Petrina and B.I. Khatset. Mykola Mykolaevych attached a big importance to this result. This fact is witnessed by the inclusion of the article to the three-volume edition of selected works of M.M. Bogolyubov (see a footnote).

Our article was published in the second issue of the first volume of the "Teoret. Matem. Fiz." journal in 1969 [1]. It is widely referenced in journals and monographs. Probably, it is on a second place by the reference amount after the article of M.M. Bogolyubov and O.S. Parasiuk on the substracting procedure.

I also want to call attention to the absolutely ridiculous speculations about what are the states of infinite systems – sequences of correlation functions or probabilistic measure. By the way, our article is named "Mathematical description of equilibrium state of classical systems based on the canonical ensemble formalism". As a matter of fact, these notions are equivalent. If there exists a sequence of correlation functions which satisfies the Kirkwood–Salzburg or Mayer–Montroll equations then there exists a probabilistic measure, and, conversely, if there exists a probabilistic measure which satisfies the Dobrushin–Lanford–Ruelle equation, then there exists a sequence of correlation functions which satisfies their equations.

Another thing is that the existence of solutions of the equations for correlation functions is proven, but there are no analogous results for the measure. Averages of observables are directly defined by the correlation functions. The phase transition problem is also related to the properties of correlation functions. All these results are expounded in our monograph written jointly with V.I. Gerasymenko and P.V. Malyshev "Mathematical Foundations of Classical Statistical Mechanics" which passed two editions in English [4].

Let me call attention to that Mykola Mykolaevych didn't like the excess generalization or shallow "bourbakism." When, in the first edition of the manuscript, I wrote that the potential is measurable, he became angry and said that this is a shallow generalization, and the sound mind says that it is enough to consider the potential as continuous with possible singularities. My hard tries to put the trinity (Ω,σ,P), which was made a fetish by our probabilists, in the article were rejected. Mykola Mykolaevich said that everything is trivial in our case, because the measure is absolutely continuous with respect to the Lebesgue measure, and the domain Λ is a sphere. By the way, Mykola Mykolaevych don't use this fetish in any of his articles on statistical mechanics. Neither we did nor D. Ruelle.

In conclusion, I want to say a couple more words about two equivalent definitions of states, F and μ_Δ.

State F is the sequence of correlation functions $F = (F_1(q_1), \ldots, F_s(q_1, \ldots, q_s), \ldots)$. Here, $F_s(q_1, \ldots, q_s)dq_1 \ldots dq_s$ is the probability of finding s particles in volumes $dq_1 \ldots dq_s$ with centers at $q_1, \ldots, q_s$ which belong to the space $\mathbf{R}^3$ under the condition that all the other particles are situated arbitrarily in $\mathbf{R}^3$.

State μ_Δ - is the sequence of the probability distribution densities $\mu_\Delta = (\mu^1_\Delta(q_1), \ldots, \mu^s_\Delta(q_1, \ldots, q_s), \ldots)$. Here, $\mu^s_\Delta(q_1, \ldots, q_s)dq_1 \ldots dq_s$ is the probability of finding s particles in volumes $dq_1, \ldots, dq_s$ with the centers at $(q_1, \ldots, q_s) \in \Delta^s$ under the conditions that all the other particles are situated outside of the domain Δ. For μ_Δ, we have the following formula which expresses it through the sequence F:

$$\mu^s_\Delta(q_1, \ldots, q_s) =$$

$$= \frac{1}{s!} \sum_{n=0}^{\infty} \frac{(-1)^n}{n!} \int_{\Delta^n} dq_{s+1} \ldots dq_{s+n} F_{s+n}(q_1, \ldots, q_{s+n}),$$

$s \geq 1$.

For μ_Δ, we have the conditions of consistency

$$\mu^s_\Delta(q_1, \ldots, q_s) = \sum_{n=0}^{\infty} \frac{(s+n)!}{s!n!} \int_{\Delta' \setminus \Delta} dq_{s+1} \ldots$$

$$\ldots \int_{\Delta' \setminus \Delta} dq_{s+n} \mu^{s+n}_{\Delta'}(q_1, \ldots, q_s, q_{s+1}, \ldots, q_{s+n}),$$

where $\Delta \subset \Delta'$.

Thus, the sequence $\mu^s_\Delta(q_1, \ldots, q_s)$ is specified everywhere in Δ^s, and the sequence $F_s(q_1, \ldots, q_s)$ is specified everywhere in $\mathbb{R}^{3s}$. That is, the corresponding measures are absolutely continuous with respect to the Lebesgue measure.

Let us consider the formula for determining the Gibbs measure of the domain Λ^n in accordance to the article by R.A. Minlos [Funkts. Anal. Prilozh., pp.60–73, 1967, formula (16)] in our notations:

$$P(\Lambda^n) = \sum_{l=n}^{\infty} (-1)^{l-n} \frac{l!}{n!(l-n)!} \frac{1}{l!} \times$$

$$\times \int\limits_{\Lambda^l} \rho_l(x_1, \ldots, x_n, x_{n+1}, \ldots, x_l) \times$$

$$\times dx_1 \ldots dx_n dx_{n+1} \ldots dx_l$$

$$= \sum_{p=0}^{\infty} (-1)^p \frac{1}{n!} \frac{1}{p!} \int\limits_{\Lambda^{n+p}} \rho_{n+p}(x_1, \ldots, x_n,$$

$$x_{n+1}, \ldots, x_{n+p})\, dx_1 \ldots dx_n dx_{n+1} \ldots dx_{n+p} =$$

$$= \frac{1}{n!} \int\limits_{\Lambda^n} \mu_\Lambda(x_1, \ldots, x_n)\, dx_1 \ldots dx_n.$$

From this formula, it is not difficult to see that the measure $P(\Lambda^n)$ is determined by the ordinary Lebesgue integral of a common function $\mu_\Lambda(x_1, \ldots, x_n)$ which is measurable at Λ^n. There are no discrete sequences, on which the mentioned measure $\mu_\Lambda(x_1, \ldots, x_n)$ is supposedly determined.

I note that, in all the monographs and journal articles, the Hamilton dynamics of infinite systems, the BBGKY hierarchy for correlation functions, and the integral DLR (Dobrushin–Lanford–Ruelle) equations for the Gibbs measure are given by the usual differential equations or integro-differential equations, instead of some difference equations built on the complete misunderstanding of the case.

Sometimes, some less educated individuals say something about Gibbs measures defined on finite configurations. This is the complete misunderstanding of the essence of the case. The probability for the domain Λ to have more than m particles satisfies the estimate

$$P(m|\Lambda) \leq e^{-\frac{\alpha m^2}{L^3} + \gamma m},$$

where $\alpha > 0, \gamma > 0$ are constants, and L is the diameter of Λ. This estimate is interpreted as the fact that Gibbs measures are situated on finite configurations, because $\lim_{m \longrightarrow 0} P(m|\Lambda) = 0$ when $L < \infty$. I accent one more time that Gibbs measures are defined in the whole $\mathbb{R}^{3\infty}$ and concentrated on finite configurations. That's why the definition of some difference operators for the Gibbs measures is senseless.

To make things even more clear, I'm giving an example of the Gauss distribution, $e^{-\beta \sum_{i=1}^{\infty} q_i^2}$. This measure is defined on the whole $\mathbb{R}^{3\infty}$ and concentrated on l_2, i.e. on the sets $\sum_{i=1}^{\infty} q_i^2 < \infty$.

The only excuse to these individuals can be a carelessness in formulations, because the estimate for $P(m|\Lambda)$ is being expressed sometimes by the following words: the Gibbs measure is defined on finite configurations. But the correct way of saying is: it is concentrated on finite configurations.

At the end, I want to say that Mykola Mykolaevych's ideas on the BBGKY hierarchy, on the existence of its solutions in the thermodynamical limit, on proofs of the derivation of kinetic equations are still developed, mostly in Kiev. Besides the great amount of articles, there are four monographic reviews published in the journal "Uspekhi Matem. Nauk" and the series of books "Soviet Scientific Review, sect. C: Mathematical Physics" [6–9], seven monographs, five of which are in English [2–5] (monograph [5] was published in the past year). Reviewers from Mathematical Review say that our monographs give the totals on researches of the BBGKY hierarchy for the past fifty years and recommend them as encyclopedias on these questions.

Thus, the heritage of M.M. Bogolyubov is a great acquisition of modern mathematical and theoretical physics.

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СПОГАДИ ПРО СПІЛЬНУ ПРАЦЮ
З М.М. БОГОЛЮБОВИМ

Д.Я. Петрина

Р е з ю м е

У цій статті я хочу розповісти про один епізод із мого довголітнього спілкування з М.М. Боголюбовим. Цей період тривав один рік, коли писалася спільна з М.М. Боголюбовим робота "Математическое описание равновесного состояния классических систем на основе формализма канонического ансамбля". Цю роботу було опубліковано в журналі "Теоретическая и математическая физика" в 1969 р. та в препринті з такою ж назвою англійською мовою в Інституті теоретичної фізики. (див. зноску).