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FROM THE HISTORY OF PHYSICS

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A wonderful story about a remarkable paper by Landau

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In 1958 the Institute of Physical Problems celebrated the Landau's fiftieth birthday. None of those present will ever forget the brilliance of the sharp wit which reigned at the celebration. Among the numerous unusual things the hero of the day was presented with were the key formulae of his ten most important papers† engraved on marble tablets. However, they bore no hint of the hydrodynamic theory of multiple hadron production (in a single high-energy nucleus or hadron collision [1, 2]). Meanwhile Landau himself used to say (to me among others) that he had spent more effort on this than on any other of his works. Moreover, now, this work has been very popular for over twenty years, it has been employed by experimenters and has been intensively developed by various groups of theoreticians all over the world. It has given birth to perhaps hundreds of papers. Why then was it not even mentioned by Isaac Konstantinovich Kikoin who gave the tablets on behalf of Kurchatov Institute? This can be explained by the wondrous fate of this paper in the world of physicists.

We shall begin with the pre-history. Before the War, when the pion was not yet known to exist and the muon discovered in cosmic rays had been taken for the nuclear meson, this identification was found to be erroneous: the muon easily penetrates through the atmosphere, i.e., is a weakly interacting particle and cannot therefore maintain nuclear forces. On the other hand, it was also in cosmic rays that important evidence was found in favor of the fact that primary particles (and a lot of secondary ones) give birth to many particles in a single collision with atomic nuclei of the air. How could this all be brought into agreement?

At that time some physicists were fascinated by the modified Fermi theory of beta-decay proposed by Konopinskiĭ and Ulenbek. The theory was characterized by the presence of derivatives of wave (operator) functions of an arbitrarily high order s, $\gamma_\mu \partial^s \varphi / \partial x_\mu^s$, in the Lagrangian. But if $\varphi \sim \exp(\mathrm{i}px)$, each differentiation implies a multiplication by p. Even if the coupling constant g is weak, terms $g^2(p/m)^{2s}$ (m is the mass of a single particle) appear which for large p and s effectively lead to a strong interaction. Two years later the theory was disproved and forgotten. But having used it, Heisenberg formulated the theory of multiple generation: in a single collision of particles in the case $g(p/m) \gg 1$ many particles are produced. If the latter inequality holds for these newly created particles, a second cascade will be observed. The multiplication will stop as soon as we have $g(p/m) \lesssim 1$ [3]. I have said all this for the sake of the following quote from Heisenberg. "Clearly," he said, "the final distribution by momenta of generated particles will be defined by Planck's black-body radiation function". Regarding this assertion as obvious, he did not clarify it. But an important idea was expressed: when, even proceeding from the quantum-field operator equation, one obtains many weakly interacting (by the moment the duplication has stopped) particles, classical thermodynamical considerations can be applied to them .

After the failure of the theory due to Konopinskiĭ and Ulenbek, the work by Heisenberg was forgotten, too. Landau did not know about this work. Some time later, Heisenberg created quite a different theory of multiple generation of particles proceeding from the nonlinear wave equation [4]. G A Milekhin [5] later proved, to the great satisfaction of Heisenberg (I was a witness and a participant in the discussion), the principle identity of Heisenberg's work and that of Landau which appeared soon after, but seemed outwardly absolutely different.

But in 1951, when pions were already known, the paper by Fermi [6] came out. He was also aware of the fact that in the case of a 'head-on' collision between two strongly interacting hadrons or high energy nuclei, they stop in the center-of-mass system (CMS) and release all their energy into a small volume (a Lorentz-contracted proper volume of the two particles) thus producing matter with a very high energy density and temperature. This matter then fragments into a lot of particles whose number and momentum distribution are determined by classical thermodynamics. Fermi obtained all this.

However, neither Fermi nor Pomeranchuk who noticed his mistake [7] were apparently aware of Heisenberg's work. But it occurred to Pomeranchuk that particles participating in a strong interaction and escaping from a small volume would interact and go on producing new particles, transforming into one another and annihilating until they found themselves at distances exceeding the effective interaction radius $r \sim m_\pi^{-1}$ (m_π is the pion mass). The temperature T of this final state, its entropy, the number of particles, etc. can be readily calculated. Only one parameter r was involved which was known to an order of magnitude, or through the related temperature T_f. The energy dependence of multiplicity, $n \sim E^{1/2}$ ($2E = \sqrt{s}$ is the total energy of two primary particles in CMS) obtained by Pomeranchuk differed from that due to Fermi, $n \sim E^{1/4}$.

The matter expanding to the final state cannot, clearly, be regarded as an ensemble of individual hadrons. To the emphatic question, "What is this?", Pomeranchuk would answer, "Well, think of it as a boiling operator liquid".

Now it was the turn of Landau to enter the game. He was aware that for a large number of particles and a high energy density in a certain continuous matter one can apply classical thermodynamics and, therefore, hydrodynamics which both

† A complete list of L D Landau's works is given at the end of this article.

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have the same criterion of applicability. One need only allow for the relativism because hydrodynamics should be relativistic. This had already been formulated in his and E M Lifshitz' course of the theory of continuous media. Now it found its first application. The equations merely had to be supplemented with the equation of state and solved proceeding from an initial volume (the Lorentz-contracted volume of the primary particles that stopped) and the total particle energy. Landau assumed the equation of state to be the same as in the case of an ultra-relativistic electron-positron gas,

$$p = c_0^2 \varepsilon \,, \qquad c_0^2 = \frac{1}{3} \,, \tag{1}$$

where p is the pressure, ε is the energy density, and c_0 is the velocity of sound. Many years later Shuryak [8] made the same calculation according to Bethe – Ulenbek and involved all possible resonances to obtain $c_0 \sim 1/5$, which was in better agreement with the data of the time on multiplicity in the region $\sqrt{s} \sim 5-7$ GeV. But here the multiplicity is not yet very large, and we do not arrive at the final result.

Landau considered a simplified version of the problem: from the initial pancake state the matter expands cylindrically until the transverse expansion becomes significant. Then a free conical spread of non-interacting particles occurs. Nevertheless, consequent papers, in which the hydrodynamic problem of three-dimensional expansion was solved far more rigorously (by Milekhin [9] and much later in a different way by Shuryak [10]) and the thermal particle motion superimposed on the hydrodynamic flow was taken into consideration, yielded results very close to that obtained by Landau.

It is noteworthy that immediately after the appearance of Landau's paper some important corrections were made. Firstly, Khalatnikov [11] specified the solution for the one-dimensional cylindrical spread. Secondly, Belen'kiĭ [12] gave a more detailed analysis of the initial collision process involving real dimensions of primary particles. Assuming that for the parent matter the same equation of state holds as for the primary particles, he examined the propagation of shock waves excited by the first touch of colliding particles up to the moment when their total mass is involved in the process. Thus he specified the dimension of the initial state after which a hydrodynamic spread develops. Finally, Chernavskiĭ [13] allowed for the fact that the spread should contain a so-called running wave.

The theoretical results that followed immediately were in good agreement with the then available, although very fragmentary experimental data (which were then only obtained in cosmic ray studies) both for the dependence of the multiplicity on the primary energy and for the transverse momenta (independent of the primary energy) of the final particles.

Now that we know the hadron vacuum to be a material medium with a certain energy density and pressure $p = |\varepsilon|$, the process can be viewed differently. One can say that in a collision of Lorentz-contracted particles a very dense energy flow comes rapidly into a very small volume of vacuum. The vacuum boils up to an extremely high temperature (in terms of field theory it is described by a Fock column with an exceedingly large number of rows, that is, with the participation of an exceedingly large number of virtual particles), and then, when expanding, it cools off until the low concentration of particles allows them to become physical.

However, right after the creation of the Landau theory theoreticians were deeply pessimistic in respect of the applicability of the field theory in general and to strong interactions in particular. Since the classical consideration proposed by Landau was an approximation to the field theory (a large number of particles corresponds to high excitations of the system, i.e., to large quantum numbers when the classical approximation holds according to the correspondence principle), the inapplicability of the field theory implied the inapplicability of the classical approximation.

As for the pessimism, it was primarily caused by the discovery of the so-called 'Moscow zero' in the renormalizable field theory. It became clear that in a more thorough consideration the renormalization led to the conclusion that the higher the cut-off energy, the weaker the interaction for moderate energies. This made the Hamiltonian field theory inapplicable. True, the result was not obtained exactly, and some approximations were used. That is why some prominent theoreticians went on developing the quantum field theory, but even Landau himself declared [14] that "the Hamiltonian method for strong interactions has become outdated and ought to be buried, but, of course, with all due honors" and that employment of the coordinate-dependent operator wave functions themselves was inadmissible. The overwhelming majority of theoreticians all over the world shared this viewpoint.

Discussing the situation in his last paper [14] Landau stated: "The 'nullification' of the theory has now been implicitly recognized even by the theoreticians who go on criticising it formally". Such a conclusion could indeed have been drawn because the ideology of the Heisenberg S-matrix admitting a consideration of only free particles — before and after their interaction — was adopted by most theoreticians as a reliable basis for possible studies. In this context Landau referred to his paper with Paerls: "Almost 30 years ago Paerls and I pointed out that according to the relativistic quantum theory, no quantities characterizing interacting particles can be measured, and the only measurable quantities are the momenta and polarization of free particles".

The fifteen-year epoch of a diverse search for a theory involving only free-end diagrams etc. began. Its physical basis should have been formed only by "the unitarity relation and the locality principle" [14]. The intensive attempts made at that time included the study of the analytical properties of such diagrams, the dispersion relations, the analytical S-matrix theory, the modification of the Redge method to the general complete theory which played an exceedingly useful role in the phenomenological analysis of processes with strong interactions, the study of the nonlinear field theories and even of the nonlocal theories rejected by Landau.

Curiously enough, one of the decisive arguments against the nonlocal theory in Landau's opinion was the fact that "the number of mesons produced in collisions at high energies coincided with the Fermi formula [6] (and, we add, with the hydrodynamic Landau theory, although for different reasons — E.L.F.) which calls for the application of statistical thermodynamic arguments to dimensions incomparably smaller than any possible smearing radius" (in the nonlocal theory — E.L.F.). Indeed, in cosmic rays such an agreement was achieved as soon as nucleons were Lorentz-contracted hundreds of times (i.e., to a thickness of the order of 10^{-15} cm).

These studies, which had been conducted by the most talented theoreticians for about 15 years, yielded many fruitful results in concrete problems but nevertheless failed to suggest a desirable complete theory. Moreover, the enthusiasm in respect of these 'antifield' approaches shaded a very important paper by Yang and Mills [15] that appeared at that time and for many years later underlay the contemporary gauge field theory. In 1960 Landau wrote: "It seems to me that the theory has lately made a notable progress in the indicated direction and we shall not wait long till the equations of the new theory are written" [14]. The new theory did appear eventually but along the lines of Lagrange's theory with the Hamiltonian, the direction that had been rejected by Landau. This was the theory of electro-weak interactions and generally of gauge fields.

Landau thus had a contradictory attitude towards his hydrodynamic theory. On the one hand, to the end he did believe it to be correct and useful. He would repeatedly answer so to those, including me personally, who asked him directly. On the other hand, he apparently considered that it must be a quasi-classical approximation to a new but not the old type field theory. But for a huge number of other theoreticians the theory did not seem to exist at all. During that dim period it was only discussed in a few papers by Japanese theoretical physicists (M Namiki, S Iso and K Mori, H Esawa, I Tomosawa and H Umesawa, and also by D Ito and H Tanaka) and in this country only a group of FIAN researchers (C Z Belen'kiĭ, D S Chernavskiĭ, E L Feĭnberg, N M Gerasimova, G A Milekhin, and I L Rozental') stubbornly advanced in this direction. There was another man, R Hagedorn in CERN, thought of as an eccentric, who developed his ideas of a thermodynamically statistical nature that were, in fact, close to the hydrodynamic theory, but of quite a different type (this was already in the mid-1960s). It was around 1968 that the young É V Shuryak (Novosibirsk) got engaged in this work. This was a brave deed of his because it was considered almost indecent among serious theoreticians to speak of the hydrodynamic theory. Indeed, the non-quantum classical thermodynamics and hydrodynamics were being treated in a volume dozens of times smaller than the nucleon volume! All that took place in the epoch of refined proofs of the depravity of even the quantum field theory, 'Moscow zero', Haag's theorem, etc.

But the energy of particles in accelerators gradually increased and so did their multiplicity in thoroughly investigated events of collisions of two high-energy protons (in the early 1970s it reached $n \sim 6$ on average in collisions with energy $\sqrt{s} \sim 60$ GeV in CMS), and one could no longer avoid the analysis of their distributions in transverse momenta p_t, in rapidities y and pseudorapidities η (the concept had long been exploited in cosmic ray studies and appeared automatically in the Landau theory). All the works on the development of the hydrodynamic theory led to a Gaussian distribution in y and η ($y \approx \eta$ for $y > 1$). Thus, for a collision of identical nuclei of atomic number A, Landau obtained the following distribution in η (m_{N} is the nucleon mass):

$$\frac{\mathrm{d}n}{\mathrm{d}\eta} = \frac{\langle n \rangle}{\sqrt{2\pi L}} \exp\left(-\frac{\eta^2}{2L}\right), \qquad L \approx \ln \frac{\sqrt{s}}{2Am_{\mathrm{N}}} . \tag{2}$$

A more consistent solution for a three-dimensional hydrodynamic spread of particles allowing for the thermal motion of the particles produced yielded almost the same result

$$\frac{\mathrm{d}n}{\mathrm{d}\eta} = \frac{\langle n \rangle}{\sqrt{2\pi L}} \exp\left(-\frac{y^2}{2L}\right) \tag{3}$$

with somewhat different L values: $L = L_m$ in Ref. [8] and $L = L_s$ in Ref. [9]. Furthermore, it turned out that

$$\frac{\mathrm{d}n}{\mathrm{d}p_{\mathrm{t}}} \propto \exp\left[-\frac{(m_i^2 + p_{\mathrm{t}}^2)^{1/2}}{T_{\mathrm{f}}}\right] p_{\mathrm{t}}^{3/2} , \tag{4}$$

where m_i is the mass of particles produced in the collision under study, T_{f} is the temperature at the moment of spread of finite free particles (the so-called 'frozen temperature').

These formulae make use of only two assumptions, namely, of the equation of state $\varepsilon = 3p$ (i.e., for $c_0^2 = 1/3$; a modification exists for any c_0) and of the value of T_{f} known in advance to the order of magnitude, $T_{\mathrm{f}} \sim m_\pi^{-1}$, which agrees surprisingly well with that obtained from the experimental one according to $\mathrm{d}n/\mathrm{d}p_{\mathrm{t}}$. The agreement with the experimental data [the dependence $n(s)$, the universal dependences of $\mathrm{d}n/\mathrm{d}p$ on $(m_i^2 + p_{\mathrm{t}}^2)^{1/2}$, and others] was amazing. But as an English proverb says, "nobody is so deaf as he who does not want to hear". These successful results (the formulae were obtained as far back as the 1950s) were ignored. In 1969 Feynman obtained $\mathrm{d}n/\mathrm{d}y$ (on the basis of the parton model) in the form of a 'table', i.e., a plain distribution between limiting values of rapidity y. This was immediately adopted by both experimenters and most of theoreticians as an unconditional law.

But an accumulation of experimental data gradually made way, at first very slowly, and evoking surprise and mistrust, for the hydrodynamic Landau theory. I remember being invited for the first time to the Aix-en-Provence Conference of 1973 where I was to give a talk on this theory. The atmosphere that reigned there was approximately like this: "What nonsense are you going to speak? All right, so be it, tell us and we shall listen".

They were listening patiently. But in the middle of the conference L Foa, who was preparing for the concluding talk, asked me to call at his large room, where the graphs of the experimental data and other materials lay out at the tables. He took me to the large graph of new exact data on $\mathrm{d}n/\mathrm{d}y$ and thoughtfully scratched his head without saying a word. The curve had a Gaussian form in accord with Landau's theory and showed no Feynman's 'table'. This was, in fact, the starting point. At the 1974 Conference in Leipzig I began my talk with the words: "Once there was a tribe speaking its own strange language. The tribe was gradually dying out and the language was almost lost. But civilized people who grouped around accelerators came to remember that a strange tribe had once existed. They searched out one old man still alive from that tribe and asked him to tell of their language. I, too, will try to do this."

The interest to the hydrodynamic approach increased substantially when the creation of gauge quantum field theories and, in particular, the unified electro-weak theory rehabilitated the quantum field approach and, in turn, the quasi-classical approximation to it. The 'boiling operator liquid' again had a theoretical right to exist. But still it would hardly have been admitted by the 'decent society' if it had not been for two circumstances.

Firstly, this was the rapid accumulation of the confirming data in accelerator experiments with an increasingly high energy. In 1976, in his review of the latest data on multiple production of particles in accelerators, M le Bellac [16] said: "If we had got acquainted with the Landau hydrodynamic theory earlier, we might perhaps have avoided many difficulties in the interpretation of the experimental data".

Secondly, the creation of quantum chromodynamics and the recognition of the fact that the hadron vacuum is a material medium with a high energy density and pressure were no less important from the psychological point of view. The theory of this vacuum was the statics, while the hydrodynamic theory due to Landau and his predecessors Heisenberg, Fermi and Pomeranchuk was the dynamics of this medium.

Now that QCD and the gauge theories in general have been developed, this theory, i.e., phase transitions in a dynamically evolving medium has become a subject of investigation for hundreds of theoreticians. It is conventionally believed that a collision of very high energy nuclei leads first to the formation of a quark-gluon plasma which in cooling down and expanding undergoes a phase transition to transform to hadron matter in which chiral invariance is broken (the particle mass is restored) and the deconfinement vanishes (quarks bind to become hadrons and their production and annihilation operators just form the "boiling operator liquid"). After a further hydrodynamic expansion the temperature falls to T_{f} and a free spread of the final particles begins. Unfortunately, no physical signal has yet been found that would testify to the existence of a QCD plasma.

There exists however a more sophisticated possibility. In the scheme described nothing has been said about the massive constituent quarks although it was with their help that the very idea of quarks was formulated. This is primarily explained by the fact that the existing consistent QCD operates with only 'almost massless' point quarks which cannot provide a basis for the formulation of the theory of constituent quarks (see, for example, the report [17] by K Wilson who reviews the four-years of vain attempts by his group in this direction and considers the solution of this problem to be absolutely necessary).

Meanwhile there exists a scheme [18] of the hydrodynamic theory with a double phase transition: in expanding and cooling down, a QCD plasma first undergoes a transition with chiral symmetry breaking (at a temperature T_{ch} which is roughly estimated as ~ 200 MeV) into a state with predominantly free constituent quarks (and a necessary admixture of hadrons). With further hydrodynamic expansion the temperature falls to a certain T_{d} at which constituent quarks bind into hadrons. In its physical meaning this 'temperature of constituent quark deconfinement' is the 'Hagedorn temperature', or the maximum temperature for hadron existence. The idea that T_{d} and T_{ch} are not coincident was expressed long ago [19]. It was even shown [20, 21] that the inequality $T_{\mathrm{d}} \leqslant T_{\mathrm{ch}}$ must hold. At first such a model failed to result in a successful scheme. But a recent investigation clearly showed that such a hydrodynamic model with two phase transitions (DPTM — Double Phase Transition Model) may be successful with a difference $T_{\mathrm{ch}} - T_{\mathrm{d}} \sim 50$ MeV and with constituent quark deconfinement at a sufficiently low energy of about 1 GeV/nucleon of colliding nuclei or hadrons.

Thus, the Landau hydrodynamic theory that had been criticised and rejected for such a long period (of 15 – 20 years) has now been widely recognized for over two decades and has undergone far-reaching and diverse development. It has become commonplace and the name of Landau is not now mentioned in connection with it.

A theory created half a century ago cannot of course meet the requirements of contemporary researchers, but still the elegance of the theory in which classical physics 'works' quite validly in the super-micro-world cannot be but fascinating.

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